U.S. Department of Energy Hydrogen and Fuel Cells Program

2016 Annual Merit Review and Peer Evaluation Report

June 6–10, 2016
Washington, DC

U.S. DEPARTMENT OF ENERGY

DOE/GO-102016-4890
October 2016

U.S. Department of Energy Hydrogen and Fuel Cells Program

2016 Annual Merit Review and Peer Evaluation Report

June 6–10, 2016
Washington, DC

October 2016 **DOE/GO-102016-4890**

NOTICE

This report was prepared as an account of work sponsored by an agency of the United States government. Neither the United States government nor any agency thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of any information, apparatus, product, or process disclosed, or represents that its use would not infringe privately owned rights. Reference herein to any specific commercial product, process, or service by trade name, trademark, manufacturer, or otherwise does not necessarily constitute or imply its endorsement, recommendation, or favoring by the United States government or any agency thereof. The views and opinions of authors expressed herein do not necessarily state or reflect those of the United States government or any agency thereof.

Prologue

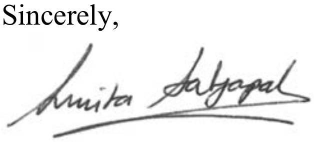
Dear Colleague:

This document summarizes the comments provided by peer reviewers on hydrogen and fuel cell projects presented at the fiscal year (FY) 2016 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 6–10, 2016, Washington, D.C. In response to direction from various stakeholders, including the National Academies, this review process provides evaluations of the DOE-funded projects in applied research, development, demonstration, and analysis of hydrogen and fuel cell technologies. Acting Assistant Secretary for the Office of Energy Efficiency and Renewable Energy (EERE) David Friedman opened the joint plenary session with more than 1,000 attendees, followed by a keynote address from Senator Byron L. Dorgan (ret.). The joint plenary also included overview presentations from the Fuel Cell Technologies Office and the Vehicle Technologies Office, as well as both offices' annual awards presentation. A plenary for Hydrogen and Fuel Cells Program participants included overviews on each of the eight programs: 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 its projects and overall programs from relevant experts with depth and breadth of knowledge across a number of broad areas. The recommendations of the reviewers are taken into consideration by DOE technology managers in generating future work plans. The table in this report lists the projects presented at the review, evaluation scores, and the major actions to be taken during the upcoming fiscal year (October 1, 2016–September 30, 2017). The projects have been grouped according to program 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 2017 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 2017 AMR, which is presently scheduled for June 5–9 in Washington, DC. Thank you for participating in the FY 2016 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 *Krishna Reddi; Argonne National Laboratory*	3.0	X			Reviewers appreciated the project for maintaining a solid approach and addressing fundamental and key issues through its scenario modeling efforts. They recognized that the analytical examination of key cost factors and commercial feasibility of different pathways under different conditions is helpful in setting research priorities. Reviewers recommended that the approach should evolve to accommodate limits in data availability and that the project should work to involve more industry partners to improve the quality of the data. They expressed concern about the use of a single value as a cost data point for infrastructure and hydrogen, and they suggested that a range of values be used to feed the model to capture uncertainty.
PD-025	Fatigue Performance of High-Strength Pipeline Steels and Their Welds in Hydrogen Gas Service *Joe Ronevich; Sandia National Laboratories*	3.1	X			Reviewers praised the overall approach of this project, specifically the focus on low- and high-strength welds. They also noted that the project is well thought out and relevant to the development of long-term delivery pathways with strong potential impact on pipeline cost reductions. They noted that the project team has made good progress to date but recommended a stronger focus on the role of microstructure in accelerating crack growth and on demonstrating the relevance of the project results to U.S. Department of Energy (DOE) targets and industry. Reviewers also noted that they would like to see more information on the project collaborations, such as with ORNL, NIST, and the Colorado School of Mines.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-031	Renewable Electrolysis Integrated System Development and Testing *Michael Peters; National Renewable Energy Laboratory*	3.1	X			According to reviewers, the project shows considerable progress in developing concepts for electrolyzer integration with intermittent renewable energy sources. Reviewers noted that all milestones in fiscal year (FY) 2015 and FY 2016 were complete and future milestones were on track. They commended the project team for drawing relevant and insightful conclusions from the data, as opposed to just reporting numerical results, and on productive cooperation between NREL and industry participants. Reviewers recommended that the results obtained in the project should be made publicly available so that other research groups can further analyze the data to fine tune their energy storage concepts and designs.
PD-038	Biomass to Hydrogen (B2H2) *Pin-Ching Maness; National Renewable Energy Laboratory*	3.3		X		Reviewers found the project approach to be sound and reasonable, and they commended the project team for its effective partnerships and notable accomplishments toward meeting project milestones and DOE goals. Reviewers questioned the value of the ionic liquid treatment task, stating that the reasoning for focusing on a new feedstock was not clear; instead, they recommended focusing on a single feedstock. They also expressed interest in seeing additional details on methods and quantitative results for some of the project tasks.
PD-088	Vessel Design and Fabrication Technology for Stationary High-Pressure Hydrogen Storage *Zhili Feng; Oak Ridge National Laboratory*	2.7		X		Reviewers appreciated the project's vessel demonstration results and analysis showing a pathway for meeting DOE's cost reductions targets. They questioned, however, the use of steel/concrete composite as the most appropriate approach and specifically wanted to see validation of the fatigue life of the vessel in hydrogen. They stressed the importance of including the cost of transport, handling, and site preparation in the cost analysis, as these costs could negate the savings of this new vessel technology when compared to today's incumbent technologies.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-096	Electrolyzer Component Development for the Hybrid Sulfur Thermochemical Cycle *William Summers; Savannah River National Laboratory*	3.1	X			Reviewers commended the project's progress in solar-thermochemical plant design, process design, and cost analyses using process flowsheet models and the Hydrogen Analysis (H2A) model analysis. They also commented on the productive collaborations with highly qualified research groups and industry partners. Reviewers said that they would like to have seen more detailed technical and economic inputs and assumptions used for the techno-economic analysis. They also highlighted membrane performance and durability in the hybrid sulfur cycle's electrolysis step as a key technical challenge.
PD-100	700 bar Hydrogen Dispenser Hose Reliability Improvement *Kevin Harrison; National Renewable Energy Laboratory*	3.4	X			Reviewers praised this project, noting its excellent approach and highlighting its relevance to the reliability of the hydrogen dispenser hose, which is a key station component that currently has few vendors and exhibits high failure rates. They were impressed by the project's identification of leaks from nozzle fittings and were interested in learning more about the magnitude and behavior of these leaks. Reviewers suggested improved project collaboration with industry, for example through inclusion of fitting manufacturers, and also recommended publishing technical reports and/or journal articles that share project results.
PD-101	Cryogenically Flexible, Low-Permeability Hydrogen Delivery Hose *Jennifer Lalli; NanoSonic, Inc.*	3.4	X			Reviewers praised this project's approach and accomplishments to date and highlighted project relevance, particularly in light of the current lack of hydrogen dispensing hose suppliers on the market and the limited durability of the available hoses. They expressed specific appreciation for the project's inclusion of hose fittings. Reviewers also commended the project team's collaboration with NREL, though they noted challenges in collaborating with industry. They recommended that future focus include the hose's impact on fuel quality.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-102	Analysis of Advanced Hydrogen Production Pathways *Brian James; Strategic Analysis, Inc.*	3.1	X			Reviewers recognized the high-impact and usefulness of the techno-economic analyses performed by the project team as well as the team's expertise and experience in this area. They would have liked to have seen more information from relevant industry partners in the development of the case studies, though they acknowledged the challenges presented by the low Technology Readiness Level (TRL) of the dark fermentation and solid-oxide electrolysis cell (SOEC) cases presented. Reviewers would also like to have seen further details on the technical and economic assumptions of the analysis. They specifically expressed concerns about the aggressiveness of some of the assumptions presented for the future cases studies.
PD-103	High-Performance, Long-Lifetime Catalysts for Proton Exchange Membrane Electrolysis *Hui Xu; Giner, Inc.*	3.3	X			Reviewers gave high scores to the project for its progress in developing new, reduced-platinum group metal (PGM) electrocatalysts for polymer electrolyte membrane (PEM) electrolysis, which offer the potential to lower the costs and promote wider acceptance of the PEM technology. Reviewers noted the well-defined roles, productive collaboration, and "healthy competition" among the project participants. A specific project strength cited was the development of standard testing protocols, though the challenge of getting broad acceptance of these protocols was also noted. Reviewers said that in spite of good initial performance of the new catalysts, the long-term durability is still lacking. They recommended concentrating on durability improvements in future work.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-107	Hydrogen Fueling Station Pre-Cooling Analysis *Amgad Elgowainy; Argonne National Laboratory*	3.2	X			Reviewers commended the project for its technically-sound and comprehensive approach to pre-cooling analysis. Reviewers praised the technical analysis for its contribution to overcoming fundamental challenges in pre-cooling and were impressed by project timeliness. Reviewers commented that the potential impact of pre-cooling on the price of the station could be minimal since the refrigeration loop is a small part of the total cost; they recommended an increase in the scope of the fueling station analysis. They also noted that the refrigeration cycle analyzed might not be representative of the industry and recommended inclusion of additional industry partners to address this.
PD-108	Hydrogen Compression Application of the Linear Motor Reciprocating Compressor *Eugene Broerman; Southwest Research Institute*	2.6		X		Reviewers noted that the project has the potential to improve compressor reliability if successful and highlighted the project team's success in developing detailed designs to meet project milestones to date. Reviewers expressed concerns, however, that the project approach is too focused on theoretical assumptions and that it lacks sufficient go/no-go decision points. They commented that stronger collaborations with industry early on may have flagged key design issues, including limited efficiency and the costs and durability of the materials selected. Reviewers expressed particular concern with the technical feasibility of achieving DOE's efficiency target of 1.3 kWh/kg.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-109	Steel Concrete Composite Vessel for 875 bar Stationary Hydrogen Storage *Zhili Feng; Oak Ridge National Laboratory*	2.3		X		Reviewers noted the value of the project's initial approach to stationary storage vessels using concrete reinforcement but emphasized that, based on project results, the current, updated vessel designs do not offer a viable pathway to hydrogen delivery and storage. Reviewers generally agreed that this technology would not be competitive when benchmarked against other available technologies for stationary storage. They commented that the project approach has focused on cost optimization but has not addressed technical feasibility and compatibility with existing stations. For example, the cycle life and demand for vessels of this size have not been addressed.
PD-110	Low-Cost Hydrogen Storage at 875 bar Using Steel Liner and Steel Wire Wrap *Amit Prakash; Wiretough Cylinders*	3.3	X			Reviewers praised the project's approach and progress made to date in developing the wire-wrapped technology for stationary hydrogen storage. They highlighted the successful ASME certification of this technology as a particular accomplishment. Reviewers expressed interest in seeing additional information specifically related to the vessel's resilience to fatigue in hydrogen. In particular reviewers would like to know how autofrettage affects fatigue crack growth in hydrogen under various pressures and temperatures.
PD-111	Monolithic Piston-Type Reactor for Hydrogen Production through Rapid Swing of Reforming/Combustion Reactions *Wei Liu; Pacific Northwest National Laboratory*	3.0	X			Reviewers commended the project team for its progress in developing and testing innovative catalysts and carbon sorbent materials, and they recognized the strong collaborations between industry, national laboratory, and academia partners. They noted, however, significant operational challenges facing the swing reactor system integration and control. Reviewers felt that H2A and greenhouse gas emissions analysis employed an oversimplified set of assumptions, and they recommended devoting more effort to the operational aspects of system integration and mass and heat balance in the reforming/regeneration cycles. They also expressed concern that the effect of bio-oil feed variability was not properly analyzed.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-113	High-Efficiency Solar Thermochemical Reactor for Hydrogen Production *Tony McDaniel; Sandia National Laboratories*	3.2	X			Reviewers scored the project well for its relevance to long-term, large-scale renewable hydrogen production, as well as its effective project planning and execution with well-defined roles and capable partners. They specifically highlighted the project team's progress in the design and validation of the cascading pressure receiver reactor for solar-thermochemical redox cycles. Reviewers recommended that technical and economic inputs and assumptions used for the techno-economic analysis be updated and improved. They expressed specific concern that significant heliostat cost reductions appear to be necessary to meet DOE's hydrogen production cost targets according to the current techno-economic projections.
PD-114	Flowing Particle Bed Solarthermal Reduction–Oxidation Process to Split Water *Al Weimer; University of Colorado*	3.0	X			Reviewers commended the project's comprehensive approach and noted the significant progress made with a highly qualified group of collaborators. They specifically highlighted the effectiveness of the multi-phase reactor modeling, performance prediction, and materials discovery. Reviewers recommended that project priorities be shifted toward efforts that increase the TRL, since the techo-economic analysis has identified a pathway for meeting DOE hydrogen production cost targets. Reviewers generally noted that the range of project objectives were too broad and recommended further refinement of activities.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-115	High-Efficiency Tandem Absorbers for Economical Solar Hydrogen Production *Todd Deutsch; National Renewable Energy Laboratory*	3.5	X			Reviewers highly scored the project and were impressed by the project team's ability to enhance the efficiency of III-V semiconductor photoelectrochemical (PEC) devices to a new world record of 16.3%. They specifically highlighted the project team's expertise and innovation in employing the inverted metamorphic multijunction approach to accelerate PEC device development. Reviewers, however, expressed concern over the limited stability and high cost of the III-V materials under development. They generally agreed that increasing the durability of the materials will be necessary in order to meet the upcoming targeted demonstration of 875 hours stability at high efficiencies.
PD-116	Wide-Bandgap Chalcopyrite Photoelectrodes for Direct Solar Water Splitting *Nicolas Gaillard; University of Hawaii*	3.5	X			Reviewers praised the project for its focus on an important class of chalcopyrite materials that has the potential to meet long-term DOE goals for PEC hydrogen production. They specifically commended the well-designed project for its demonstrated ability to precisely tune the bandgap of these materials to produce high-efficiency tandem devices. Reviewers expressed concern over the project's ability to achieve long-term durability targets, especially since the project team has focused primarily on the absorbers and less on the surface chemistry and catalysis. They recommended that the researchers focus on increasing durability.
PD-123	High-Performance Platinum-Group-Metal-Free Membrane Electrode Assemblies through Control of Interfacial Processes *Katherine Ayers; Proton OnSite*	3.5	X			Reviewers gave high scores to the project for its logically-structured work plan and excellent progress in developing non-PGM catalysts and enhancing alkaline membrane stability. They commented that the project's success offers the potential to achieve significant reduction in the capital cost of electrolyzers, which is critical for technology introduction on a larger scale. Reviewers recommended performing additional H2A analysis of the impact of the non-PGM catalysts on hydrogen production cost. They also would like to have seen a more detailed investigation of the significant and unexplained effect of adding potassium carbonate to the system.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-124	Solid-Oxide-Based Electrolysis and Stack Technology with Ultra-High Electrolysis Current Density (>3A/cm^2) and Efficiency *Randy Petri; Versa Power Systems*	3.3	X			Reviewers commended the project, specifically highlighting the demonstration of impressive cell performance at extremely high current densities (>3A/cm^2). They also noted, however, the lower efficiencies and higher degradation rates observed under these high-current operating conditions. They highly recommended the project team's thorough techno-economic analysis to assist in the determination of the optimum current density for the solid-oxide electrolysis cell (SOEC) stack, which balances performance with capital cost.
PD-125	Tandem Particle Slurry Batch Reactors for Solar Water Splitting *Shane Ardo; University of California, Irvine*	3.0	X			Reviewers appreciated the project team's approach to analyzing the feasibility of a particle-based PEC reactor through comprehensive physical modeling. They specifically highlighted the significant progress made in modeling electrolyte effects and in synthesizing/analyzing candidate absorber materials. Reviewers expressed concern over the project's ability to meet some upcoming milestones, including the reduction of piping and pumping energy demand by 80%. They also recommended better leveraging of proposed project collaborations.
PD-126	Compressorless Hydrogen Refueling Station Using Thermal Compression *Kenneth Kriha; Gas Technology Institute*	2.9	X			Reviewers expressed satisfaction with the project's initial progress in modeling and data collection, and they noted that the technical approach was comprehensive. They also recognized that the project offers significant potential to lower station costs if successful. Reviewers expressed concern over the cost of increasing a station's footprint to accommodate the numerous small vessels in this approach and over the potential of heat leaks in this system. They additionally commented that storage vessels assumed in the project are not yet commercial. Reviewers urged that data from project demonstrations be used to assess the concept's technical and economic feasibility.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-127	Sweet Hydrogen: High-Yield Production of Hydrogen from Biomass Sugars Catalyzed by In Vitro Synthetic Biosystems *Y-H Percival Zhang; Virginia Tech*	3.2	X			Reviewers noted that the proposed hydrogen production pathway is innovative and that the project is making progress toward its goals, particularly in protein expression and peak production rates. They expressed concern, however, over the practicality of the approach, and noted that the techno-economic analysis presented was mostly qualitative and not thorough enough. Reviewers also raised questions about whether the hydrogen production rates in this approach could be sufficiently prolonged. They recommended enhanced collaborative leveraging of other research.
PD-130	Improved Hydrogen Liquefaction through Heisenberg Vortex Separation of Para- and Orthohydrogen *Christopher Ainscough; National Renewable Energy Laboratory*	3.5	X			Reviewers praised the project for its innovative and promising approach to small-scale hydrogen liquefaction and specifically highlighted the project's significant potential to reduce the cost of hydrogen production and delivery, if successful. They commended the project team for first modeling, then validating the model with testing, and they encouraged the team to move forward on actual vortex tube experimentation and validation. Reviewers recommended the development of enhanced techno-economic models for better assessing the potential for future cost savings.
PD-131	Magnetocaloric Hydrogen Liquefaction *Jamie Holladay; Pacific Northwest National Laboratory*	3.3	X			Reviewers gave high scores to the project for making substantial progress over a short period of time, highlighting its effective leveraging of strong expertise and knowledge bases in the development of novel magnetocaloric materials for hydrogen liquefaction. They recommended, however, that the project team clearly identify the key novel technical features that distinguish the current project from past work and that they increase industry collaboration, particularly to better characterize scale-up potential. Reviewers expressed slight concern that the scope of the project was overly broad.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
PD-132	Advanced Barrier Coatings for Harsh Environments *Shannan O'Shaughnessy; GVD Corporation*	3.5	X			Reviewers praised the project for its progress and approach, specifically highlighting the vacuum tumbler approach to manufacturing. They also commented on the strong collaboration across industry, applauding the inclusion of a seal manufacturer, a manufacturer of compressor equipment, an industrial user, a hydrogen fuel system designer, and a national laboratory in the development to ensure the coatings are being designed for the application and use environment. Reviewers recommended investigating the possible contamination that may outgas from the coating.
PD-133	Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) – Consolidation *Christopher Ainscough; National Renewable Energy Laboratory*	3.2	X			Reviewers commended the project approach for its significant potential impact on hydrogen fueling infrastructure cost reduction and highlighted the impressive project progress that has been made to date. They felt, though, that more detailed information on the project schedule and more clarity on the project results compared to a benchmark station would be beneficial. They appreciated the current laboratory and industry collaborations; they recommended the inclusion of additional industry partners.
PD-134	Cryo-Compressed Pathway Analysis *A.J. Simon; Lawrence Livermore National Laboratory*	3.2	X			Reviewers appreciated the project for its innovation and for the cutting-edge nature of the cryo-compressed options. However, they expressed skepticism about the likelihood of cryo-compressed dispensing being adopted by hydrogen refueling stations as a competitor to other incumbent technologies for dispensing 700 bar compressed gas. Reviewers recommended that future work in this project should consider the full well-to-wheels analysis of cryo-compressed dispensing at scales consistent with capacities of current and future stations.

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 commended the project for providing unbiased analyses of hydrogen storage options and showing depth in technical evaluation across multiple storage approaches. Reviewers also commended the project's work on system/material trade-offs, assessing design variations and engineering features for diverse hydrogen storage systems and materials, and highlighting areas that either have potential for improvement or are already constrained to current values. However, reviewers cautioned that the assessment of a high-pressure metal hydride storage option needs to be completed with greater emphasis on overall thermal management issues of the charging performance. Reviewers also recommended that the project actively seek out experimental data from experienced researchers when the source of data for analysis is unavailable or unreliable.
ST-004	Hydrogen Storage Engineering Center of Excellence *Don Anton; Savannah River National Laboratory*	3.3			X	The reviewers were very satisfied with the approach and accomplishments of the Hydrogen Storage Engineering Center of Excellence (HSECoE) and stated that its findings were of utmost relevance to the overall Hydrogen Storage program. They felt that the large group of partners was sufficiently diverse and collaborations were well-organized and beneficial for the project. The reviewers also specified that making the modeling package available to the community was very significant and that the data obtained on the storage systems will provide a solid foundation for development when a suitable material emerges.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-008	Hydrogen Storage System Modeling: Public Access, Maintenance, and Enhancements *David Tamburello; Savannah River National Laboratory*	3.3	X			This is a follow-on project to the HSECoE. The reviewers commended the project for its efforts to enhance the performance and user-interface of the models and to ensure that the hydrogen storage research community is able to access and use these models in the most practical and user-friendly manner. The reviewers agreed that it is important to preserve the wealth of information and understanding of engineering concepts and required hydrogen storage material properties developed during the HSECoE. While reviewers also applauded the project's emphasis on the end user and strong collaboration with HSECoE stakeholders, they stated that including input/feedback from users who are not former HSECoE members could be beneficial to the overall success of the effort.
ST-063	Reversible Formation of Alane *Ragaiy Zidan; Savannah River National Laboratory*	3.1	X			The reviewers agreed that some progress had been made in alane synthesis and crystallization, specifically noting the development of the MgNi-based cathode to reduce dendrite formation during the electrochemical process. They added that the project has the potential to meet U.S. Department of Energy (DOE) goals for portable power applications, and therefore, they applauded its relevance. The reviewers questioned the collaboration between the project and their partners and specifically worried that the division of labor and communication lines between the two seem unclear.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-100	Hydrogen Storage Cost Analysis *Brian James; Strategic Analysis, Inc.*	3.2	X			Reviewers commended the project's approach and in depth analysis, including an uncertainty analysis that vets and captures potential cost reduction concepts. Reviewers commended project interactions and collaborations, including data exchange with other institutes and industrial partners. Reviewers suggested collecting more data on low-cost carbon fibers and towpreg to refine assumptions where excessive fuzz causes tow breakage and results in increased winding time. Reviewers also recommended considering economic drivers such as cost versus performance metrics, to ascertain what drives the "buy" decision. Reviewers commented that the model has a strong foundation but that the project can add other features such as certification costs, tank finishing/rework, and scrap costs.
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*	2.9		X		Reviewers noted that the project is relevant because it advances the understanding of the impact cryo-compressed vessels can have on hydrogen storage capacity. However, reviewers stressed how important it is for the project to address key technical issues, as it is not clear whether the project has a grasp on aspects related to thermal insulation, dormancy, or tank liner failures. The reviewers complimented the team for completing the commissioning and certification of the cryo-pump testing facility but stated they would like to see non-invasive methods and instrumentation for evaluating and monitoring tank robustness and quality used before conducting further tank testing. Reviewers also noted that collaboration between project partners seems adequate but suggested obtaining additional input from stakeholders with extensive expertise in high-pressure tank design. This is a joint project funded by the Hydrogen Storage, Technology Validation, and Hydrogen Delivery programs.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-113	Innovative Development, Selection, and Testing to Reduce Cost and Weight of Materials for Balance-of-Plant Components *Jon Zimmerman; Sandia National Laboratories*	3.1	X			Reviewers commented that the project is well designed and strong in the fundamental understanding of hydrogen embrittlement. They noted there should not be overreliance on stacking fault energy. Reviewers also commented that it is unclear how the experiments are used to validate the theory and whether configurational degrees of freedom have been considered. Reviewers recommended that the project include Cr, which is another important composition variable, and should not limit the main composition variable to predominantly Ni. Reviewers commended the project for strong collaboration with materials companies and component suppliers, specifically for engaging balance-of-plant and stainless steel manufacturers. Recommendations include acquiring further input from manufacturers regarding the cost and machining of these materials.
ST-114	Next-Generation Hydrogen Storage Vessels Enabled by Carbon Fiber Infusion with a Low-Viscosity, High-Toughness Resin System *Brian Edgecombe; Materia*	3.4	X			Reviewers commended the project's significant accomplishments, including employing a good mix of modeling and experiments to infuse and test panels and small-scale tanks to demonstrate feasibility of use, as well as preparing and bursting small Type 3 composite overwrapped pressure vessel (COPV) tanks. Reviewers recommended that the project team work on confirming relationships between voids, composite performance, and carbon fiber reduction opportunities. Reviewers also recommended leveraging the vast experience of the composites community in vacuum assisted resin transfer molding processing to fully accomplish the objective of vacuum infusing a full-scale prototype tank. Reviewers commended the project's strong technical team and good collaboration among partners. However, reviewers noted that the project would benefit from a series-production tank manufacturer either as a partner or in a consulting role to better guide development toward commercialization.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-115	Achieving Hydrogen Storage Goals through High-Strength Fiberglass *Hong Li; PPG Industries, Inc.*	2.5		X		Reviewers commented that the loss of fiber strength, as compared to the loss of strength in pristine glass fiber, is a major setback. Without improved stress rupture performance, it will be hard to reduce the tank design safety factor as proposed to offset the additional weight, volume, and manufacturing cost required for the added fiber. Reviewers recommended that the project address the fiber manufacturing issues to produce glass fiber with low translation loss.
ST-116	Low-Cost a-Alane for Hydrogen Storage *Richard Martin; Ardica*	2.9	X			The reviewers commented that significant progress has been made on the cost models and that the methodology has been clearly explained and focused. They confirmed the project's relevance for small portable power applications. The reviewers stated that while the teams were good, the level of collaboration with national laboratories could be improved. They also suggested that future efforts be focused on progress with the reactor.
ST-118	Improving the Kinetics and Thermodynamics of Mg(BH4)2 for Hydrogen Storage *Brandon Wood; Lawrence Livermore National Laboratory*	3.0	X			The reviewers complimented the project's highly integrated theoretical, characterization, and experimental approaches. They noted that this project fits nicely into the overall Hydrogen Storage program and should be able to interface with the Hydrogen Materials—Advanced Research Consortium (HyMARC) extremely well, as many of HyMARC's capabilities will suit the project's needs. The reviewers commented that work on this specific system is highly relevant, as the material is one of the few that has the potential to meet the storage targets. They also felt that reversibility and cycling studies should be included in future work.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-119	High-Capacity Hydrogen Storage Systems via Mechanochemistry *Vitalij Pecharsky; Ames Laboratory*	2.5		X		The reviewers commended the project's high-risk, high-reward approach to target a new class of high-capacity materials. They also commended the initial theoretical work to screen potential target compounds. However, the reviewers expressed concern that the targeted compounds can be synthesized. The reviewers believe that the project would benefit from possible future interactions with the HyMARC. They suggested that the planned borohydride-graphene composite work be dropped to allow more effort on the mechanochemical synthetic tasks.
ST-120	Design and Synthesis of Materials with High Capacities for Hydrogen Physisorption *Brent Fultz; California Institute of Technology*	2.6		X		The reviewers felt that the project had made progress toward its milestones at these early stages. However, they stated that the presentation lacked a sufficient description of why the project's targeted materials and strategies were selected. They also expressed concern regarding the microscopy results, claiming that gold atoms dispersed on the surfaces do not show significant agglomeration. The reviewers also questioned the extent of the stated collaborations with national laboratories.
ST-121	High-Capacity and Low-Cost Hydrogen-Storage Sorbents for Automotive Applications *Hong-Cai (Joe) Zhou; Texas A&M University*	2.0		X		While the reviewers stated that the project's goal of developing materials with hydrogen storage greater than the typical 1 wt.% per $500 m^2/g$ is valid and well-defined, they had significant issues with several aspects of the project. They understood that the main target material displayed higher uptake than expected based on surface area but were disappointed that the project did not focus on the scientific reasons why this occurred. The reviewers were also unhappy that this target material did not meet the go/no-go metrics, and the presentation did not attempt to provide an explanation for why.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-122	Hydrogen Adsorbents with High Volumetric Density: New Materials and System Projections *Don Siegel; University of Michigan*	3.3	X			The reviewers applauded the manner in which the project used computational screening to direct synthesis and characterization of materials. They complimented of the team and its progress in the first year of the project. The reviewers also felt that the project would benefit from improved access to other existing materials databases and suggested that the project attempt to investigate high hydrogen capacities closer to room temperature.
ST-126	Conformable Hydrogen Storage Coil Reservoir *Erik Bigelow; Center for Transportation and the Environment*	2.8	X			Reviewers noted the storage geometry approach is novel with potential for improved volumetric density and installation flexibility. Reviewers commented that the hydrogen permeation target needs to be based on industry permeation standards, which has a lower value than the current standard based on loss of useable hydrogen. Reviewers also commented that the project should ensure permeability is managed safely. Reviewers recommended that the project evaluate Kevlar® strength reduction resulting from the known abrasion induced from vibration, as well as analyze failure modes to evaluate burst pressure in the conformable configuration. Reviewers also suggested that the project include an original equipment manufacturer as a partner to identify showstoppers and drive the design and requirements.
ST-127	Hydrogen Materials–Advanced Research Consortium (HyMARC): A Consortium for Advancing Solid-State Hydrogen Storage Materials *Mark Allendorf; Sandia National Laboratories*	3.2	X			Overall, the reviewers were satisfied with the structure and organization of the consortium and believe that HyMARC has the potential to make significant progress in the development of hydrogen storage materials. Specifically, the reviewers regarded the parallel development of foundational computational and experimental methods to be a positive and logical strategy and noted that progress and collaboration among the team members has been sufficient for its first year. Reviewers did point out that work at national laboratories investigating graphene nanobelts could be better integrated into the overall consortium compared to the other tasks.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-128	HyMARC: Sandia National Laboratories Effort *Mark Allendorf; Sandia National Laboratories*	3.1	X			The reviewers complimented the strong team that has been put together as part of the national laboratory efforts within HyMARC, as well as its extensive network of collaborators. Several of the capabilities were identified as being impactful and important to the field of hydrogen storage, including the upgraded high-pressure reactor, the new clean sample transfer systems, and the low-energy ion scattering (LEIS) instrument. The reviewers expressed slight concerns over the ongoing work on older materials, specifically the Li_3N and $NaAlH_4$ systems and suggested a careful prioritization of future efforts to ensure that information gained from the model systems will be carried forward to newer materials.
ST-129	HyMARC: Lawrence Livermore National Laboratory Effort *Brandon Wood; Lawrence Livermore National Laboratory*	3.2	X			The reviewers had positive comments about the national laboratory component of the HyMARC, specifically the group's extensive computational capabilities. They were satisfied with the progress made in the first year of the project and believed that the work being carried out has the potential to be of value in the development of hydrogen storage materials. They also pointed out that the foundational knowledge that the modeling efforts can provide are of need in the area. The reviewers expressed a desire to have seen more details on why specific materials were chosen for investigation.
ST-130	HyMARC): Lawrence Berkeley National Laboratory Effort *Jeffrey Urban; Lawrence Berkeley National Laboratory*	3.0	X			The reviewers viewed the national laboratory component of HyMARC to have made sufficient progress at this early stage. They regarded the efforts as being well-coordinated within HyMARC and having significant external collaborations as well. Spectroscopic work was identified as being important for the field. The reviewers did raise some minor concerns that these efforts are more of a materials development approach than the overall HyMARC goal of developing foundational knowledge about storage mechanisms. They also stated that the work on Mg encapsulation may need to be re-evaluated for its relevance to the overall project.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-131	Hydrogen Storage Characterization and Optimization Research Efforts *Thomas Gennett; National Renewable Energy Laboratory*	3.2	X			The reviewers commended several aspects of this effort, including the hydrogen capacity validation services, development of the thermal conductivity apparatus for external use, and organization of the round robin volumetric capacity testing. They agreed that these services are highly relevant to the DOE Hydrogen Storage program and important for the community as a whole. They felt that the team and its level of collaboration are well-managed and that they have made significant progress in their first year. The reviewers also commented that certain aspects of the national laboratory materials development work may need to be re-evaluated for its potential to yield useful storage materials.
ST-132	Hydrogen Storage Characterization Research Efforts *Tom Autrey; Pacific Northwest National Laboratory*	3.3	X			The reviewers were happy with the developments in the first year of the efforts to enhance the strong nuclear magnetic resonance spectroscopy capabilities and to tie them into the consortium and the storage portfolio as a whole. They indicated that the team involved is strong and that the level of collaboration with the many partners is good. They agreed that the effort should provide significant scientific details about storage behavior and that the goals of the project clearly support the DOE's hydrogen storage objectives.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
ST-133	Hydrogen Storage Characterization and Optimization Research Effort *Jeffrey Long; Lawrence Berkeley National Laboratory*	3.2	X			The reviewers complimented the group as being at the forefront of metal-organic framework research and stated that the collaborations they have formed for this project are well-established and productive. They specifically commended the demonstration of two hydrogen molecules at one open metal site and viewed this as an important result in the area of sorbent materials. The reviewers believe that the materials targeted by the project have the potential to be viable onboard storage materials and that the group's focus on high volumetric capacities at ambient temperatures is valid. While the reviewers commended the project's materials development efforts, they also stated that its scope should be more tailored to reflect the overall goals of the project team.

Fuel Cells

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-017	Fuel Cells Systems Analysis *Rajesh Ahluwalia; Argonne National Laboratory*	2.7		X		Reviewers commented that the approach was satisfactory overall and that the project maintained a strong collaboration effort. However, some reviewers noted that it was difficult to fully understand what has been accomplished and how all the data results will really impact fuel cell systems. Reviewers stated that the project would benefit if it focused less on the nanostructured thin film (NSTF) approach for future work.
FC-018	Fuel Cell Vehicle and Bus Cost Analysis *Brian James; Strategic Analysis, Inc.*	3.4	X			Reviewers were in consensus that the approach is solid. They commented that the team has done well in documenting all results and estimates while providing quality analysis. They also noted that the team has reduced the range between gas diffusion layer and bipolar plate costs and the team made recommendations for further cost savings. The reviewers recommended that future cost evaluations show where processes were volume-optimized.
FC-020	New Fuel Cell Materials: Characterization and Method Development *Karren More; Oak Ridge National Laboratory*	3.2	X			Reviewers commended the approach and highlighted the progress made in three-dimensional (3-D) imaging of catalyst layers. They also commended the project for its collaborations. They noted that since the automotive and commercial membrane electrode assembly (MEA) companies may be hesitant to share their state-of-the-art MEAs for outside evaluation and publication, the team may need to find a more realistic way to obtain MEA samples for comparison. The reviewers recommended an increased emphasis on new and improved microscopy techniques, and not just application. Also, they noted that the scope should be directed toward imaging catalyst layers under wet or in situ conditions.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-021	Neutron Imaging Study of the Water Transport in Operating Fuel Cells *David Jacobson; National Institute of Standards and Technology*	3.2	X			Reviewers were impressed with the project's approach to improve fuel cell water imaging needs. They stated that the neutron imaging capabilities are impressive and contributing to the advancement of more tools for water management. Reviewers commented that the current capabilities of the facility seemed to be underutilized. Reviewers recommended that x-ray/neutron combined experiments be predicated on interest.
FC-052	Technical Assistance to Developers *Tommy Rockward; Los Alamos National Laboratory*	3.4	X			Reviewers stated the project's approach is generally good, using accepted industry practices and procedures. They commended the project team's broad knowledge of polymer electrolyte membrane (PEM) technology and its positive impact on the collaborative work. Reviewers stated that the narrow focus on PEM technology was a weakness and recommended that the project be expanded to include the application of knowledge gained.
FC-081	Fuel Cell Technology Status: Degradation *Jennifer Kurtz; National Renewable Energy Laboratory*	3.0	X			The reviewers commended the project's interaction with industry to collect data. However, they stated that the uniform analysis (no matter what the technology is) and the scattered data make interpretation and comparisons difficult. One reviewer recommended a comparison between different international regions.
FC-097	Stationary and Emerging Market Fuel Cell System Cost Analysis – Primary Power and Combined Heat and Power Applications *Vincent Contini; Battelle*	3.3	X			Reviewers commented that the approach does an adequate job of identifying the main contributions to the cost of fuel cell systems. They noted that the project provides a lot of valuable data and analysis; however, results would benefit from information from additional commercial suppliers, including international ones, selling combined heat and power (CHP) systems.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
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*	3.3	X			Overall, reviewers were in agreement that the approach is generally good. Reviewers stated that the project produced excellent results. However, the project would have benefited from the inclusion of additional technologies, such as high temperature PEM. Reviewers recommended an attempt to calculate the health/environmental externality financial cost confidence interval to highlight fuel cell societal benefits.
FC-104	High-Performance, Durable, Low-Cost Membrane Electrode Assemblies for Transportation Applications *Andrew Steinbach; 3M*	2.7		X		Reviewers noted that the approach and progress achieved were good and that collaborations with other institutions were strong. However, it seems like progress is not sufficient to meet the robust goal of allowing NSTF technology to be the design of choice for future automotive stacks. Reviewers recommended that the project focus on changes to the basic support structure of NSTF or to non-NSTF MEAs in order to address NSTF technology limitations.
FC-106	Rationally Designed Catalyst Layers for Polymer Electrolyte Membrane Fuel Cell Performance Optimization *Deborah Myers; Argonne National Laboratory*	3.1			X	Reviewers stated that the approach is reasonable and can be used to perform careful analysis of relevant materials. They noted that the project made good progress and included a strong team. However, some reviewers highlighted the limitations of the insight gained from this project. They noted that the project did not seem to provide information about mechanisms or alternative approaches that could lead to improved MEA performance.
FC-107	Non-Precious Metal Fuel Cell Cathodes: Catalyst Development and Electrode Structure Design *Piotr Zelenay; Los Alamos National Laboratory*	3.2			X	The reviewers commended the project for its significant progress over its lifetime and its relevance and potential to reduce PEM fuel cell cost. They noted that the team demonstrated non-platinum group metal (non-PGM) catalysts with increased activity making good progress toward meeting the U.S. Department of Energy (DOE) targets. Reviewers stated that the project needs additional focus on catalyst layer engineering and high current density operation, which should be the focus of future efforts.

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*	3.5	X			Reviewers were all in agreement that the approach was excellent and the project was well executed. They stated that, with an amazing team and strong polymer background, the project was able to provide results with proper control and targets. Reviewers recommended more work on elucidating membrane degradation issues.
FC-110	Advanced Hybrid Membranes for Next-Generation Polymer Electrolyte Membrane Fuel Cell Automotive Applications *Andrew Herring; Colorado School of Mines*	2.7		X		Reviewers commented that the approach pursued is promising but that the project achieved limited progress. They noted that there needs to be more focus on the membrane mechanical properties and its potential durability. Reviewers recommended that the project further explore the viability of the heteropoly acid approach and demonstrate it in MEAs.
FC-116	Smart Matrix Development for Direct Carbonate Fuel Cell *Chao-yi Yuh; FuelCell Energy, Inc.*	3.4	X			Reviewers noted that the approach is very clear and promises to improve the durability of molten carbonate fuel cells. They stated that the team was able to accomplish many milestones and produced excellent results by demonstrating a new matrix that will improve performance and durability relative to the baseline. However, reviewers stated that the project should provide more detail about the materials and processes used during the analysis.
FC-128	Facilitated Direct Liquid Fuel Cells with High-Temperature Membrane Electrode Assemblies *Emory DeCastro; Advent Technologies, Inc.*	2.9	X			Reviewers stated that the approach is novel and builds on previous work. However, it is unclear whether this approach will be able to achieve relevant targets. They noted that the performance does not seem like it will really crossover to or impact the PEM fuel cells for transportation. Reviewers recommended more technical detail be presented and techno-economic analysis to demonstrate technology competitiveness in specific markets.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-129	Advanced Catalysts and Membrane Electrode Assemblies for Reversible Alkaline Membrane Fuel Cells *Hui Xu; Giner, Inc.*	2.7		X		Reviewers stated that progress was achieved; however, they did not all agree that the selection of catalyst materials was sensible. They expressed disappointment that the catalysts were not tested in an MEA. They noted that the team did not examine an innovative class of materials for ORR/OER and made little attempt to perform a detailed structure property relationship study with catalyst activity. The reviewers recommended further catalyst testing and materials down-selection.
FC-130	Development of Platinum-Group-Metal-Free Catalysts for Hydrogen Oxidation Reaction in Alkaline Media *Alexey Serov; University of New Mexico*	3.1	X			Reviewers stated that the approach was generally good with reasonable accomplishments. However, reviewers noted lack of sufficient information for proper accomplishment evaluation. They noted that the team made promising initial results, but it is not clear whether the kinetic data can be translated to MEA data. The reviewers recommended doing more testing, while also providing more catalyst benchmark data for the relevant systems.
FC-131	Highly Stable Anion-Exchange Membranes for High-Voltage Redox-Flow Batteries *Yushan Yan; University of Delaware*	2.6		X		Reviewers found the synthetic approach of combining a stable cation with a stable backbone to be solid and reasonable, but were concerned that the degradation tests are not the most accurate. In addition, reviewers expressed concern about the results achieved, particularly with respect to conductivity and stability. They recommended adding conductivity and stability targets or milestones and establishing a go/no-go decision point.
FC-132	Innovative Non-Platinum-Group-Metal Catalysts for High-Temperature Polymer Electrolyte Membrane Fuel Cells *Sanjeev Mukerjee; Northeastern University*	3.0	X			Reviewers were impressed by the strong collaborative team and the project's innovative and promising approach to eliminating PGM from fuel cells. However, reviewers noted that the project had already missed two milestones and that fuel cell performance was not satisfactory. They recommended that the project focus on addressing catalyst performance improvement, perhaps by identifying the most promising formulation and focusing on it.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-135	Fuel Cell Consortium for Performance and Durability – Consortium Overview *Rod Borup; Los Alamos National Laboratory*	3.1	X			Reviewers universally lauded the strength of the project team and the proposed approach to collaboration with each team member focusing on its core competency. However, they warned that integration of new partners and coordination of the whole consortium could be a weakness if a strong and clear communication plan is not in place. Reviewers recommended that the consortium should focus more on novel fuel cell testing techniques.
FC-136	Fuel Cell Consortium for Performance and Durability – Electrocatalysts and Supports *Debbie Meyers; Argonne National Laboratory*	3.3	X			Reviewers praised the relevance of the project's focus on durability of catalysts and supports, as well as the collaboration among team members. They noted, though, that collaboration with other DOE-funded projects and with suppliers may be a challenge. Reviewers recommended stronger collaboration with other members of the Fuel Cell Performance and Durability consortium.
FC-137	Fuel Cell Consortium for Performance and Durability – Electrode Layer Integration *Shyam Kocha; National Renewable Energy Laboratory*	3.1	X			Reviewers stated that the overall approach of applying learnings from rotating disk electrode (RDE) studies to the optimization of MEA-catalyst layers for state-of-the-art catalysts with the help of modeling is good. They noted, however, that the team may be focusing too much on mitigation strategies and recommended that the project focus more on a foundational understanding of the root causes of degradation.
FC-138	Fuel Cell Consortium for Performance and Durability – Ionomers, Gas Diffusion Layers, Interfaces *Adam Weber; Lawrence Berkeley National Laboratory*	3.3	X			Reviewers were impressed by the strength of the team, its access to an extraordinary amount of characterization equipment and techniques and the project's relevance to DOE goals. They identified few weaknesses but noted that the project would benefit from increased interaction with industrial partners and original equipment manufacturers (OEMs). Reviewers recommended that the project team maintain an emphasis on membrane interfacial resistance.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-139	Fuel Cell Consortium for Performance and Durability – Modeling, Evaluation, Characterization *Rangachary Mukundan; Los Alamos National Laboratory*	3.3	X			Reviewers identified the team members and their well-balanced approach with specific goals and targets as a project strength. They noted, though, that the project team could improve the quality of its collaborations with commercial suppliers and with stack developers. Reviewers recommended that the project better define the modeling work.
FC-140	Tailored High-Performance Low-Platinum-Group-Metal Alloy Cathode Catalysts *Vojislav Stamenkovic; Argonne National Laboratory*	3.1	X			Reviewers praised the project for developing novel in situ characterization techniques that enable real-time measurements of Pt dissolution and for the high activity of its catalysts. However, they expressed concern that the project was focused too heavily on RDE testing and ORR activity without a commensurate focus on MEA testing. Reviewers noted that the project would benefit from moving more quickly to MEA testing activities.
FC-141	Platinum Monolayer Electrocatalysts *Radoslav Adzic; Brookhaven National Laboratory*	2.7		X		Reviewers stated that the project has strong team members demonstrating novel electrocatalysts. However, they also stated that these novel electrocatalysts rely too heavily on replacing Pt with other PGM catalysts. Also, reviewers commented that the project remains overly dependent upon RDE testing. Reviewers recommended a shift to focus on the non-PGM core materials, such as niobium and niobium nitride.
FC-142	Extended Surface Electrocatalyst Development *Bryan Pivovar; National Renewable Energy Laboratory*	3.0	X			Reviewers praised the strength of the project team and the rational approach to using catalyst powders, which lends itself to high specific activity and a higher "ceiling" for activity. The reviewers stated, however, that the project was not placing enough emphasis on mitigating Ni leaching or Pt dissolution. Recommendations were mixed, ranging from identifying a method to evaluate the stability of the nickel substrates to increasing the project's emphasis on integrating the powders into catalyst layers.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-143	Highly Active, Durable, and Ultra-Low-Platinum-Group-Metal Nanostructured Thin Film Oxygen Reduction Reaction Catalysts and Supports *Andrew Steinbach; 3M*	3.1	X			Reviewers noted that the project team has a good track record of working with NSTF and that NSTF is a good platform for high throughput with the potential to achieve high activity and durability. They stated, however, that the project has not prioritized operational robustness, a key technical barrier to the technology. Therefore, they recommended increasing the project's focus on improved operational robustness and, in particular, including automotive OEMs in order to specifically probe relevant operating conditions.
FC-144	Highly Accessible Catalysts for Durable High-Power Performance *Anu Kongkanand; General Motors (GM)*	3.1	X			Reviewers stated that the project is relevant to achieving DOE targets and uses a systematic approach with clearly defined goals. One weakness noted was the lack of a clear path toward understanding and minimizing Pt and Co dissolution during fuel cell operation. Reviewers universally recommended that the project focus more on catalyst development.
FC-145	Corrosion-Resistant Non-Carbon Electrocatalyst Supports for Proton Exchange Fuel Cells *Vijay Ramani; Illinois Institute of Technology*	2.7		X		The reviewers noted that the project team has a good grasp on the challenges associated with the project, has a proven track record in developing and executing similar projects, and has a systematic approach. However, the project does not address technical problems with metal supports, does not have an alternative approach if the proposed systems do not work, and the approach is not innovative. Recommendations varied widely from clarifying the material criteria to paying more attention to hydrophilicity in oxide supports.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
FC-146	Advanced Materials for Fully Integrated Membrane Electrode Assemblies in Anion Exchange Membrane Fuel Cells *Yu Seung Kim; Los Alamos National Laboratory*	3.3	X			Reviewers noted the multi-faceted approach including a wide range of ionomers with good alkaline stability and the excellent synthetic chemistry expertise. They also noted, however, that it is not yet clear how anion exchange membrane fuel cell (AEMFC) approaches will compete with PEM fuel cells for accomplishing hydrogen-based energy conversion. In addition, they expressed concern that low-PGM loading or non-PGM catalysts were not addressed and recommended that a non-PGM catalyst be considered in the binder selection process.
FC-147	Advanced Ionomers and Membrane Electrode Assemblies for Alkaline Membrane Fuel Cells *Bryan Pivovar; National Renewable Energy Laboratory*	3.4	X			Reviewers noted that the team has excellent participants with experience working together and that the team has a tightly focused approach to a novel system for AEMFCs. Despite the novel approach, they noted that it is unclear whether AEMFCs will ultimately achieve commercial relevancy. The only recommendation was to expand work on MEA performance.
FC-149	Multiscale Modeling of Fuel Cell Membranes *Adam Weber; Lawrence Berkeley National Laboratory*	3.2			X	Reviewers noted that the project has a novel approach. They stated, however, that the project would benefit from experimental interactions with collaborators and that it was unclear whether the work would be relevant to other ionomers. They recommended that the work be expanded to include the investigation of perfluorosulfonic acid membranes with other side chains as well as hydrocarbon ionomers.

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*	3.5	X			Reviewers stated that the approach is very good and that there is little that can be improved upon. They also noted that the project was well designed to provide quality information on various control technologies. Reviewers stated that the project team has a formidable collection of facilities and people with the highly specific skills required by the task; they see little room to improve the team's collaboration. The reviewers suggested that providing a summary chart of inspection techniques, including information such as the target defect or variable, required detection limits, required scanning or detection rate, state of development, and state of adoption, would be useful for the end user.
MN-012	Clean Energy Supply Chain and Manufacturing Competitiveness Analysis for Hydrogen and Fuel Cell Technologies *Pat Valente; Ohio Fuel Cell Coalition*	2.8	X			Reviewers thought the project's approach to creating and supporting supply chains was generally good. They expressed mixed sentiment regarding the regional technical exchange centers, with some reviewers stating that the team had done an excellent job in establishing the centers and other reviewers questioning the importance of regional exchange centers. In addition, reviewers stated that the project team needs to improve the project's focus and to do a better job of tracking the project's impact with clear metrics. The reviewers recommended that the data collected from the technical exchanges be carefully analyzed to help DOE better achieve its goals.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
MN-013	Fuel Cell and Hydrogen Opportunity Center *Alleyn Harned; Virginia Clean Cities at James Madison University*	3.5	X			The reviewers were impressed with the team's approach and noted that significant progress has been made, particularly in collecting information and combining it into a single website to be used by the fuel cell community. However, they expressed concern that the project team does not have a clear measure for success and thought that the team should identify specific products that might be early commercial markets. The reviewers suggested that the project team clarify some details, such as the metrics used to determine project success and the manner in which the website is going to be maintained after federal funding has ended.
MN-014	U.S. Clean Energy Hydrogen and Fuel Cell Technologies: A Competiveness Analysis *Patrick Fullenkamp; GLWN – Westside Industrial Retention & Expansion Network*	3.1	X			Reviewers noted that the approach is well structured and effective in generating a competitiveness analysis that is consistent in methodology with previous competitiveness analyses, and they were impressed with the progress made with original equipment manufacturer (OEM) and Tier 1 supplier surveys. The reviewers stated that the project team could have explored the results more thoroughly, including investigating discrepancies between OEM and Tier 1 survey responses. Reviewers noted that it is unclear whether the project will benefit the DOE beyond the current cost analysis and market reports. Reviewers recommended that the team further explore the assessments of manufacturing readiness by OEMs and Tier 1 suppliers.
MN-017	Manufacturing Competitiveness Analysis for Hydrogen Refueling Stations *Margaret Mann; National Renewable Energy Laboratory*	3.2	X			Reviewers stated that the project team's approach is effective, and they noted that the team is successful in making the cost analysis thorough for each component. They thought, however, that the team is trying to study too many subjects in such detail that assumptions are being made without sufficient information. Further, the reviewers noted that the analysis may be too dependent upon the assumptions made for each sub-system. They recommended the project team reach out to existing manufacturers and developers to verify the team's assumptions and review the results.

Technology Validation

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
TV-001	Fuel Cell Electric Vehicle Evaluation *Jennifer Kurtz; National Renewable Energy Laboratory*	3.5	X			Reviewers remarked that the project uses an objective approach in providing valuable real-world insight into fuel cell electric vehicle (FCEV) performance. It was noted that significant understandings have been gained over the past several years of data collection and evaluation. However, reviewers stressed that it is essential to acquire data from the newer generation of commercial vehicles recently introduced in the market. Reviewers also suggested that the driver and refueling interface be evaluated.
TV-008	Fuel Cell Bus Evaluations *Leslie Eudy; National Renewable Energy Laboratory*	3.7	X			Reviewers appreciated that the data are from buses that are in daily revenue service and that there is close collaboration with transit agencies. Increased collaboration with the DOE Vehicle Technologies Office and international partners was advised. It was noted that the value of data was being challenged because of the small number of buses, which are aging, and it was suggested that data be normalized to account for these factors. Reviewers also suggested further investigation into the infrastructure specific to fuel cell electric buses.
TV-017	Hydrogen Station Data Collection and Analysis *Sam Sprik; National Renewable Energy Laboratory*	3.3	X			Reviewers noted that the value of this project will grow as more stations come online and praised the involvement of California stakeholders. They also cautioned that the varying levels of detail collected from partners and discord related to data presented undermines the project's value. Reviewers strongly suggested that all retail hydrogen fueling stations report operational and cost data. Suggestions for future evaluation involved examining same vs. different design stations and small- vs. large-capacity compressor stations, while also strengthening international collaboration and data benchmarking.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
TV-019	Hydrogen Component Validation *Daniel Terlip; National Renewable Energy Laboratory*	3.1	X			This project was viewed as consistent with H2USA priorities and as providing crucial information for increasing hydrogen station reliability. Reviewers cautioned that corrections for altitude should be included in the analyses, as testing is conducted at elevation, but most station deployments are at sea level. While commenting that the project involves robust participation from industry, increased collaboration with various stakeholders through the H2Tools platform was recommended. Reviewers further commented that more emphasis should be placed on discovering the root cause of component failures and providing high-level design suggestions.
TV-025	Performance Evaluation of Delivered Hydrogen Fueling Stations *Ted Barnes; Gas Technology Institute*	3.0	X			The reviewers noted the importance of obtaining real-world performance data on delivered hydrogen fueling stations and commended the collaboration between partners and the progress with the initial stations. However, permitting issues delaying data collection on the remaining three stations were a point of concern. It was suggested that data beyond number of fills—such as fill variations and boil-off rates—also be collected and evaluated.
TV-026	Development of the Hydrogen Station Equipment Performance (HyStEP) Device *Terry Johnson; Sandia National Laboratories*	3.8			X	Reviewers were impressed with the swift deployment of the device—which was seen as vital to accelerating station commissioning—and commended the management of the project. It was suggested that feedback from potential future users be obtained and that the device could potentially also be used for hydrogen quality testing and periodic gauging of station performance.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
TV-027	Station Operational Status System (SOSS) 3.0 Implementation, SOSS 3.1 Upgrade, and Station Map Upgrade Project *Ben Xiong; California Fuel Cell Partnership*	3.7	X			Reviewers praised the project on multiple fronts—its success in implementing the Station Operational Status System on all California stations, enhancing data collection, providing information that is vital to gaining customer acceptance, and developing a disaster recovery plan. However, they stressed that all hardware and software requirements should be fully vetted by experts and that sensitivities around privacy of customer data be considered. Reviewers also suggested considering the addition of several tank categories in order to accommodate vehicles with larger tanks (e.g., buses) and relevant state-of-charge calculations.
TV-028	Advanced Hydrogen Fueling Station Supply: Tube Trailers *John Aliquo; Air Products and Chemicals, Inc.*	3.3	X			This project was viewed by reviewers as being very beneficial to the development of hydrogen infrastructure, with the potential to reduce the need for compressors, which are significant contributors to station issues. Reviewers stressed that obtaining approval from the U.S. Department of Transportation for moving the high-pressure tube trailers on roads should be a priority and that specific system cost goals should be added.
TV-029	Performance and Durability Testing of Volumetrically Efficient Cryogenic Vessels and High-Pressure Liquid Hydrogen Pump *Salvador Aceves; Lawrence Livermore National Laboratory*	3.3	X			Reviewers believed that the project has a strong team and commended the use of team capabilities in safety testing. Reviewers commented that this project may be occupying a limited niche, but they still found it of value for FCEV commercialization. They strongly recommended collaboration with and input from more than one automaker. It was also mentioned that cost analysis and comparative analysis with gaseous storage would add value.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
TV-031	Dynamic Modeling and Validation of Electrolyzers in Real-Time Grid Simulation *Robert Hovsapian; Idaho National Laboratory*	3.3	X			This project was regarded by reviewers as promising and important to understanding how electrolyzers provide benefits to the grid and how penetration of renewables may be increased. Collaboration with key partners, including utilities, was praised. Reviewers suggested including an electrolyzer company partner and investigating revenue streams of future projects with a higher penetration of renewables; evaluating the impact sub-systems supporting the electrolyzer will have on response times; and considering a 4,000 to 8,000 hour demonstration.
TV-032	Fuel Cell Electric Truck Component Sizing *Ram Vijayagopal; Argonne National Laboratory*	3.2			X	Reviewers regarded trucks as a valid market for fuel cells and remarked that the modeling performed provided a good foundation for designing fuel cell trucks. However, they expressed that the modeling would need to be validated with real-world performance using prototype vehicles. Examining life cycle cost and greenhouse gas analyses was suggested as a next step.
TV-033	Brentwood Case Study *Carl Rivkin; National Renewable Energy Laboratory*	3.1	X			Reviewers thought that there would be some useful learnings from the Brentwood case study. However, they stressed that the applicability of learnings would be limited and that investigating the implementation of hydrogen stations at retail sites would have been of more value. It was suggested that messaging on the learnings be coordinated with relevant industry groups and stakeholders; lessons learned from operations be added; and that the findings be revisited and updated as further experience is gained.
TV-034	Fuel Cell Hybrid Electric Delivery Van Project *Jason Hanlin; Center for Transportation and the Environment*	2.8	X			Reviewers noted that the potential impact of this project was promising and that the project team is demonstrating progress. Collaboration with partners was praised and seen as highly valuable. Teaming with a hydrogen tank manufacturer was suggested. It was also suggested that providing fueling for the vans be a focal point earlier in the project.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
TV-037	Hydrogen Meter Benchmark Testing *Michael Peters; National Renewable Energy Laboratory*	3.3	X			Reviewers believed that this effort is important to understand flowmeter performance and meet SAE J2601 standards for refueling and that the project made use of good collaborations. They felt that greater value could be achieved by developing standards and methodologies that can be used across flowmeter manufacturers. It was highlighted that effects of operating conditions—such as cumulative errors during tank fill, ambient weather extremes, and varying vibration conditions—will likely be of more interest to station designers. Reviewers strongly suggested including station owners and operators in the effort.

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*	3.4	X			Reviewers praised this project's improvement in the areas of collaboration and outreach, particularly for involving a variety of stakeholders. They commended the Continuous Codes and Standards Improvement approach as serving a critical area of need. Reviewers encouraged even further development in the area of outreach on a regional basis and also recommended clarification in areas where there is perceived overlap with other projects.
SCS-002	Hydrogen Component Research and Development *Robert Burgess; National Renewable Energy Laboratory*	3.2		X		Reviewers noted the project's good root cause analysis and forensic review of the problem with temperature and pressure relief device failures and felt that the results would inform industry practices. Other reviewers felt that the effort was limited in its impact, having a small sample size. They also applauded the collaboration with the stakeholders and the effort to incorporate feedback into the work plan. Reviewers found the proposed future work to be too broad and recommended clarification of direction.
SCS-005	Research and Development for Safety, Codes and Standards: Material and Component Compatibility *Chris San Marchi; Sandia National Laboratories*	3.4	X			Reviewers praised this project for its strategy, relevance, and international and domestic coordination, not only with other research institutions and industry but with code development organizations (CDOs) and standards development organizations (SDOs) as well. They noted the efforts to make the project data available broadly through an online database and encouraged further work toward this end. The reviewers recommended that the future work plan be more detailed for clarity.

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*	3.3	X			Reviewers commended the project's expansion of scope to include recirculation effects and the evaluation of fuel quality under realistic conditions. They noted the progress of the team in developing a prototype detector and expressed a desire to see results from the validation testing. Reviewers felt that the progress of the American Society for Testing and Materials (ASTM) portion of this work needs to move forward more aggressively but also acknowledged that the ASTM portion may be beyond the control of the project team.
SCS-011	Hydrogen Quantitative Risk Assessment *Katrina Groth; Sandia National Laboratories*	3.6	X			Reviewers praised this project for developing a valuable software tool, which can overcome many codes and standards (C&S) barriers. They praised the reports and user guide outputs as well as the coordination and inputs to several CDOs and SDOs. Reviewers encouraged the consideration and implementation of user feedback and recommended that the project continue to add additional models.
SCS-019	Hydrogen Safety Panel, Safety Knowledge Tools, and First Responder Training Resources *Nick Barilo; Pacific Northwest National Laboratory*	3.5	X			Reviewers applauded the expanded impact of the Hydrogen Safety Panel to include non-DOE work. They also noted the international collaboration for first responder training. Reviewers recommended that care be given to avoid scope creep, given the broad nature of the project tasks. Reviewers also raised concerns about having sufficient resources to update items developed elsewhere and hosted on H2Tools.org and whether the efforts to transfer external resources to the site might be duplicative.
SCS-021	National Renewable Energy Laboratory Hydrogen Sensor Testing Laboratory *Bill Buttner; National Renewable Energy Laboratory*	3.4	X			Reviewers commended the blind study approach to sensor validation and felt that the sensor testing portion of the project was very comprehensive. They also stated that the collaborations with industry were excellent. They recommended that clear documentation of sensor application guidance continue to be pursued. Reviewers raised some concerns about the test procedure for the planned vent profile measurement task and made several recommendations, which are contained in the full report.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SCS-022	Fuel Cell & Hydrogen Energy Association Codes and Standards Support *Karen Quackenbush; Fuel Cell & Hydrogen Energy Association*	3.2	X			Reviewers praised the efforts to coordinate and track a variety of C&S activities, and reviewers found the scope of the coordination work to be impressive. The reviewers felt that the direct accomplishments of the project were overshadowed by the number of activities being presented. Reviewers recommended that the purpose of the matrix be made clear so that the benefits to the DOE are easily understood.
SCS-025	Enabling Hydrogen Infrastructure through Science-Based Codes and Standards *Chris LaFleur; Sandia National Laboratories*	3.7	X			Reviewers commended the value and progress of this work and the direct impact it can have on many critical barriers. They particularly praised the real-world alternate means application efforts and the related collaboration. Reviewers recommended that the project work directly with authorities having jurisdiction in states beyond California.
SCS-026	Compatibility of Polymeric Materials Used in the Hydrogen Infrastructure *Kriston Brooks; Pacific Northwest National Laboratory*	3.6	X			Reviewers praised the results already achieved by such a new project, the focus on regular collaboration, and broad stakeholder input, and reviewers stated that the work is highly valuable. They made several recommendations regarding specific tests to be performed and noted that the broad number of materials being studied may be limiting. Reviewers recommended that the project ensure that previous results are taken into account.

Market Transformation

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
MT-008	Hydrogen Energy Systems as a Grid Management Tool *Richard Rocheleau; Hawaii Natural Energy Institute*	3.1	X			Reviewers stated that this project ties together multiple benefits (e.g., electrolyzer demonstration, renewable hydrogen for fuel cell deployments, enabling intermittent renewables) into a single package and helps increase awareness and clarity of the permitting process for deployments. Reviewers stated that the proposed future work is similar to the future work proposed for 2015. The reviewers were not clear on the reason for all the delays, such as the MTA shuttle bus conversion that was previously scheduled for September 2015 and is now listed as future work for 2016, and indicated that more attention to project execution barriers is needed.
MT-011	Ground Support Equipment Demonstration *Jim Petrecky; Plug Power*	3.4	X			Reviewers stated that this project has a high potential to meet Hydrogen and Fuel Cells Program goals and enable demonstration for a wide breadth of additional applications. Although reviewers were satisfied in general with progress made in terms of evaluation, design, and development of learnings, concerns about fuel cell stack performance and the timeline for completing the project were expressed. Reviewers also stated that the specific stack problems should also have been explained.
MT-013	Maritime Fuel Cell Generator Project *Joe Pratt; Sandia National Laboratories*	3.3			X	Reviewers noted that this project's objectives were relevant, specifically the focus on lowering emissions and technology/finance risk in a market that needs more efficient power technology is relevant. Reviewers commented that the project addresses the DOE's goal to enable and accelerate expansion of hydrogen and fuel cell system use and that lessons learned from this deployment can be used for similar technologies and other ports. They felt that development of a business case and identification of follow-on opportunities are imperative. Additional deployments with this system and concrete plans on how to expand the number of deployments are needed according to reviewers.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
MT-014	Demonstration of Fuel Cell Auxiliary Power Unit to Power Truck Refrigeration Units in Refrigerated Trucks *Kriston Brooks; Pacific Northwest National Laboratory*	2.7	X			Reviewers agreed that the project is relevant and is a logical extension of other fuel cell applications, such as forklifts. Reviewers mentioned that very low operational time is hampering progress and specific go/no-go decision points were not expressed clearly. Also, reviewers stated that the timeline for the demonstration with the recently added partners is not yet clearly developed. Reviewers noted that progress has been slow and the degree of commitment on the part of the industrial partners is questionable.
MT-017	Medium-Duty Parcel Delivery Truck *Thomas Griffin; FedEx Corporation*	3.4	X			Reviewers stated that this application has great potential and that the project fits well within the DOE's goals and objectives. Bringing one system online, evaluating its performance, and then deploying 19 systems at various sites seems like a reasonable approach, according to reviewers. Some noted that, although there has been a setback with collaborators, evaluating duty cycles and designing appropriate system specifications was time well spent. One reviewer noted that more explanation on refueling is needed.
MT-020	Fuel Cell–Battery Electric Hybrid for Utility or Municipal Medium- or Heavy-Duty Bucket Trucks – Fuel Cell-Powered Auxiliary Power Module *Abas Goodarzi; US Hybrid Corporation*	3.1		X		Reviewers noted that this application is an opportunity for near-term deployment of fuel cell technology, and this project is making progress toward evaluating the market. Reviewers commented that the potential impact of this project will be very limited without a better financial analysis. Insufficient information was provided to definitively understand the energy efficiency and air pollution reductions achieved. Reviewers said that there is an absence of go/no-go decisions and there is not enough detail on the battery storage system.

Systems Analysis

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SA-035	Employment Impacts of Hydrogen and Fuel Cell Technologies *Marianne Mintz; Argonne National Laboratory*	3.1	X			Reviewers acknowledged that the project was well developed and that applying "input/output" modeling was a good approach. The project benefited from strong collaboration with industry and academia but should clearly identify involvement of original equipment manufacturers (OEMs) and the energy companies. Future work should consider expanding the model to include geographical and market impacts, and the resulting job retraction of displaced industries.
SA-039	Life-Cycle Analysis of Water Consumption for Hydrogen Production *Amgad Elgowainy; Argonne National Laboratory*	3.4	X			Reviewers agreed that the project established a good fundamental understanding of water consumption associated with hydrogen pathways, which is essential for comparing multiple fuel pathways and resource analysis. Reviewers further stated that the project provides a good refinement and greater resolution of previous analysis and is critical to hydrogen production pathways. Suggestions included expanding collaboration to multiple stakeholders, including the international community, and more extensive peer review of data and assumptions. Reviewers agreed that the model should be expanded to include regional water assessment.
SA-044	Impact of Fuel Cell and Hydrogen Storage Improvements on Fuel Cell Electric Vehicles *Aymeric Rousseau; Argonne National Laboratory*	3.1	X			Reviewers observed that the project strategy was sound and uses well-respected models to assess the impact of future fuel cell improvements on fuel cell electric vehicle (FCEV) cost and performance. Reviewers thought that project results were extremely useful and relevant in developing future R&D strategies. Suggestions included an assessment of costs at low-volume production levels and more transparency of assumptions and data. Reviewers also suggested that future work consider the marginal costs vs. the marginal benefits of achieving key program targets.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SA-052	The Business Case for Hydrogen-Powered Passenger Cars: Competition and Solving the Infrastructure Puzzle *Robert Rosner; University of Chicago*	2.9		X		Reviewers noted that the project is beneficial in examining the profitability of hydrogen infrastructure beyond government incentives. Reviewers said that the project's collaboration activities should include input from the venture capital and financial community, hydrogen suppliers, and OEMs. They suggested that future work include vetting the input cost data and market analysis of the rollout of the first-generation hydrogen generation stations.
SA-055	Hydrogen Analysis with the Sandia ParaChoice Model *Rebecca Levinson; Sandia National Laboratories*	3.2	X			Reviewers commented that using previously developed models as input and exploring uncertainties and tipping points is a good approach. Reviewers said that the project enables the analysis of market segmentation and market assumption inputs to further explore fuel cell vehicle market penetration. Reviewers commented that the project would benefit from additional collaboration with industry stakeholders and coordination with other models to minimize redundancy. Also, the transparency of the range of values assigned to key variables should be articulated, according to reviewers.
SA-057	Life-Cycle Analysis of Emerging Hydrogen Production Technologies *Amgad Elgowainy; Argonne National Laboratory*	3.6	X			Reviewers noted that the project has made good progress in developing life-cycle analyses for emerging hydrogen production pathways. This information will be valuable in assessing future R&D. Reviewers stated that the efforts should continue to add other emerging hydrogen production technologies, such as photobiological, photochemical, and solar thermochemical systems. Reviewers said that it is critical to engage and collaborate with stakeholders and other entities. They suggested that future work include adding probability distributions for key inputs and parameters and engaging the international community for model input.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SA-058	Analysis of Incentives and Policy Impacts on the Market for Alternative Fuels and Vehicles *David Greene; University of Tennessee*	3.1			X	Reviewers determined that the data and findings from the project are valuable and relevant to understanding cost drivers and policy impacts of transitioning to alternative fuel vehicles and hydrogen FCEVs in particular. They said that the lessons learned provide good information for deployment of FCEVs and will help federal and state governments to better understand implications of policies and incentives. Reviewers suggested that future work include a review of E85 and natural gas infrastructure incentives.
SA-059	Sustainability Analysis *Marc Melaina; National Renewable Energy Laboratory*	3.2	X			Reviewers noted that the addition of the sustainability project will enhance the analysis portfolio and that the project is relevant to hydrogen supply, but that analysis should include the economic and social aspects as well. Reviewers noted that this analysis should encompass more than an "index." They said that the inclusion of stakeholders in the steering team is an excellent way to encourage and extend collaboration activities. Reviewers recommended that future work include a broader mix of hydrogen supply channels, such as liquid hydrogen, distributed natural gas reforming, and central electrolysis.
SA-060	Evaluation of Technology Status Compared to Program Targets *Marc Melaina; National Renewable Energy Laboratory*	3.0			X	The reviewers recognized that the project approach uses vehicle simulation based on program targets with the market adoption potential to create long-term scenarios. They further stated that the project outcome identifies FCEV penetration based on achieving targets. Reviewers stated that the study would benefit from evaluating the scenario based on technology development's falling short of the technical targets. Suggestions included more involvement from OEMs and hydrogen stakeholders. Reviewers recommended that future work consider consumer adoption with incentives, convenience of refueling, and comparison of sales scenarios with planned station deployment.

Project Number	Project Title *Principal Investigator Name & Organization*	Final Score	Continue	Discontinue/ Further Review	Completed	Summary Comments
SA-061	National Fuel Cell Electric Vehicle and Hydrogen Fueling Station Scenarios *Marc Melaina; National Renewable Energy Laboratory*	3.1			X	Reviewers commented that the approach and strategy of using scenario analysis was very effective in assessing the impacts of targets. However, reviewers stated that the analysis should include the impacts of not meeting Hydrogen and Fuel Cells Program targets as well. They recommended that the cost assumptions be improved to include more realistic figures for items such as land rent and electrical power supply. They also recommended that future work include regional considerations and incentives beyond zero-emissions vehicle credits and that the analysis results be reviewed by financial stakeholders.
SA-062	Expanded Capabilities for the Hydrogen Financial Analysis Scenario Tool *Marc Melaina; National Renewable Energy Laboratory*	3.4			X	Reviewers acknowledged that the project aligns well with the Hydrogen and Fuel Cell Program objective of supporting hydrogen infrastructure development, specifically with the addition of a comprehensive financial model to account for multiple cost variables. Reviewers noted that the additions made to the tool are extensive and useful in estimating the economies of refueling stations. Reviewers commended the project on the strong level of collaboration and recommended that future work consider the addition of maintenance of fueling station equipment.

Table of Contents

Introduction

The fiscal year (FY) 2016 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 6–10, 2016, 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 Advanced Research Projects Agency – Energy [ARPA-E]) in areas relevant to hydrogen and fuel cells were also presented at the FY 2016 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 2016 accomplishments and FY 2017 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 and hydrogen system 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	Aceves, Salvador	Lawrence Livermore National Laboratory
2	Afzal, Kareem	PDC Machines, Inc.
3	Ahluwalia, Rajesh	Argonne National Laboratory
4	Ahn, Channing	California Institute of Technology
5	Ainscough, Chris	National Renewable Energy Laboratory
6	Allendorf, Mark	Sandia National Laboratories
7	Ardo, Shane	University of California, Irvine
8	Arif, Muhammad	National Institute of Standards and Technology
9	Autrey, Tom	Pacific Northwest National Laboratory
10	Benard, Pierre	Hydrogen Research Institute
11	Benjamin, Thomas	Argonne National Laboratory
12	Bonner, Brian	Air Products and Chemicals, Inc.
13	Bordeaux, Christopher	Bordeaux International Energy Consulting LLC
14	Borup, Rodney	Los Alamos National Laboratory
15	Bouwkamp, Nico	California Fuel Cell Partnership
16	Bowden, Mark	Pacific Northwest National Laboratory
17	Bowman, Robert	Oak Ridge National Laboratory
18	Boyd, Robert	Boyd Hydrogen LLC
19	Brooks, Kriston	Pacific Northwest National Laboratory
20	Brown, Craig	National Institute of Standards and Technology
21	Bunnelle, Eric	Exxon Mobil Corporation
22	Burgunder, Albert	Praxair, Inc.
23	Capauno, Chris	Proton OnSite
24	Cargnelli, Joseph	Hydrogenics Corporation

No.	Name	Organization
25	Centeck, Kevin	U.S. Army, TARDEC
26	Chapman, Bryan	Exxon Mobil Corporation
27	Chernicoff, William	Toyota Motor Corporation
28	Choudhury, Biswajit	DuPont
29	Collins, William	Consultant
30	Creager, Stephen	Clemson University
31	Cullen, David	Oak Ridge National Laboratory
32	Curry-Nkansah, Maria	Argonne National Laboratory
33	Dale, Nilesh	Nissan Technical Center North America, Inc.
34	DeSantis, Daniel	SAINC
35	Dillich, Sara	Retired, U.S. Department of Energy
36	Dinh, Huyen	National Renewable Energy Laboratory
37	Dismukes , Charles	Rutgers University
38	Dobbins, Tabbetha	Rowan University
39	Edwards, David	Air Liquide Advanced Business and Technologies
40	El-Awady, Jaafar	Johns Hopkins University
41	Elrick, William	California Fuel Cell Partnership
42	Eudy, Leslie	National Renewable Energy Laboratory
43	Ewan, Mitch	University of Hawaii, Manoa
44	Farese, David	Air Products and Chemicals, Inc.
45	Fenske, George	Argonne National Laboratory
46	Fitzgerald, Jay	U.S. Department of Energy
47	Francfort, Jim	Idaho National Laboratory
48	Funk, Stuart	LMI
49	Ganesan, Prabhu	Savannah River Consulting LLC
50	Garcia Hombrados, Alberto	Fuel Cells and Hydrogen Joint Undertaking (FCH JU)
51	Garzon, Fernando	University of New Mexico
52	Gennett, Thomas	National Renewable Energy Laboratory
53	George, Paul	Battelle
54	Gittleman, Craig	General Motors
55	Grassilli, Leo	Consultant
56	Grot, Stephen	Ion Power
57	Gupta, Ram	Virginia Commonwealth University
58	Haight, Andrea	Composite Technology Development, Inc.
59	Halevi, Barr	Pajarito Powder LLC
60	Hamilton, Jennifer	California Fuel Cell Partnership
61	Han, Taehee	Nissan Technical Center North America, Inc.
62	Hartman, Brent	CSA Group
63	Herbert, Thorsten	NOW GmbH
64	Herring, Andy	Colorado School of Mines
65	Hirano, Shinichi	Ford Motor Company
66	Holladay, Jamie	Pacific Northwest National Laboratory
67	Horacek, Phil	Powertech
68	Houchins, Cassidy	Strategic Analysis, Inc.
69	Hua, Thanh	Argonne National Laboratory
70	James, Brian	Strategic Analysis, Inc.
71	Jensen, Craig	University of Hawaii, Honolulu
72	Jerram, Lisa	Navigant
73	Josefik, Nicholas	U.S. Army Corps of Engineers
74	Keller, Jay	Consultant
75	Khalil, John	United Technologies Research Center (UTRC)

No.	Name	Organization
76	Kim, Yu Seung	Los Alamos National Laboratory
77	King, Joel	U.S. Army, TARDEC
78	Knights, Shanna	Ballard Power Systems
79	Kocha, Shyan	National Renewable Energy Laboratory
80	Kongkanand, Anusorn	General Motors
81	Kopasz, John	Argonne National Laboratory
82	Kraigsley, Alison	National Institute of Health
83	Krause, Theodore	Argonne National Laboratory
84	Kuppa, Shashi	U.S. Department of Transportation
85	Kurtz, Jennifer	National Renewable Energy Laboratory
86	Lakshmanan, Balsu	General Motors
87	Lee, Doohwan	University of Seoul
88	Linkous, Clovis	Youngstown State University
89	Lipman, Timothy	University of California, Berkeley
90	Liu, Di-Jia	Argonne National Laboratory
91	Ludlow, Daryl	Ludlow Electrochemical Hardware
92	Markovic, Nenad	Argonne National Laboratory
93	Martinez, Andrew	California Air Resources Board
94	Masten, David	General Motors
95	McWhorter, Scott	Savannah River National Laboratory
96	Melaina, Marc	National Renewable Energy Laboratory
97	Miller, James	Argonne National Laboratory
98	Minh, Nguyen	University of California, San Diego
99	Mittelsteadt, Cortney	Giner, Inc.
100	Mohtadi, Rana	Toyota Motor Corporation
101	Moretto, Pietro	European Commission, Joint Research Centre
102	Mukerjee, Sanjeev	Northeastern University
103	Mukundan, Rangachary	Los Alamos National Laboratory
104	Myers, Charlie	Trenergi Corporation
105	Notardonato, William	National Aeronautics and Space Administration
106	Nyberg, Eric	Washington State University
107	Odgaard, Madeleine	IRD Fuel Cells LLC
108	Oesterreich, Bob	Air Liquide
109	Olson, Gregory	Consultant
110	Ott, Kevin	Los Alamos National Laboratory
111	Parks, George	FuelScience LLC
112	Patel, Pinakin	Fuel Cell Energy, Inc.
113	Peden, Chuck	Pacific Northwest National Laboratory
114	Perry, Mike	United Technologies Research Center
115	Pivovar, Bryan	National Renewable Energy Laboratory
116	Polevaya, Olga	Nuvera Fuel Cells, Inc.
117	Prasad, Ajay	University of Delaware
118	Quackenbush, Karen	Fuel Cell and Hydrogen Energy Association
119	Ramirez-Cuesta, Timmy	Oak Ridge National Laboratory
120	Ramsden, Todd	National Renewable Energy Laboratory
121	Rice, Brian	University of Dayton Research Institute
122	Richards, Mark	Fuel Cell Energy, Inc.
123	Rinebold, Joel	Connecticut Center for Advanced Technology, Inc.
124	Rohatgi, Aashish	Pacific Northwest National Laboratory
125	Rousseau, Aymeric	Argonne National Laboratory
126	Rowe, Ian	U.S. Department of Energy
127	Rufael, Tecle	Chevron Corporation

No.	Name	Organization
128	Sandrock, Gary	Consultant
129	Serov, Alexey	University of New Mexico
130	Serre-Combe, Pierre	CEA (Alternative Energies and Atomic Energy Commission [France])
131	Siegel, Don	University of Michigan, Ann Arbor
132	Sievers, Robert	Teledyne Energy Systems
133	Sofronis, Petros	University of Illinois, Urbana-Champaign
134	Soto, Herie	Shell Oil Company
135	Spendelow, Jacob	Los Alamos National Laboratory
136	Stamenkovic, Vojislav	Argonne National Laboratory
137	Stavila, Vitalie	Sandia National Laboratories
138	Steinbach, Andy	3M
139	Steiner, Nadia	Universite de Franche-Comte
140	Stottler, Gary	General Motors
141	St-Pierre, Jean	University of Hawaii, Manoa
142	Swartz, Scott	NexTech Materials LTD
143	Swider-Lyons, Karen	U.S. Navy, Naval Research Laboratory
144	Tamhankar, Satish	Linde
145	Tchouvelev, Andrei	A.V.Tchouvelev & Associates Inc.
146	Thomas, Sandy	Clean Car Options
147	Toughiry, Mark	Department of Transportation
148	Tran, Thanh	U.S. Navy
149	Tsimis, Dionisis	Fuel Cells and Hydrogen Joint Undertaking (FCH JU)
150	Udovic, Terry	National Institute of Standards and Technology
151	Ulsh, Michael	National Renewable Energy Laboratory
152	Valdez, Thomas	National Aeronautics and Space Administration – Jet Propulsion Laboratory
153	Vanderborgh, Nicholas	Los Alamos National Laboratory
154	Veenstra, Mike	Ford Motor Company
155	Verduzco, Laura	Chevron Corporation
156	Vogel, John	Combined Energies LLC
157	Wagner, Frederick T.	General Motors
158	Waldecker, James	Ford Motor Company
159	Wang, Conghua	TreadStone Technologies, Inc.
160	Weber, Adam	Lawrence Berkeley National Laboratory
161	Wei, Max	Lawrence Berkeley National Laboratory
162	Wheeler, Douglas	DJW Technology LLC
163	Williams, Mark	National Energy Technology Laboratory
164	Woods, Stephen	National Aeronautics and Space Administration
165	Xu, Hui	Giner, Inc.
166	Yan, Yushan	University of Delaware
167	Yandrasits, Michael	3M
168	Zelenay, Piotr	Los Alamos National Laboratory

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 program. Comments about program management are provided in Appendix B.

Analysis Methodology

A total of **131** Fuel Cell Technologies Office (FCTO) projects were reviewed at the meeting. As shown in Table 1, **168** review panel members participated in the AMR process, providing a total of **716** 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 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 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%)

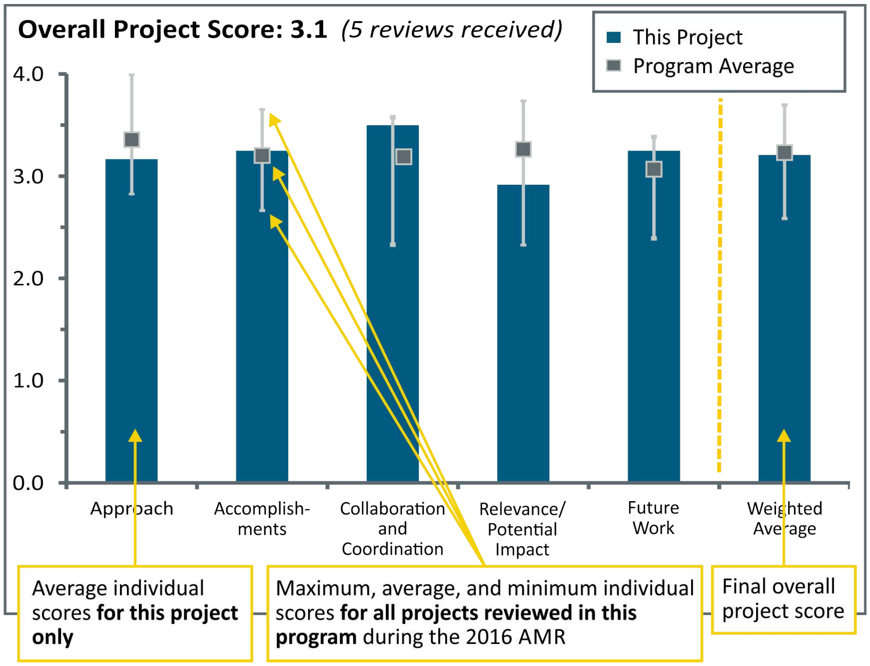
For all programs, reviewers were also asked to provide qualitative comments regarding the five criteria, specific strengths and weaknesses of the project, and any recommendations relating to the work scope. These comments were also entered into the online, private database for easy retrieval and analysis.

Organization of the Report

The project comments and scores are grouped by program (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 FCTO's planning scheme. Each of these sections begins with a brief description of the general type of research and development or other activity being conducted. Next are the results of the reviews of each project presented at the 2016 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 program. 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 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 program. 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, average, and minimum values for all of the projects in Project A's program (the last three lines in Table 2) 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

2016 — Hydrogen Production and Delivery
Summary of Annual Merit Review of the Hydrogen Production and Delivery Program

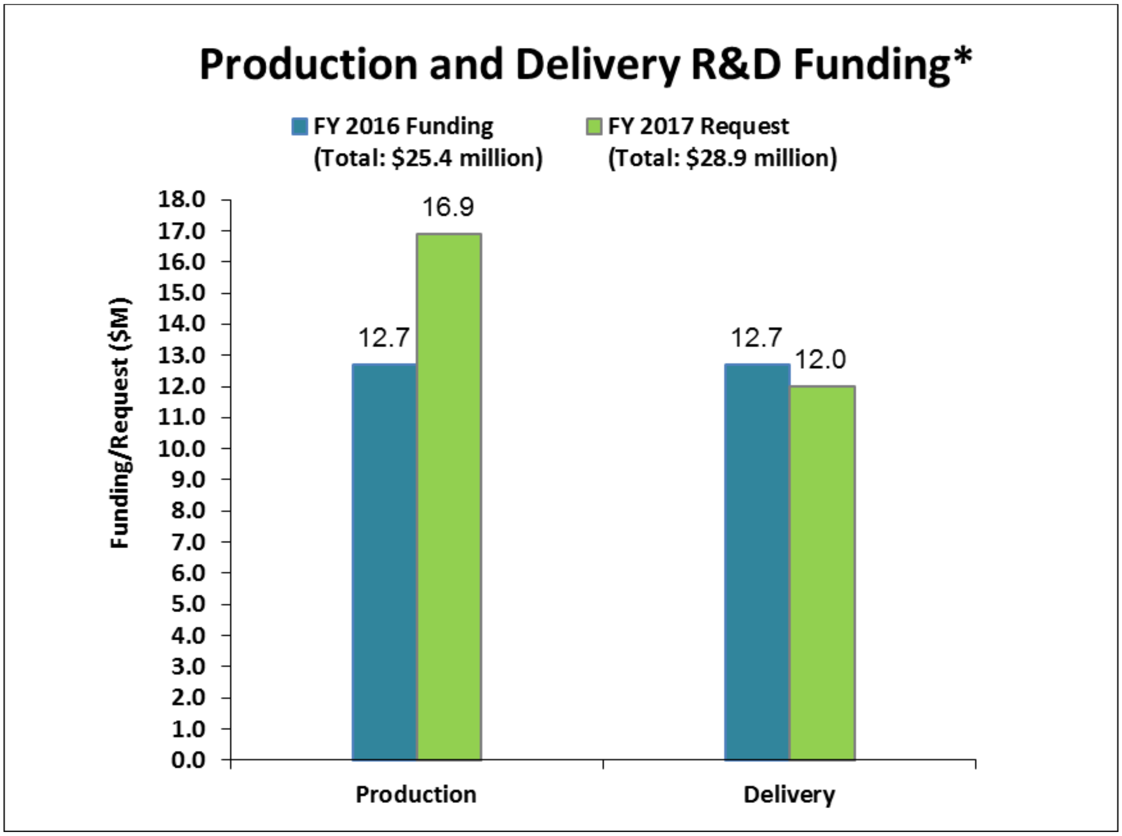
Summary of Reviewer Comments on the Hydrogen Production and Delivery Program:

This review session evaluated hydrogen production and delivery research and development (R&D) activities in the U.S. Department of Energy's (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, as well as an overarching analysis of hydrogen production pathways. Production project sub-categories included thermal and thermo-electrochemical conversion of bio-derived feedstocks, advanced water splitting, direct solar thermochemical (STCH) and photoelectrochemical (PEC) water splitting, biological hydrogen production, and hydrogen production pathway analysis. The hydrogen delivery projects reviewed included R&D for low-cost, reliable delivery technologies (pipelines and tube trailers), hydrogen fueling station components (compression, storage, and dispensing), novel liquefaction technologies, and strategic delivery techno-economic pathway analysis.

The reviewers recognized the Hydrogen Production and Delivery program as focused, effective, well managed, and having a clear strategy to achieve DOE goals and objectives. Reviewers commented positively on the relevance of delivery projects to both near- and long-term priorities and needs, and they commended the achievements of production projects in innovative systems design and use of techno-economic analysis. Reviewers encouraged more detailed studies of both near-term and future costs of production and delivery technologies, especially those at lower technology readiness levels (TRLs); reviewers also recommended continued leveraging of relevant industry-partnership opportunities and greater collaboration with other domestic and international government agencies. Continued and strengthened emphasis on industrial collaboration and stakeholder engagement was strongly recommended.

Hydrogen Production and Delivery Funding:

The fiscal year (FY) 2016 appropriation for the Hydrogen Production and Delivery program was $25.4 million, with funding distributed approximately evenly between hydrogen production and hydrogen delivery technologies. The production portfolio funding focus in FY 2016 was on advanced water splitting pathways such as STCH, PEC, and other electrolysis technologies, as well as the addition of new fermentative hydrogen production projects competitively selected in FY 2015. This emphasis will continue in FY 2017 with the addition of a newly organized advanced materials consortium and additional high temperature electrolysis work. FY 2017 planning is based on a $28.9 million budget request (~$16.9 million apportioned to production R&D). The consortium approach to identifying and utilizing resource nodes to accelerate development of renewable hydrogen production pathways will continue to be developed. The delivery portfolio emphasis in FY 2016 was on reducing station technology costs, such as those associated with storage vessels and dispensing hoses, and on advanced compression and liquefaction technologies for the mid- to long-term. The portfolio also includes funding for hydrogen fueling infrastructure analysis. This emphasis will continue in FY 2017 with ~$12.0 million apportioned from the budget request with an additional focus on the reliability of critical components, such as forecourt compressors, meters, and other dispensing equipment.



* 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:

Twenty-nine projects were reviewed, receiving scores ranging from 2.30–3.50, with an average score of 3.15. The scores are indicative of the technical progress that has been made over the past year in the hydrogen production and delivery R&D portfolio.

Production Projects

Hydrogen Production Pathway Analysis: One project was reviewed in the area of hydrogen production pathway analysis. The project received a score of 3.1. Reviewers recognized the high-impact and usefulness of the techno-economic analyses performed by the project team, as well as the team's expertise and experience in this area. They would have liked to have seen more information from relevant industry partners in the development of the case studies, though they acknowledged the challenges presented by the low TRL of the dark fermentation and solid-oxide electrolyzer cell (SOEC) cases presented. Reviewers would also like to have seen further details on the technical and economic assumptions of the analysis. They specifically expressed concerns about the aggressiveness of some of the assumptions presented for the future cases studies.

Advanced Electrochemical Water Splitting: Four projects in the area of hydrogen production from advanced electrochemical water splitting were reviewed, receiving an average score of 3.3. Projects included efforts to decrease the platinum group metal (PGM) loading of the proton exchange membrane (PEM) electrolysis cells; efforts to completely eliminate PGM materials and enhance membrane stability in anion exchange membrane (AEM) electrolysis cells; efforts to understand electrolysis under variable electrical load; and the development of SOECs operating at extremely high current density. Reviewers praised the progress on low-PGM PEM and non-PGM AEM electrodes while maintaining performance and durability compared to commercial baselines with higher-PGM electrodes. They also praised the progress on the demonstration of current density exceeding 3A/cm^2 in SOEC cells. They commented that success in these projects offers the potential to achieve significant reduction in the capital cost of electrolyzers, which is critical for technology introduction on a larger scale. All projects in the category have completed or are on track to complete major milestones. Reviewers recommended continuing work to better understand mechanisms of the electrochemical processes in the electrolyzer stack, further optimize operating conditions for each technology, and better evaluate the long-term hydrogen cost ramifications of technologies improvements.

Bio-Derived Feedstock Conversion: One project was reviewed in the area of bio-feedstock conversion, with a score of 3.0. Reviewers commended the project team for its progress in developing and testing innovative catalysts and carbon sorbent materials as well as for its strong, multi-partner collaborations. They expressed concern over the significant operational challenges facing the swing reactor system integration and control. The reviewers also questioned the assumptions of the reactor's fuel flexibility and bio-oil feedstock costs in terms of the system's performance and potential to meet the hydrogen production cost goal.

Biological Hydrogen Production: Two projects were reviewed in the area of biological hydrogen production and these received an average score of 3.16. One project is focused on microbial processes, pairing fermentation of biomass with Microbial Electrolysis Cells (MECs) to produce hydrogen from the waste effluent. The other is an in vitro process using isolated enzymes to convert starches to hydrogen. Reviewers commended both projects on their accomplishments and making progress toward their goals, but they expressed some concern about overall feasibility of biological conversion of biomass to hydrogen. The reviewers noted that the in vitro biosystems project was pursuing a novel pathway and commended the progress toward protein expression and peak production rates. Reviewers questioned the practicality of the project, noting that the techno-economic analysis presented was mostly qualitative and may not have considered all costs. Reviewers complimented the fermentation and MEC project for its effective partnerships. Reviewers questioned the value of studying parallel feedstock pretreatment methods in the fermentation project and requested additional details and quantitative results for some tasks.

PEC Hydrogen Production: Three PEC projects were reviewed, receiving an average score of 3.33. 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 and chalcopyrite thin-film materials. Projects were rated highly for advancing the efficiency of PEC devices through improvements in the interfaces between materials. Reviewers highlighted the use of qualified collaborators who contributed unique expertise and capabilities to the projects. Recommendations for future work included placing stronger focus on increasing the stability and durability to meet upcoming milestones.

STCH Hydrogen Production: Three projects were reviewed in the area of STCH hydrogen production projects, with an average score of 3.0. Two of the projects focus on two-step, metal-oxide-based reaction cycles, and the third addresses a hybrid sulfur (HyS) reaction cycle, which includes an electrolysis step. Reviewers praised the innovative approaches and achievements in all three projects, including the following: (1) combined experimental and modeling efforts for materials discovery; (2) reactor design and prototype builds that will allow for hydrogen production demonstrations; and (3) the screening and characterization of advanced membranes and a system design that allows for 24-hour operation for the HyS cycle. Reviewers also recognized key challenges in all three projects, such as their potential to meet the hydrogen cost goal and, again, they recommended continued efforts on the techno-economic analysis for these technologies. They specifically recommended including realistic capital costs; better definitions of assumptions; and an effort to establish materials discovery approaches, testing protocols, and reporting standards for the STCH community.

Delivery Projects

Hydrogen Delivery Techno-economic Analyses: Three projects were reviewed in this area, with an average score of 3.1. These projects included updates of the Hydrogen Delivery Scenario Analysis Model (HDSAM) to version 3.0, analysis of the energy and cost of hydrogen precooling systems, and techno-economic analysis of the cryo-compressed delivery pathway. Projects were praised by reviewers for their technical robustness and relevance to DOE objectives. Recommendations were made for projects to collaborate more closely with industry partners and to more clearly explain the basis of the assumptions presented.

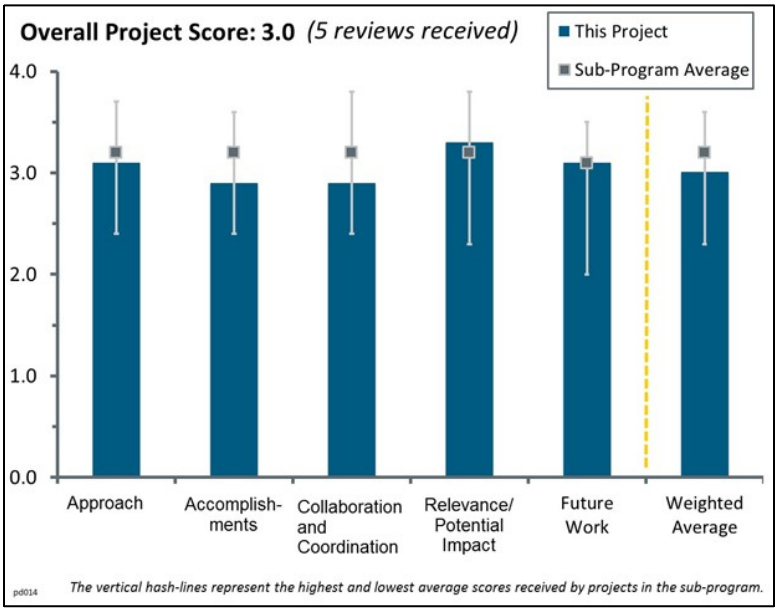
Hydrogen Delivery Technologies: One project was reviewed in the area of hydrogen pipelines, receiving a score of 3.1, and two projects were reviewed in the area of hydrogen liquefaction, receiving an average score of 3.4. The pipeline project was praised for technical robustness (testing in high-pressure environments, studying welds, accounting for residual stresses, and testing multiple types of fiber-reinforced pipeline). Reviewers suggested that the steel pipeline project collaborate more closely with industry to ensure real service conditions are represented. Reviewers praised the liquefaction projects for using novel approaches that could have significant impacts on hydrogen production costs, as well as their accomplishments to date. Suggestions included increasing industry collaboration, characterizing cost, and focusing on scale-up potential.

Hydrogen Fueling Station Technologies: Nine projects were reviewed on hydrogen dispensers, compression, storage, and station operation. They received an average score of 3.0. The projects on dispensing hoses were praised for their technical approach and relevance, which included providing accelerated cycle life testing for key components of the hydrogen station, including fittings. The projects on compression technologies were praised for the potential to lower station costs and improve reliability if successful. Reviewers expressed concern over the project's thermodynamic efficiency compared to incumbent technologies and suggested that the team obtain guidance from experts in electric motors and compression. Reviewers expressed concern with the ability of the steel concrete composite vessels (SCCV) to compete with incumbent storage technologies. They commented that the design of the high-pressure (875 bar) SCCV has not been optimized for fatigue life and that the costs of transporting and installing the SCCV technology may negate any cost savings associated with the vessel. The hydrogen storage project on wire wrapping Type I vessels was commended for its approach and promising burst test results. Reviewers suggested that the cost of this technology be assessed in greater detail. The project on tube trailer consolidation strategy testing and development was praised for its potential for near-term cost reduction, but reviewers suggested that additional industry collaboration would be helpful.

Project #PD-014: Hydrogen Delivery Infrastructure Analysis

Krishna Reddi; Argonne National Laboratory

Brief Summary of Project:



This project will assess impacts of delivery and refueling options on the cost of dispensed hydrogen by (1) modeling refueling costs in early fuel cell electric vehicle markets, (2) evaluating the impact of design and economic parameters, (3) identifying cost drivers of current technologies, and (4) developing estimates of delivery and refueling cost reduction with market penetration. The project aims to support existing U.S. Department of Energy (DOE)-sponsored tools and assist with Fuel Cell Technologies Office (FCTO) planning.

Question 1: Approach to performing the work

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

- The approach appears to cover all the major delivery methods. However, the presentation did not make clear how the cost impact of variations within a delivery method was considered. For example, tube trailers today carry hydrogen at 200–350 bar. Some gas merchants have or are planning 400–450 bar trailers as well. It is unclear whether the scope of the H_2A Delivery Scenario Analysis Model (HDSAM) is meant to model transportation costs of hydrogen or the entire delivery cost, i.e., including the capital and operating expenses (capex and opex) of the hydrogen refueling station (HRS) itself. The project references “Delivery Infrastructure Analysis”; however, it appears only transportation costs are considered. In considering the total delivery cost of hydrogen from terminal to end user, a particular delivery method can affect the capex and opex of the HRS itself. This is a strategy being considered by gas merchants that control the transport and HRS portions of hydrogen delivery. Again using the example of tube trailers, by using higher-pressure tube trailers, the cost of terminal compression and trailer capex would increase, but capex and opex of the HRS would decrease—a smaller or more efficient compressor could be used, owing to higher pressure to the inlet of the compressor. Another approach could see the tube trailer being used as the “low bank” in a cascade system in addition to supplying hydrogen for compression to the “high bank,” reducing the amount of storage capex for the station (in other words, moving HRS capex to transportation capex). This would have advantages in the near term when a singular capex in the trailer would increase the asset utilization while decreasing capex costs across multiple HRSs.
- The overall approach with detailed scenario modeling is reasonable and appropriate. The challenge is to properly account for variable input data and uncertainty of assumptions with regard to key parameters. The investigators are continually refining the model to improve on the quality of the output.
- The model is still limited by the use of cost estimates vs. after-turn-key cost checks. The source of the cost reduction factors is unclear (this was answered late in the presentation but is still not very clear or, at a minimum, is not transparent or substantiated). The presentation should make the data sources more transparent and referenceable/reviewable, and allocate some additional time to ensuring the appropriate or reasonableness of the inputs. The work is limited by the lack of data. The approach needs to evolve to accommodate these limitations to better inform DOE’s research direction. It is hard to have confidence in the results.
- How far industry really uses the results for their research, development, and demonstration strategy is questionable.

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 made good progress in introducing various technology states into the model and further tuning the existing consideration factors.
- Version 3.0 of the model appears to be a step change improvement over the previous version (2.3). Significant progress has been made in examining various scenarios and options since the last Annual Merit Review. However, some of the results presented appear to be unclear and confusing. It is unclear whether sufficient data were available to project the component cost trends presented. Proper representation and better clarity would be helpful. The results are shown to project delivery cost for the long-term, high-volume case via all the three options—gaseous tube trailer delivery, liquid hydrogen (LH2) delivery, and pipeline delivery—to be about the same at $4/kg. It is not clear how this is possible. Some pathways are just not feasible or practical for a given scenario; e.g., a tube trailer cannot deliver to multiple large stations at 1,000 kg/day capacity (slide 8). It is not clear whether the tube trailer is assumed to be at current 200 bar pressure or at 500 bar or some other pressure—that will also have an impact. The baseline assumptions appear inconsistent; e.g., while cost comparisons show HRS capacity of 1,000 kg/day, the emissions results show capacity of 500 kg/day (slide #13). There are inaccuracies in the data presented that should be corrected. Market penetration numbers are different in different slides (2% vs. 10%), which makes it confusing to understand and compare the results. The conclusion on slide 17 does not seem to be consistent with the graph shown, if it is for the 700 bar case.
- Both main delivery pathways (gaseous hydrogen (GH2) tube trailer and LH2 truck) increased by roughly 10%. This would not be considered a "small increase," as mentioned on slide 8. It is not clear what the consequences are or how this is fed back to industry. Results on slides 14–16 are not very surprising. Slide 17 shows an overall trend in the wrong direction. Again, the consequences and how this fed back to industry are unclear.
- The model, or the results as they were shared, is limited. The lack of sensitivity and uncertainty consideration limits the usefulness to use for a key objective in identifying cost drivers. The large price differences between HDSAM 2.3 and HDSAM 3.0 should serve as a warning and lesson on the dependence of discrete price quotes.

Question 3: Collaboration and coordination with other institutions

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

- Good collaboration efforts and partner participation are indicated. Additional input from component manufacturers, gas suppliers, and station operators would be helpful.
- Collaborations exist but do not seem to have addressed concerns.
- As of the presentation, there is a huge lack of industry collaboration. It is not clear how much industry players such as Linde, Air Liquide, and Air Products are contributing. Maybe the project could elaborate more on that in next year's presentation.

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 critical activity. The results should help determine key cost factors on which to focus as well as suggest commercial feasibility of different pathways under different conditions. The results presented make an impact on outsiders' views of the feasibility of hydrogen fuel cell vehicles. Therefore, it is important that the data are truly representative and that the results are accurate.
- The work being done is necessary to understand the current and projected costs in the hydrogen infrastructure space.

- The project is of high value for FCTO to set priorities and cost and performance targets.
- The work, as it is presented, is too high-level.

Question 5: Proposed future work

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

- Everything seems to be covered.
- The future work addresses essential key parameters and updates. The addition of a cryo-compressed pathway would be useful.
- Future work captures some important areas in which data or information is needed to improve usefulness of the modeling. However, the proposals seem focused more on precision than on accuracy.

Project strengths:

- With so many hydrogen transport technologies being explored and considering the variations within those technologies, the project team has tackled a challenging problem with good success. The approach appears to be sound—just further honing of the model is needed.
- Strengths include a strong modeling background, well-thought-out selection of pathways and corresponding parameters, and deep understanding of fundamentals and key issues.
- The project is trying to focus on empirical data collection and expert/industry input.
- The project provides an in-depth analysis tool for FCTO.

Project weaknesses:

- If the researchers have collected information on the range of costs, both from the production/equipment side and regional/local issues, it seems to have been condensed into a single value rather than used as a range of values (i.e., 90% range and median or mean). Without that range, it is not clear how a practitioner should or would use the model to evaluate an actual station design. This was effectively brought up in the prior year review, and it does not seem to have been addressed. Data sources, references, etc. need to be more transparent. At a minimum, a summary of the input data needs to be provided. The reliance on a single individual for the cost reduction values is concerning, especially since there is little to no evidence provided on the validity.
- Data for some parameters are insufficient, and there is a lack of consistency of assumptions and clarity in presentation of results.
- As of the presentation, there is a lack of direct industry involvement.

Recommendations for additions/deletions to project scope:

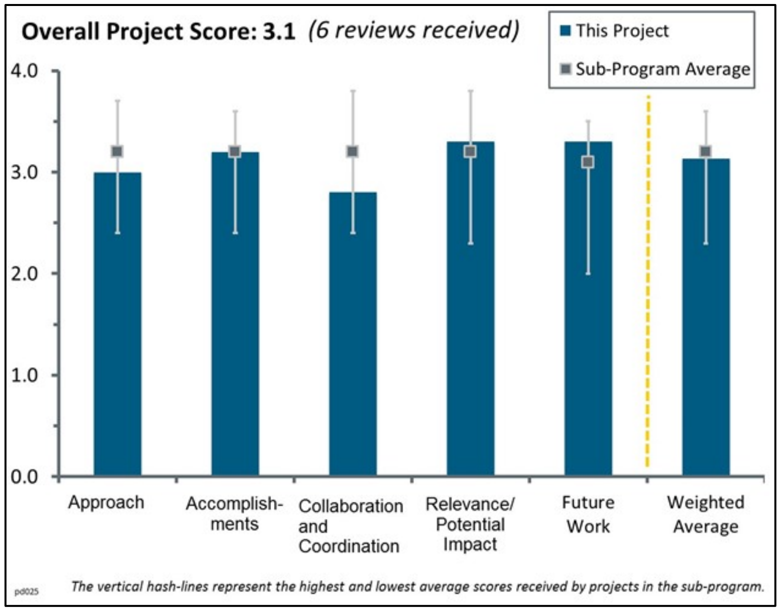
- It would be helpful to add sensitivity analysis and show range bars indicating uncertainty. Additional realistic scenarios and refinement of current pathways with technological advances and improved understanding as well as data availability would be helpful.
- Output next year needs to emphasize the range of real-world costs and emphasize the range of outcomes.
- Regular reviews with industry should be added.

Project #PD-025: Fatigue Performance of High-Strength Pipeline Steels and Their Welds in Hydrogen Gas Service

Joe Ronevich; Sandia National Laboratories

Brief Summary of Project:

The primary objective of this project is to enable deployment of high-strength steel for hydrogen pipelines to facilitate cost reductions. Specific goals are to (1) demonstrate fatigue performance of high-strength girth welds in hydrogen gas and compare performance to low-strength pipe welds, and (2) establish models that predict pipeline behavior as a function of microstructure in hydrogen to inform future development.



Question 1: Approach to performing the work

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

- It appears that the original schedule and scope of work are being followed according to plan. The approach and methods proposed continue to be followed in the early phases of this work.
- The experimental approach followed by Sandia is the best there is. Assessing the fatigue crack growth resistance of low and high strength welds is the safest way to go if we are to increase pipeline strength to reduce cost. In addition, the proposal that crack growth through base material, the heat affected zone, and weld be investigated under constant stress intensity factor range (see slide 12) is also a well-thought-out and targeted approach to save time and resources. However, there was no information about the National Institute of Standards and Technology (NIST) approach on predictive modeling, nor about the Oak Ridge National Laboratory (ORNL) work on promising microstructures for enhanced hydrogen resistance.
- In general, the approach was sound. The principal investigator (PI) discussed that cyclic loading is the core difference between station pipelines and regular industrial pipelines. It did not seem that the PI performed cyclic testing, and this prevented the project from being rated a 3.5 for its approach.
- The approach is mostly centered on quantifying the effect of the microstructure on the fatigue performance and crack propagation in hydrogen gas environments. The team focus on samples extracted from different regions near and at the welds is well-thought-through. However, it is not clear how the team will utilize the data from the experiments, or how the data will help in developing the models to predict pipeline behavior as a function of microstructure. The presentation gives a sense that the approach is mostly empirically based, with no significant feedback between modeling and experiments.
- The results are communicated to relevant code committees, but the approach does not seem to include a review process with 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 project demonstrated that friction stir welding (FSW) and gas metal arc welding (GMAW) welds experience almost identical hydrogen-induced acceleration of fatigue crack growth. This is an interesting result by itself, as it holds promise for higher-strength pipelines. The identification of crack pathways for

specimen preparation and subsequent testing is also a well-thought-out accomplishment, indicative of a systematic plan.
- Consistent results prove the testing protocol is good and repeatable. The protocol has a great ability to provide results that can be used to calculate wall thickness using measured crack growth laws.
- This project started less than a year ago. The team has already conducted experiments in both air and in hydrogen for a couple of different pipes.
- The preliminary microstructural evaluations, specimen preparation, and techniques are being conducted according to the proposed plan. Future tests appear to be on schedule.
- Promising basic findings have been achieved, and good testing facilities built up. There is no transparent documentation of potential and achievements compared to DOE targets.
- Well-defined standard development.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- Good, clear collaboration with industry and national labs.
- The collaborations between ORNL and NIST have been clearly articulated. The extent of the involvement of the Colorado School of Mines (CSM) and ExxonMobil is not yet clear. It is not clear if their roles will increase in years 2 and 3.
- The collaboration with partners appears to exist, particularly with ORNL. However, it is yet to be seen how effective collaboration with CSM will be. The Gantt chart references NIST, but the work scope appears to be from CSM.
- Collaboration with ORNL holds promise because of ORNL's capabilities in assessing weld microstructures. The reviewer could not comment on the collaboration with NIST because their approach was not outlined in the presentation.
- On slide 14, it is mentioned that the industry collaboration means that they (in the past) supplied pipes. It is unclear if the collaboration is limited to that or if there are review loops with industry. It is unclear how project results are fed back to industry.

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.

- Quantifying microstructure-fracture relationships in hydrogen is very important to aid in the development and validation of physics-based predictive models. It is clear that this has the most potential impact.
- The project can be impactful on cost reduction of pipelines. In addition, the project has the potential to impact understanding of weld response to hydrogen-induced fatigue. The profession's understanding is not very advanced in this area
- The understanding and relevance of work to program objectives are excellent and well-aligned. It is expected that the results of the project will be relevant.
- The opportunity to use high pressure pipelines is good, although somewhat down the line. New and expensive infrastructure would be required, along with expensive and lengthy permitting.
- There is no transparent documentation of potential and achievements compared to DOE targets.
- Currently, composite overwrapped pressure vessels (COPVs) provide much more relevant promise than these pressure vessels for storage of hydrogen.

Question 5: Proposed future work

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

- Reviewing new generation steel is a long-term possibility and is a needed and good approach.

- This project started nine months ago, so the bulk of the work proposed has yet to be completed. The reviewer feels the work appears well-planned and understood, and believes the PI is well-prepared to complete the scope of work.
- The proposed future work seems adequate towards the goals of the project. However, some emphasis on quantifying the role of microstructure on crack initiation in hydrogen would also be of significant importance for the predictive models.
- The reviewer assumes the use of Gleeble aims at developing specific microstructures that will replicate the transitioning of the microstructure from the weld to the baseline material. This is a nice approach, but no details have been given as to whether this replication of the microstructure is relevant to that in real-world welds.
- Everything seems to be covered.

Project strengths:

- The capabilities and tests planned appear to align well with the original proposed scope.
- Good systematic approach and proposed way forward.
- Well-thought-out and relevant in the long term.
- The involvement of Sandia and ORNL.

Project weaknesses:

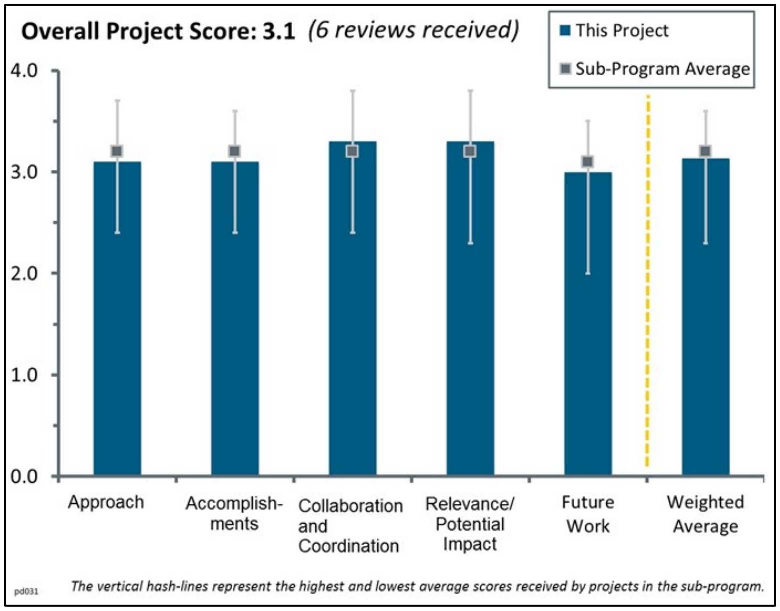
- None.
- No weaknesses were identified.
- Nothing has been outlined on the contribution from NIST, and only limited information regarding the contribution from ORNL.
- No benchmark to DOE targets. What is the potential contribution of the project? Industry collaboration.

Recommendations for additions/deletions to project scope:

- The project demonstrated that FSW and GMAW welds experience almost identical hydrogen-induced acceleration of fatigue crack growth. This is an interesting result by itself as it holds promise for higher strength pipelines. However, this conclusion needs to be explained mechanistically based on the microstructures involved. In fact, this conclusion may change depending on the frequency used at testing. It could be that at lower frequency and higher R ratios, the higher strength weld can be more susceptible to hydrogen accelerated fatigue crack growth. In addition, the project should test the threshold stress intensity factor range for hydrogen-induced accelerated fatigue. The reviewer believes the two thresholds for the low and high strength welds will be found to be different. It is the reviewer’s understanding that acicular ferrite microstructures are the most resistant to hydrogen-induced failure. This is a general belief that to the reviewer’s knowledge has not been tested experimentally against fatigue crack growth. It is unclear that Sandia and ORNL plan to explore this issue.
- Include some efforts targeting the role of microstructure on crack initiation.
- It is recommended that the role and relationship of CSM with NIST be more clearly identified. Related to this, having CSM identified in the Gantt chart seems appropriate, as it seems their participation is significant and on the same scale as NIST.

Project #PD-031: Renewable Electrolysis Integrated System Development and Testing

Michael Peters; National Renewable Energy Laboratory



Brief Summary of Project:

The objectives of this project are to (1) validate cell, stack, and system electrolyzer performance; (2) explore and optimize electrolyzer system efficiency and performance under varying power operation as well as integration with hydrogen infrastructure components; and (3) track the progress over long-duration testing. These objectives support the goals of integrating electrolyzers with intermittent renewable power sources as well as increasing the durability of electrolyzer stacks operating under variable loads while maintaining high system efficiency.

Question 1: Approach to performing the work

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

- The approach is well organized, and the objectives address Barriers G, I, J, and L well. The project does a good job at evaluating the effect of variable operation with renewable energy systems (photovoltaic [PV] solar and wind) on stack durability. The hydrogen savings through the variable drying and the direct current (DC) balance of plant (BOP) power that will be explored is also a positive aspect of the project.
- There is excellent integration of hardware demonstration with industry collaboration.
- The approach is relevant, including investigating effects of variable loads and variable versus fixed drying, and how both affect efficiency and hydrogen purity; however, the project scope seems somewhat small.
- This seems to be a well-thought-out approach. Slide 3 is not appropriate; it states that renewable hydrogen is necessary "to make a significant impact" on greenhouse gas (GHG) emissions. Coal-based integrated gasification combined cycle (IGCC) and carbon capture and storage (CCS) can make significant reductions in GHGs: in the electrical sector, hydrogen from IGCC and CCS would reduce GHGs by a factor of 12.8 compared to a coal-ST plant and by 5.8 times compared to a natural gas combined cycle plant. In the vehicle sector, steam methane reformation (SMR) to produce hydrogen for a fuel cell electric vehicle (FCEV) reduces GHGs by 22% compared to a gasoline hybrid and by 52% compared to a conventional gasoline internal combustion vehicle. These reductions are significant. The project should be careful not to let the perfect (renewable hydrogen) be the enemy of the good (SMR hydrogen).

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 experiments showed that variable and constant drying show no major differences in terms of degradation. Hydrogen savings were demonstrated through the variable drying approach. These contribute to the reduction of costs of hydrogen produced from renewables, which contributes to the target of $2 per gasoline gallon equivalent by 2020.
- Demonstrating that electrolyzer performance is not degraded by variable inputs is a very good achievement.

- A project cannot do much better than this one did; all milestones in fiscal year (FY) 2015 and FY 2016 were complete, and future milestones are on track. Also noted is a new advance in which dried hydrogen was fed to linear actuated valves to control hydrogen loss and DC/DC converters were used to run pumps. There were significant hydrogen savings from the variable drier. The only major concern is that specific Multi-Year Research, Development, and Demonstration Plan (MYRDDP) targets were not explained in great detail. Also, the high silica content found in the stack water should be addressed in the next iteration.
- Multiple parallel investigations show considerable progress. The team seems to draw relevant and insightful conclusions from data as opposed to just reporting numerical results. Analysis of decay rates seems to show a wide range of results depending on stack and testing protocol. It is not clear that meaningful conclusions are drawn from the testing (or can be drawn from the data). Replacement of the constant hydrogen dryer losses with a variable loss (due to a variable valve) is a clear improvement. What is not clear is why Proton Onsite has not done this already on its own. It seems like a basic systems integration and optimization task. Examination of the all DC BOP architecture is a task well suited to the National Renewable Energy Laboratory (NREL) with consultative input from Proton OnSite.
- Quantifiable DOE goals for this project were not apparent, but the importance of performance assessments to advance general DOE goals is understood.

Question 3: Collaboration and coordination with other institutions

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

- Clear work involves Xcel Energy, Proton OnSite, Giner, and others at NREL.
- Collaboration with Proton Onsite seemed particularly beneficial with respect to evaluating electrolyzer degradation.
- The partners in this project all have valuable expertise and seem to be working well together.
- The project is teamed with very appropriate and experienced partners.

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 focuses on tasks well aligned with DOE goals. Understanding and tracking real-world stack decay rates is vital to the long-term success of electrolysis. BOP reliability and loss minimization is also vital to DOE goals.
- This project is essential to enabling production of large quantities of renewable hydrogen from solar PV and wind.
- This project can facilitate an increase in the share of renewables by providing storage opportunities, as well as grid services through frequency regulation. The hydrogen savings and increased efficiency through the DC BOP can also contribute to DOE goals.
- It is unclear to what extent this work supports progress towards MYRDDP targets because of the terse description of indicators and the lack of tie-in to those targets in the slides.

Question 5: Proposed future work

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

- The proposed future work is essential to showing the potential of this off-grid approach.
- A future focus on power regulation and variable voltage integration with renewables is appropriate.
- Emphasis on direct coupling to renewables is appropriate.
- Planned advances include a voltage measuring device for each cell in the stack so that voltage can be measured at any point in time. One additional barrier is that MATLAB is needed for the full computational analysis, yet the project does not have money for it.

Project strengths:

- The project is evaluating DC/alternating current (AC)/AC/DC, which is how the grid would supply power, and not DC/DC, which would be the case with a standalone integrated renewable source.
- The project is addressing the barriers very well and has been well organized. The partners are experienced and have very good expertise. The innovation potential with all DC BOP components is very good.
- NREL is well suited to analyzing the long-term collection and analysis of data and is uniquely qualified to conduct variable power load testing and modeling.
- The collected data could be of value to various stakeholders in fine-tuning their energy storage designs.
- There is good cooperation with the electrolyzer manufacturer.

Project weaknesses:

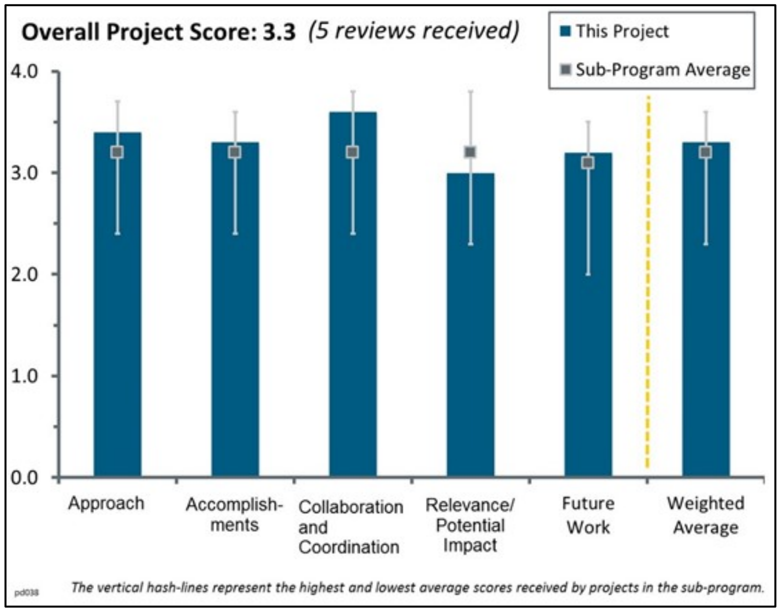
- The reviewer did not see any specific weaknesses.
- It would have been better if the original hours of the first two stacks were known.
- The project should make sure to critically assess the feasibility plan of the proposed research and development and expound on how it relates to MYRDDP targets.
- The decay rate data appear to be quite variable, with insufficient analysis to discern the causes of the variation.
- The project is not deriving mechanistic understanding that would be needed when translating the results to industrial scenarios.

Recommendations for additions/deletions to project scope:

- The project should make the data publicly available so that different users can come up with their own conclusions.
- It is not sufficient merely to measure and quantify performance. That is a necessary and appropriate first step but must be followed up with additional analysis to draw conclusions from the data.

Project #PD-038: Biomass to Hydrogen (B2H2)

Pin-Ching Maness; National Renewable Energy Laboratory



Brief Summary of Project:
The objective of this project is to develop direct fermentation technologies to convert renewable lignocellulosic biomass resources to hydrogen. The project addresses techno-economic feasibility of hydrogen production via biomass fermentation in three tasks. Task 1 optimizes bioreactor performance, focusing on de-acetylated and mechanically refined (DMR) biomass to lower feedstock costs. Task 2 focuses on using ionic liquid (IL) pretreatment for biomass processing. Task 3 develops and applies genetic tools to modify metabolic pathways aimed at improving hydrogen molar yield. Task 3 integrates a microbial electrolysis cell (MEC) reactor into the system, producing hydrogen while cleaning the fermentation effluent to improve the overall hydrogen molar yield.

Question 1: Approach to performing the work

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

- The team of experts, from multiple national labs and universities, has significantly improved hydrogen production and production rate while reducing production costs through advancing multiple experimental components, including the bioreactor batch process, use of DMR processing of corn stover, and the double elimination of the two pyruvate pathways.
- The approach is sound. The group is making progress on the stated aims while also addressing potential barriers and challenges.
- The engineering of *C. thermocellum* to overcome the hydrogen molar yield barrier seems reasonable. The knockouts are a good approach to change hydrogen production. The engineering of the microbial electrolysis cell is innovative and complements the genetic work nicely. The reasoning for using IL-derived sugars is not clear.
- Fuel Cell Technologies Office (FCTO) goals for biomass to hydrogen are identified well and addressed by this project.
    - Taking advantage of the National Renewable Energy Laboratory's (NREL's) pretreatment expertise to leverage Bioenergy Technologies Office (BETO) funding is laudable.
    - It is not clear that the necessary increase in solids loading for the DMR stover is achievable without significant advances in reactor and process engineering. Utilization of xylose by *C. thermocellum* is likely to be diauxic, so incorporating this into the already problematic high solids, long retention time fed-batch fermentation will be a large process engineering challenge.
    - It is not clear what the ILs task adds to this project. Increasing the IL tolerance of *C. thermocellum* is interesting but no analysis was presented to say that ILs would be a better pretreatment fit for this process than DMR or acid pretreatment. In fact, given the BioEnergy Science Center's (BESC's) extensive work with acid pretreatment and *C. thermocellum* as well as *C. thermocellum*'s tolerance to inhibitors produced during this pretreatment, it is not clear that DMR is superior to acid pretreatment for this process.

- In Task 3 it is unclear how novel the mutations are completed or how they compare to the work done at BESC on this organism. Overexpression of Hyd2 seems like a productive area of research, and the approach here is good.
- Task 4 seems promising in several aspects, including stainless steel fiber felt (SSFF) replacement. Utilization of the effluent from NREL is a good tie-in process-wise, but the fate of the more problematic compounds here seems potentially difficult (e.g., lignin monomers) but is a good strategy for the organic acids.

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.

- This project has come very close to meeting the hydraulic retention time (HRT) target for hydrogen production. For it to become commercially viable, the concentration of hydrogen will need to improve substantially. This may be accomplished through adding the elimination of ethanol production, which would require further funding to complete.
- Given the FCTO goals, this project seems to be making progress and is meeting milestones, although it seems that a lot of the most difficult steps are proposed for the following years (e.g., increased sugar/solids loading).
- The group has had a number of accomplishments. This is a new project that has not been previously reviewed. It was stated that this work is leveraging previous work. It was unclear which results (if any) presented were from previously funded work. Having reviewed presentations by this research team in the past, there were some findings that seemed similar to past work, although this may have been for a different system.
- The engineering of *C. thermocellum* in order to overcome the hydrogen molar yield barrier seems to be going very well. The knockouts of the lactate and formate pathways seem to be going well and both show an impressive change in hydrogen production. Assuming this scales up well, this mutant could provide a solid supply of biohydrogen. The electrolysis cell has made improvements and appears to be on track and functional. The IL work shows that the cells are fairly intolerant to IL-derived sugars. The reasoning for using these sugars (which are pretty toxic to the bugs) is not clear and perhaps not as good as just using some standard acid pretreated or DMR hydrolysate. Trying to acclimate *C. thermocellum* to this particular hydrolosate, which is not widely used, seems to be a questionable effort in this project considering how low the technology readiness level of IL-sugar is compared to other cellulosic options. Exploring the usefulness of IL as a path to cellulosic sugars is definitely something that needs to be done, but this may not be the project in which to be doing it.
- The slide 3 table indicates a yield of hydrogen that is 29% (2011), 33% (2015), and 50% (2020) of the maximum theoretical yield per glucose equivalent from cellulose. These yields need to be supported by data indicating the time period to obtain this yield. The microbial electrolysis cell (MEC) production rate is given in the slide 3 table in volumetric units of hydrogen and is 1 L/L-reactor-day (2015) and 4 L/L-reactor-day (2020). This should be normalized to the dry weight of biomass used, not the volume of the reactor. The rate needs to be specified if initial rate (day 1) or peak rate (which days) or average rate (which days). The amount of energy input from the electrolyzer needs to be stated and the net energy yield of hydrogen produced needs to be stated. In slide 4, the percentages after treatment do not add up to 100% for pretreated corn stover and DMR treatments—this should be explained.
  - In slide 5, the average hydrogen rate in data shown as 200 mmoles/96 hours for 4 sequential feed cycles (days). It is unclear what portion of each component (there are three listed) in the DMR precursor is converted to hydrogen. It is unclear what portion remains and could be recycled. It is unclear what portion is lost. It is unclear how much cell dry weight is used relative to the DMR weight (5 g/d). It is unclear what the rate of cell replenishment is needed. Figures show that after four sequential feed cycles the rate of hydrogen production falls off significantly—this needs to be explained. The hydrogen production vs. time is not stationary. It is not clear that it is understood what is responsible for the growth and decay kinetics and how this affects future needs.
  - In slide 6, the bar graph shows no error bars and no indication of the number of replicates. These data do not meet the confidence standards of customary scientific evaluation. Medium costs data set refers to a small reactor volume and a single day trial. There is no determination of the medium

costs based on a multi-day trial at a scale applicable to the pilot scale. There is some conflicting information; the MOPS buffer is important for cell pH and fitness, but performance without MOPS results in not heavily diminished (i.e., less than 10% loss) hydrogen production. In slides 7, 8, and 9, which report on IL extraction of untreated corn stover, data is presented in such a way as to mask the results of conversion yields.

- Slide 9 indicates that all three ILs produced insufficient extraction of cellulose components. No clear conclusion was given. This appears to be a lost effort. It is unclear if this project was initiated based on solid theoretical or experimental expectation of high yield.
- In slide 10, task 3, mutagenesis, descriptions of the single gene alterations are missing. Comparisons to known mutant strains in other bacteria are not stated even though these gene knockouts are widely tested in other bacteria and yeast. Growth rates of the mutant cell lines are not given and should be included so the consequences of the genetic change can be assessed. Viability of the mutant cell lines in normal conditions encountered in the bioreactors over the course of useful lifetimes is not given. This section needs more in-depth characterization.
- In slides 11 and 12, relative hydrogen production rates are given for a single hour, but no description of the conditions required to observe this rate or longer periods is provided. Absolute hydrogen production rates are not given. Longer-term measurements and alternative conditions are not given. In slide 14, task 4, it is unclear why the data in the figure have such wide swings in current density. The data are presented without adequate description of the experiment. It is unclear what the rationale was for using a smaller cathode vessel volume. It seems that the rate of hydrogen ion production and subsequent saturation of the cathode with adequate hydrogen ion concentrations for hydrogen evolution reaction (HER) had a very strong impact on evolved hydrogen, but the reasons were not explained. It is unclear if the rate of hydrogen ion production varied due to aerobic/anaerobic, light/dark, or proton gradient conditions. These are important experimental controls that must be applied to give valid data. Non-Nerstian pH dependence and activity increase with alkalinity. System performance appears not to conform with any standard form.
- In slide16, it is unclear in task 4 whether the membrane is polymer electrolyte membrane (PEM) or anion exchange membrane (AEM). It seems it should be PEM. Regardless of which membrane was used, it is unclear whether there has been analysis of the off-gas and the electrolyte from the cathodic reduction of hydrogen ions to hydrogen. It is unclear if there is natural organic matter cross-over from the bio side of the device to the cathodic side of the device, through the membrane or otherwise.
- In slides 13–18, task 4, it seems that performance improvement upon decrease in cell volume goes in the opposite direction than that desired for a commercial process. The overpotential requirements for lower-cost stainless steel electrodes vs. platinum electrodes offsets value of this approach. Data are not explained. Engineering changes have resulted in minor changes in performance and in some cases within experimental errors.

Question 3: Collaboration and coordination with other institutions

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

- The partnerships are effective.
- Coordination is evident between NREL, Sandia National Laboratories, and Lawrence Berkeley National Laboratory.
- There are a number of strong and long-standing collaborations with both domestic and foreign investigators.
- If the IL task is to continue, more discussion between those teams on the feasibility and the direction of the work should occur. It is unclear that if *C. thermocellum* can be engineered to be tolerant to ILs whether the MEC can tolerate those concentrations.

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.

- It does appear that this project would lead to a process in which the hydrogen molar yield is higher than the current standard. The reduction of the growth medium cost is very promising and suggests that this bacteria could be generated cheaply. If this is coupled with an effective MEC, the ability to generate biohydrogen at reduced cost is there.
- The stated aims seem to be well-aligned with the DOE mission. There are a number of advances that have been made which may also be applicable beyond this work. Some questions remain about the long-term stability and viability of the engineered organisms.
- It is not clear how the project will ultimately meet the cost targets.
- Insufficient quantitative results to assess progress towards achieving benchmarks. Unclear timeline to achieve solar-to-hydrogen (STH) goals.
- Generally speaking, making hydrogen from biomass seems like a non-optimal use of the carbon in biomass. The one real advantage of biomass as a feedstock—aside from the fact that it is renewable—is that it contains carbon in organic form, which can be transformed relatively easily into easily transportable liquid fuels and chemicals. Because biomass is highly oxygenated, making relatively reduced hydrocarbon fuels necessitates the input of reducing power or the rejection of carbon dioxide, but this problem is even larger with hydrogen production in which all carbon must be rejected as carbon dioxide. Many technologies are looking at making organic molecules from sunlight, carbon dioxide, or water because of how central these carbon energy carriers are to the U.S. economy. Given that, the state of technology for electrochemical water splitting for hydrogen production, and the massively falling costs of renewable electricity from wind and solar, it is hard to see what role biomass to hydrogen could play in the renewable energy future.

Question 5: Proposed future work

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

- There is a clear path to complete the stated research goals within the time frame allotted.
- Task 1 and Task 2 of the future work do not seem to be synchronized. It is unclear why it is necessary to go back to the drawing board to a different feedstock.
- Techno-economic analysis (TEA) is qualitative, based on early stage technologies, and is highly approximate. Primary data lack error analysis and evidence of replicates. Assessment of near- vs. far-term prospects could benefit from greater quantification.
- Task 1: “Pretreatment” with a pre-made celluosome cocktail takes this project one step further from consolidated bioprocessing (CBP), as it will now include DMR and enzymatic hydrolysis, which is very similar to other proposed biological processes. That said, it will likely be necessary to achieve 175g/L loading, and thus achieve project milestones.
    - Task 2: The potential advantage of ILs over DMR for this process needs to be better articulated for this to be a useful task. Even if it is possible for *C. thermocellum* to grow with 10% [Ch][Glu] it is not clear why this would be the preferred pretreatment method without some sort of TEA or explanation of how this pretreatment is specifically better for *C. thermocellum*.
    - Task 3: This task seems slightly risky and hard to optimize but very worthwhile. Some metabolic modeling may be helpful in the redox balance optimization.
    - Task 4: Proposed work seems reasonable.

Project strengths:

- Mutations of *C. thermocellum* seem promising, and the targeting of hydrogenase 2 is a sensible approach. The engineering team has created an impressive MEC utilizing the engineered biocatalyst. It has the potential to innovate the generation of biohydrogen.

- Project strengths include improved technique to increase sugar production as well as the collection of all sugars in one fraction; deletion of competing pathways to increase hydrogen production; progress towards adaptation of organisms to 10% IL; and reduction in the amount of precious metals needed (use of fibrous felt instead of platinum).

Project weaknesses:

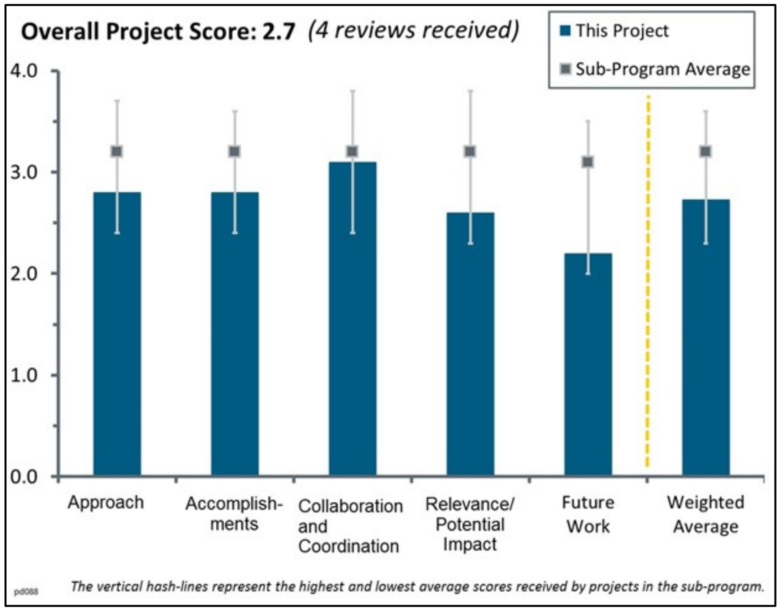
- Weaknesses include the potential long-term stability of the engineered organisms, both due to the deletion of the competing pathways and the over-expression of the hydrogenase 2 system.
- The program should consider if they would like to pursue either IL-derived sugar or more common acid-pretreated or DMR sugars. Task 1 under proposed future work focuses on optimizing *C. thermocellum* to consume DMR corn stover, yet task 2 focuses on adapting it to survive 10% IL. The project may be better served by simply narrowing down the type of sugar the investigators want to use.

Recommendations for additions/deletions to project scope:

- The project team needs to conduct a go/no go and TEA around use of ILs in task 2 very soon.
- The team should seriously question the relevance of working with multiple sugar feedstocks, especially one that needs a lot of optimization.

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 overall objective of this project is to develop and demonstrate the novel steel/concrete composite vessel design and fabrication technology for stationary storage systems of high-pressure hydrogen that meet U.S. Department of Energy (DOE) technical and cost targets. The project team will address the significant safety and cost challenges of the current industry standard steel pressure vessel technology.

Question 1: Approach to performing the work

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

- The approach the researchers took to develop and demonstrate the feasibility of a steel–concrete composite vessel for high-pressure hydrogen that meets DOE technical and cost targets has been well-planned and well-executed. The only concern is whether the choice of steel–concrete composite is the most appropriate approach. While the principal investigator (PI) did explain his viewpoints on utilizing experience from concrete industries, it is not clear that there have been any efforts by the team in assessing other viable options/designs.
- The initial project concept was interesting, considering the potential to truly locate this storage underground, as opposed to an underground room containing conventional storage. However, it is not apparent why a subscale vessel was not considered to demonstrate proof of concept first. It would have been prudent to demonstrate a proof of concept for some of the novel design approaches before making the leap to viability of manufacturing. It would have facilitated the construction of the vessel, allowed the project team to fabricate a number of vessels to address the multiple design challenges, expedited testing, and possibly introduced some additional decision gates to effectively steer the project. The PI admitted during the presentation that concrete may not have been the best choice as a reinforcement material—something that would have been apparent from subscale testing. There are questions about the effectiveness of the multilayered steel and hydrogen vent channel approach to mitigating hydrogen embrittlement (H_2E) effects. There are also questions about the ultimate fatigue life of the vessel, considering the design calls for the use of concrete in tension (not appropriate for concrete). These questions could have all been answered had numerous subscale vessels been available for testing. If the intent of the project team was to use this vessel at hydrogen refueling stations using cascade filling, the industry norm for efficient gaseous delivery, the team missed the need for multiple tanks being located onsite. This point is relevant because it appears the economic feasibility of the vessel makes sense only on a very large scale, and the PI was claiming the objective was for one massive vessel to be located onsite. While the vessel itself could meet the DOE cost reduction targets, as claimed by the PI, any transport, handling, and site preparation costs of using this vessel would negate any savings when compared to existing technology today. These are non-trivial costs that should be factored into the overall vessel cost per kilogram since these are costs unique to this vessel design when compared to existing technology. It is also not clear how H_2E performance will be effectively evaluated. A sound test methodology for H_2E was not proposed in the presentation. The number of pressure cycles conducted seems insufficient to simulate any appreciable hydrogen service timeframe. It is also not clear that any of the accepted H_2E evaluation methodologies could be employed on the metallic elements of the pressure vessel—it is widely believed by the industry that H_2E performance tests must be conducted in

a hydrogen environment to be effective. Any approach that relies on exposure of a material to hydrogen, then removing it from the hydrogen environment for testing, is seen as problematic since hydrogen rapidly escapes the material after removal from the hydrogen environment.

- For the most part, the project was well-designed and well-coordinated. The PI has done a good job of trying to meet the barriers identified by industry and the targets set by DOE. However, it is apparent that this project is going to be labor-intensive and will most likely not meet the manufacturing schedule required to meet the demand for 1000s of hydrogen stations.
- Stainless steel and concrete for this application is not efficient. The only benefit of this design over carbon composite is the cost.

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.

- The project overall seems to have progressed well. There are a few shortcomings—in particular, measuring and quantifying H_2E and fatigue life.
- While the accomplishments in fiscal year 2017 align with the goals of the project team, they do not adequately address several design or practicality concerns with this vessel. It would have been good to see a validation of the fatigue life of the vessel, validation that the multilayer steel walls with “vent channels” was effective in mitigating H_2E, and validation that the vessel was cost-effective compared to today’s available technology. It is not clear that any of these concerns will be addressed by the conclusion of the project.
- It is difficult to discern the level of improvement between this project and the initiation of the new project. DOE should have ended this project or simply combined the efforts prior to awarding very similar efforts.

Question 3: Collaboration and coordination with other institutions

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

- The team comprised a list of individual service providers that had appropriate expertise to accomplish the job, and the project seemed to have adequate coordination between the groups.
- A number of institutions/companies have been actively participating in this project. These contributions have been articulated by the PI.
- Since a large portion of the project appears to center around the novel multilayered steel approach, it was disappointing to see that Sandia National Laboratories—a key member of the effort to establish an international consensus for a H_2E test method—was not a collaborator in this project. A sound plan is needed to evaluate the effectiveness of the hydrogen vent channels.

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.

- This project meets the cost and performance requirements of the Program; however, what is not taken into account is the ability to manufacture and transport these vessels. These structures will most likely require manufacturing onsite, so lowering costs through economies of scale will most likely not come to fruition. The materials are low-cost but highly labor-intensive and not easily manufacturable/transportable.
- This project presents one approach to achieving the DOE goals for high-pressure hydrogen storage. However, still more work is needed to prove the feasibility in terms of fatigue life and H_2E.

Question 5: Proposed future work

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

- This project is scheduled to end in September 2016, and many of the lessons are being incorporated in a follow-on project addressing higher-pressure operation.
- The project is in its final six months. The PI has another project that seems to be tailored toward further enhancement of the design. However, it is not clear what the team plans to do in the remaining time of the current project that does not necessary overlap with the other project.
- This project is ending.

Project strengths:

- Project strengths are the demonstration of the steel–concrete composite vessel and coupling between finite element analysis and tests.

Project weaknesses:

- Project weaknesses are lack of (1) fatigue life and (2) H_2E assessment.
- Novel design aspects addressed in this project do not have a robust evaluation scheme. A number of conclusions were to be expected from the project. However, it does not appear the project will demonstrate the following: vessel demonstrates minimum fatigue resistance using concrete as a reinforcement material; use of hydrogen channels to vent hydrogen demonstrates mitigation of H_2E, thus allowing for high-strength steels to be used; with those proven features, this vessel meets or shows promise in meeting the DOE storage vessel cost target; and vessel demonstrates features are superior to existing technologies currently employed in the industry.

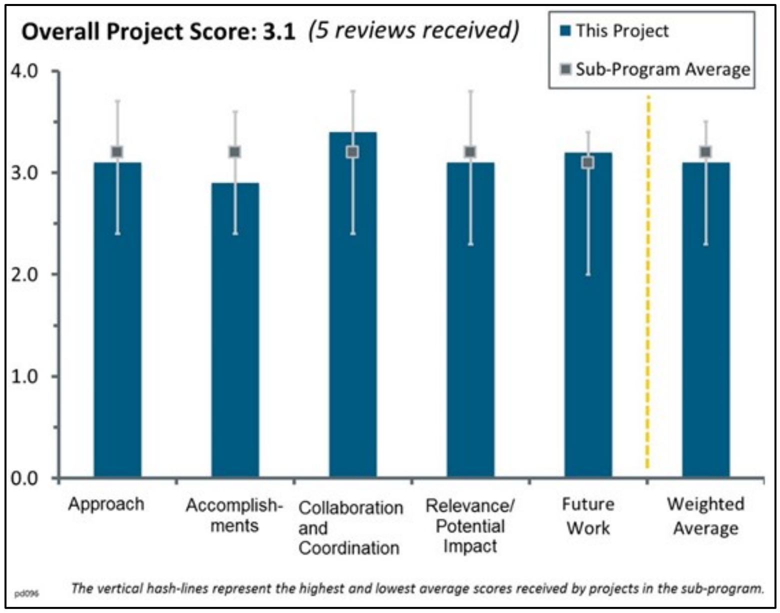
Recommendations for additions/deletions to project scope:

No responses entered.

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

William Summers; Savannah River National Laboratory

Brief Summary of Project:



The objectives of this project are to (1) develop highly efficient process designs for coupling the hybrid sulfur (HyS) thermochemical process with a concentrated solar energy system; and (2) demonstrate SO_2 depolarized electrolysis (SDE) using improved electrocatalysts and high-temperature polymer electrolyte membranes (PEMs) that permit high-efficiency hydrogen production. Fiscal year 2016 objectives are to (1) analyze and select a baseline plant design that utilizes high-temperature solar heating, (2) develop process flowsheet models and calculate plant performance and efficiency, (3) estimate operation and production costs for a commercial plant, and (4) test candidate high-temperature PEMs and demonstrate SDE performance improvements.

Question 1: Approach to performing the work

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

- This project leverages past Hydrogen Fuel Initiative investments in hydrogen production at Savannah River National Laboratory (SRNL) and past and continuing U.S. Department of Energy (DOE) investments in fuel cell component development. The process was originally funded through the DOE Office of Nuclear Energy to develop hydrogen production through a two-step HyS process (an electrolysis step combined with a high-temperature thermochemical step) using heat input from nuclear reactors. The project is building on this past investment, and on DOE-funded development of advanced membranes and catalysts, to address the coupling of the HyS process with a concentrated solar power system. The immediate objectives of the project are to demonstrate the electrolysis step using high-temperature membranes and improved electrocatalysts that permit stable, high-efficiency hydrogen production. It is very important that the project team continue to focus on the high-temperature membrane demonstrations in order to prove viability of a hybrid approach to solar thermochemical hydrogen (STCH) production. Specific barriers limiting progress were addressed, and a reasonable plan was proposed for the remainder of the project and for future work.
- The technical approach taken by the project team to work on the main tasks of this project seems reasonable: focusing on the electrolyzer component development, include polymer electrolyte membrane and electrocatalyst, as well as on the system design so as to have a more accurate cost analysis.
- SRNL has multiple variations on a sound base design. Each variation addresses a potential barrier in a logical and practical manner.
- The HyS cycle is interesting in that it is at a lower temperature than some of the other cycles and does not have the attrition issues that a solid-based system has. The researchers need to clarify many of their Hydrogen Analysis (H2A) tool assumptions, such as electricity costs. Thermal storage at >850°C is difficult, and it is unclear whether the system currently does this. Sand is typically used, and it is good to up to 600°C. The Solar Energy Technologies Office (SETO) is doing work to increase it to 800°C–900°C. It is unclear how the associated costs were included in the H2A analysis.

- The project is addressing key barriers that are relevant to the Hydrogen Production and Delivery program and has already shown, through a techno-economic analysis, a pathway to the goal of $3.7/kg. The biggest problem is the fact that a large proportion of the cost is associated with the solar capital under the assumption that the SunShot Initiative achieves its 2020 goals. This is certainly something that can be considered an external risk.

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.

- Considerable progress has been made in plant, process design, and cost analyses for an STCH production process utilizing Aspen Plus process flowsheet models and the H2A tool. Candidate high-temperature membranes (polybenzimidazole [PBI], sulfonated Diels-Alder poly(phenylene) [SDAPP], and sulfonated perfluorocyclobutyl [S-PFCB]) have been identified, and one membrane (SDAPP3) was shown to meet the go/no-go criteria of a 50 mV improvement at 91°C. The project team has selected baseline designs for the solar thermochemical plant, solar receiver, and sulfuric acid decomposer.
- The project team presented good progress on this work, including the solar HyS process design and the H2A tool for the 2020 design case.
- Generally, all barriers are addressed in this work. The method for thermal energy storage is particularly interesting, as are the methods of heating. Also, the acid decomposition work in the bayonet heater shows promise, as does the identification and potential solutions to sulfide build-up on the cathode. The H2A results appear feasible and accurate. Further detail could be provided regarding how to achieve the high temperatures stored in the sand.
- The results on the membrane electrode assemblies (MEAs) using the Pressurized Button Cell Test Facility (PCBTF) are encouraging and show promise; it would be very interesting to see the results at 130°C. The work done on the falling particle receiver has provided a solution in terms of diurnal operation. The progress of the project is substantial, but it is still very early to say whether it will have a substantial impact on DOE goals.
- Modeling the reactor is a good start. The bayonet reactor design has been around for a long time, so it is surprising that this has not been done before. A 50 mV improvement by increasing the temperature is not a surprise. This does show improvement over the 2014 Nafion data. The projected 150 mV reduction at 130°C seems very unlikely. The project seems very dependent on others for success. The majority of the cost savings are associated with the assumed SETO successes.

Question 3: Collaboration and coordination with other institutions

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

- The project team has benefited from including membrane and solar reactor and receiver experts from Sandia National Laboratories and the University of South Carolina. Industrial partners in the area of membrane development and fabrication would add additional perspective to the project, particularly in the area of fabrication options and costs. The project team continues to have excellent collaboration with modeling experts at SRNL and valuable information exchange with the German Aerospace Center (DLR). Additional industrial participation for development of the electrolysis technology using advanced membranes and catalysts (ideally for both STCH production and other end uses) will be needed to accelerate scale-up of the technology and to fully realize the potential for a two-step hybrid process.
- Collaboration appears to be excellent and includes major research groups and industry. All collaborators seem well suited to their division of the work.
- The collaboration and the expertise among the individuals of the entities working together on this project are very robust.
- There is great cooperation between the partners, who have a good deal of expertise on this subject. Further collaboration with the European project Solar to Hydrogen Hybrid Cycles (SOL2HY2) through DLR could be beneficial.
- Collaborations are very good.

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 directly related to and contributes to the goals of the Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan.
- The project supports the FCTO goal of reducing the ultimate cost of hydrogen production from renewable resources to <$2/kg as well as the objective to verify the competitive potential for STCH production by 2020. Meeting the cost goal will depend both on the successful completion of this project and on technology development beyond the scope of the project and FCTO, e.g., lowering the cost of heliostats.
- This is a renewable-powered water-splitting project for hydrogen production, so it is relevant to FCTO's goals. The potential impact is difficult to assess, as the technology is very early-stage and has many issues to solve. The cycle has the advantage of not needing to move tons of solid materials. However, the project plans to use thermal energy storage, which will require moving large amounts of sand as a thermal storage medium and may negate this advantage. In addition, the high-temperature sulfuric acid will be a materials compatibility challenge.
- This project definitely supports FCTO's efforts to develop a technology that will enable low-cost production of renewable hydrogen.
- There has been substantial progress in the project, but its impact is doubtful. The main reason for this is that the cost is highly dependent on the SunShot project.

Question 5: Proposed future work

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

- The proposed future work is highly relevant and will provide useful results.
- The proposed demonstration of SDE performance using high-temperature MEAs seems reasonable for this project.
- The proposed work for continued research and analysis is reasonable. Particular attention should be focused on detailed cost analysis and on demonstrating the performance of high-temperature membranes in order to show that a hybrid STCH cycle can compete with other solar hydrogen production methods. H2A results should be compared to those for high-temperature STCH and updated analyses for photovoltaic electrolysis and solid oxide electrolyzer cell electrolysis.
- All of the plans for future work can directly address the remaining barriers for this system. Some comparison work for the various methods of heating and storage (sand, helium, etc.) would be useful.
- It is important to test the long-term stability of the sulfonated PBI (s-PBI) membrane. This needs to be done under real conditions. The project needs to look at cycling as well as constant current.

Project strengths:

- The thermal storage design is promising, and it is encouraging to see that the system can be used even during non-sunlight hours. The testing plans, pinch point analysis, new MEA material, and bayonet design modeling contribute strongly to this work.
- The project has an experienced and skilled project team with historical knowledge of the area and expertise in the technologies critical to success. The project is leveraging past work funded through the Hydrogen Fuel Initiative. Progress has been made in system design and analysis and in investigations and consideration of designs and innovations for membrane materials, solar receivers and reactors, and sulfuric acid decomposition.
- This is the one cycle that does not require movement of tons of solid materials. This is a huge advantage.
- There is excellent expertise from the partners, and testing facilities are good.

Project weaknesses:

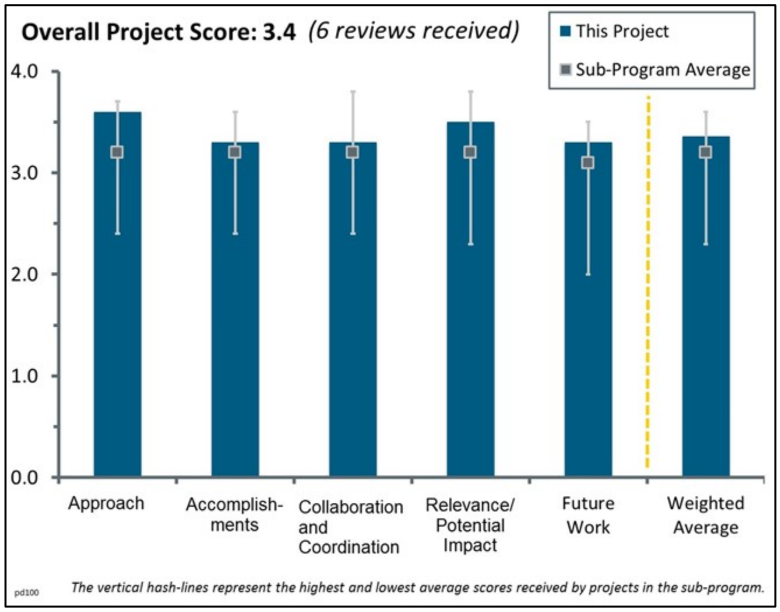
- The project presentation claims that a clear path to $2/kg hydrogen was defined. However, significantly more detail is needed regarding assumptions made and specific process and capital cost improvements that will lead to the cost goal; otherwise, the H2A analysis showing that the cost target can be met is not convincing. (Reviewers must rely on information presented, not more detailed reports or analyses that are not part of the Hydrogen and Fuel Cells Program Annual Merit Review.) It is concerning that the PBCTF still cannot achieve the desired test conditions. This needs to be resolved as soon as possible.
- The H2A analysis was nondescript in critical components. While there are many parameters that contribute to an H2A analysis, some key parameters (total capital costs, installation factors, etc.) should be listed. Steps in the waterfall chart should indicate directly what changes lower the total cost.
- The cycle requires extremely high-temperature sulfuric acid, which will cause materials compatibility challenges.
- The dependency on the SunShot Initiative's success is significant.

Recommendations for additions/deletions to project scope:

- The project team has done a good job analyzing options and approaches for the whole hybrid HyS thermochemical cycle, and has identified challenges to be addressed in future work. Time and funding limitations, however, demand that research tasks be prioritized going forward. It is crucial that the project team give the highest priority to demonstrating long-term performance and durability of s-PBI membranes as soon as possible, including SDAPP and other high-temperature membranes if time/resources permit. Advanced catalysts, optimization of the acid decomposition step and the falling particle receiver, and design and operation of an integrated system are important but will not be needed if sulfur crossover and deposition is not eliminated and an appropriate low-voltage, high-temperature membrane developed.
- In next year's presentation, it would be very good to show the progress achieved by the SunShot Initiative to see if it is reasonable to assume that the 2020 targets will be achieved.

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, which 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.

- This is a great project to provide accelerated cycle-life testing for a key component (dispenser hose) that has few vendors, high failure rates, low mean time between failures, and weak technical standards. The project provides support in all areas of deficiency.
- The project is well-designed to address most barriers, especially the critical ones.
- The project has thought through the test setup carefully.
- This is interesting work. The approach seems appropriate.
- The approach is good. The system is nicely automated, programmed to replicate human motion, and providing 24/7 operation. While the principal investigator (PI) has more than one hose, all are from the same manufacturer; to understand whether there is a systemic problem among different hoses and with the technology in and of itself, similar hoses from different manufacturers need to be tested. The PI did mention that, after this project is over, the facility will be available to test other components on request, not just hoses. That is good, but as part of this project, hoses from different manufacturers should be tested to look for systemic problems.
- The approach is technically sound. The tolerances on the robot making and breaking the nozzle receptacle connection result in tighter alignment than encountered in the real world. It would have been nice if the alignment tolerances were looser to see whether the current receptacle design properly addressed the brinelling issues previously seen on the N25 and N35 nozzles in natural gas service.

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.

- It is good to see that hydrogen supply limitations have been addressed at NREL and that laboratory safety systems are now in place to allow around-the-clock cycle testing.
- The project has demonstrated many accomplishments in terms of identifying the leakage of hydrogen before, during, and after the system has been depressurized.
- Progress is good so far.

- The progress to date is interesting. It is not clear whether the magnitude of the leaks has been determined and, if so, what the sizes of the Class 1 Zone 1 and Zone 2 volumes are.
- Recognizing that these experimental campaigns take a long time to execute, it still seems that the quantity of output is low, the number of publications is weak, and the location of the publications is weak. Presenting to a technology team and producing a YouTube video do not count. This work needs to be published in a refereed journal or refereed conference/symposium or suitable trade journals. A comprehensive general-distribution NREL report would be acceptable as a precursor to a refereed article. This project has been funded since 2013, and after three years, the team should have more to show. This work is useless unless it gets put in the public domain where it is readily available.
- The presentation is not very clear about what the main results of the work have been up to this point. It is not clear what the consequences are or how the results are fed back to industry.

Question 3: Collaboration and coordination with other institutions

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

- The project is designed in a way that allows a well-coordinated and logical interaction with other collaborators who are considered experts in the field.
- The collaborators are okay (except that NREL is not a collaborator), although a broader set of samples from other manufacturers is needed to understand any inherent systemic problems. With only one sample, no systemic problems can be identified. The testing and results are valid for only the one.
- This type of work is usually done by a commercial laboratory or a Nationally Recognized Testing Laboratory (NRTL). The fact that no NRTL is involved is surprising, as is the fact that the breakaway manufacturer and the nozzle manufacturer are not on the team.
- It seems that the industry partners provided only samples. It is not clear that a review process has been set up and, if so, that it influences the industry partners' research, development, and demonstration strategy.

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.

- Delivering hydrogen fuel to the end users is an important task and critical to the success of the Hydrogen and Fuel Cells Program. This project has a potential impact on the program, as it is well-designed to aim sharply at effectively improving the reliability of hydrogen dispenser hose under high-pressure operating conditions.
- The relevance of this work is excellent. Reliability of stations is a real Achilles heel for the fueling industry. This work needs to expand the hardware being studied so there is an "industry-wide" relevance.
- Understanding these leak behaviors is interesting and should be continued as new hose materials are introduced to the market.
- The hose accelerated cycle testing has potential for significant impact on current infrastructure problems with poor hose cycle life.
- This activity is highly relevant.
- No explicit benchmark to DOE targets was mentioned. Nonetheless, station reliability is a major issue. The project provides a useful independent test facility for hoses and receptacles.

Question 5: Proposed future work

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

- Increasing the number of cycles is critically important. Reliability at the station is probably the most important factor in customer acceptance as hydrogen technologies are deployed. A fully functional station should see on the order of 100 fuelings per day; over a year, a dispenser may see 36,000 fuelings. The service interval of only six months is way too short to be cost-effective. This technology needs to be made more robust. The future proposed work is headed in the right direction. However, hardware from other manufacturers must be included in this work, as it becomes available, to understand industry-wide systemic problems. Also, this work needs to include contaminants leaching into the fuel supply with cycling and age. We are finding that fuel quality control is a real challenge.

- The proposed work seems appropriate. It would be a comfort to learn that a NRTL, a breakaway manufacturer, and a nozzle manufacturer were included in this effort. Additionally, it would be good to hear that the tooling designs are available for the NRTLs to copy if they so choose.
- The proposed future work is logical and technically sound. However, it is lacking the risk mitigation plan for each decision point.
- The future work makes sense, although it does not seem systematically planned. It is not clear how the results will be shared with industry, not is it evident who is driving this process.
- Future work is pretty clear.

Project strengths:

- The project is technically well-designed and logically planned; it is well-coordinated with other collaborators; and it has been carried out in a timely manner, is on schedule, and is making good progress.
- It is very helpful to have 24-hour-per-day cycle testing during this time of hose standard development at the International Organization for Standardization (and thereafter at the CSA Group).
- The PI is talented and hard-working. There is good automation in the laboratory.
- The industry need and the laboratory’s expertise are project strengths.
- The project presents a useful independent test facility for hose/receptacle testing.
- Early findings are interesting, and the reviewer looks forward to more understanding of key mechanisms/patterns as the work proceeds.

Project weaknesses:

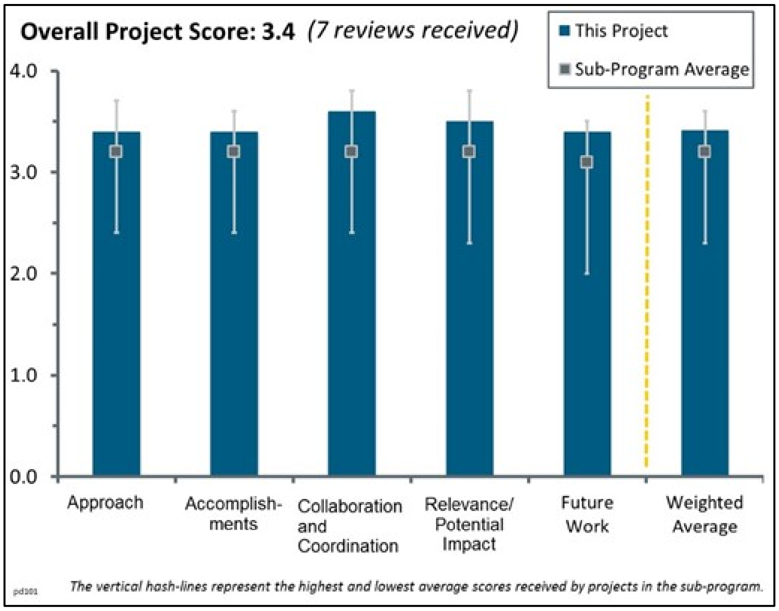
- This project is lacking a risk assessment plan, which is an important factor to consider. The risk assessment plan is necessary to allow the project to mitigate risks related to safety, reliability, cost and performance effectiveness, system limitations, and project schedule (i.e., project downtime).
- A NRTL, a breakaway manufacturer, and a nozzle manufacturer were not included in this effort. They would lend credibility and industry acceptance of the results.
- Perhaps industry is not sufficiently involved.
- The project needs to get additional hose collaborators and/or hoses for testing.

Recommendations for additions/deletions to project scope:

- A NRTL, a breakaway manufacturer, and a nozzle manufacturer should be included in this effort. The project should consider making the tooling designs available to the public for copying. The project should also consider amending the test plan to generate meaningful data to address other issues such as brinelling concurrently.
- The project should hold supplier workshops and involve the big station technology providers such as The Linde Group, Air Liquide, and Air Products.
- The team could work with more hose manufacturers and could add a second robot station to work on two hoses at the same time.
- The project needs to get additional hose collaborators and/or hoses for testing.
- It is recommended that a risk assessment plan be added to the project.

Project #PD-101: Cryogenically Flexible, Low-Permeability Hydrogen Delivery Hose

Jennifer Lalli; NanoSonic, Inc.



Brief Summary of Project:

The objectives of this project are to (1) develop a flexible dispensing hose to enable hydrogen delivery at <$2 per gasoline gallon equivalent; (2) demonstrate reliability at 50°C and 875 bar for H70 service; and (3) optimize ruggedness, cost, and safety for 70 fills per day and over two years.

Question 1: Approach to performing the work

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

- The team members clearly know their polymer science and have produced and are producing very attractive hose products. Their Achilles heel has been in fastening the ends to their hoses (crimping problems). This has been an issue for a long time and has been noted and articulated to this group in several review venues. It was great to see that the project was successful with at least one hose and end going to 60,000 psi without separation, which is outstanding. It seems certain that the project will be successful in designing an end that will be rated to the appropriate pressures; this is good work. The team members need to pay a bit better attention to the contaminant issue—100 ppm is a big number. While they have the appropriate standards (J2719 and ISO 14687-2), they did not seem to understand these standards in detail. They need to pay better attention to those standards—and unfortunately for them, the standards are undergoing revision, so the current tolerance numbers will change. The team also needs to pay attention to the species that might contaminate the fuel and any particulates that might flake off.
- The result of this project will be to have a new, qualified supplier of hoses for hydrogen station implementation. This is a good example of a funding opportunity being used to benefit a technology development while also serving the broader industry by introducing a new supplier into the mix. The awardee has done a good job of addressing the primary concerns from the previous year, expanded the scope to include fittings, and with this has demonstrated some good progress.
- This project addresses important barriers and contributes significantly to the improved technology and economic cost challenges of the component, which is an enabler for the hydrogen infrastructure.
- The approach is sound. It is unclear as to when material compatibility with polymer electrolyte membrane fuel cells (PEMFCs) is to be conducted. Considering this has the potential to be a showstopper, sooner would be better.
- The approach seems logical. Having to change the project a bit in regard to the terminations/fittings is interesting but understandable.
- The project is well designed to overcome the barriers.
- This is a really good project with potentially great promise that has suffered from lack of partners or collaborators with prior experience in the hose industry.

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 accomplishments are excellent, and the recognition and the reporting of the remaining challenges are even better. This "eyes-wide-open" approach pays dividends on the chances of success.
- This project has made great progress since the last time the reviewer saw a report (only a few months ago). The success in crimping the end to the hose is recent and very significant—the project has done a good job.
- The scope expansion to include fittings is significant, and the awardee has done a very good job of taking on this scope while continuing to deliver on the overall project. This is not necessarily something to which other awardees would have agreed.
- Good progress has been made in down-selecting the final hose materials with the best performance.
- The team has continued to make progress, and the results have great promise.
- The project seems on track. Hose costs are not a major issue, but something cheap and very durable would be helpful.
- Much progress has been booked so far, and already it can be stated that the project is a success. However, considering the work still to be done (for example, testing and qualification) with an end date of 7/27/2016, it is not credible that the project can be completed in all aspects without an extension.

Question 3: Collaboration and coordination with other institutions

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

- This is a good team/collaboration. During the question-and-answer session, the principal investigator was asked about Lillbacka participation in this project. Indeed, the involvement is that NanoSonic purchased the now in-house crimping tool. That is the extent of the involvement—no consulting, no collaboration. The principal investigator explained that Lillbacka referred to crimping as "black magic," and NanoSonic wanted/needed to make the hose entirely in-house. This explains a bit why NanoSonic was not more aggressive in seeking consulting advice from Lillbacka or other crimping companies. In the end, the project team was successful. It seems the team will be able to continue to refine and continue with successfully assembling the entire hose, with metal ends on NanoSonic's polymer hose.
- The awardee has formed a strong team for the evaluation of the hose performance. National Renewable Energy Laboratory (NREL) testing will be an excellent next step to validating the performance. It is unfortunate that the fitting manufacturers have not been more cooperative, as it seems that this has caused considerable problems for the project team. In spite of this, the project team seems to have taken on this scope and is producing good results.
- The project is designed in such a way that it allows a well-coordinated and logical interaction with other collaborators.
- The selection of partners is outstanding.
- There is nice collaboration between industry and the national laboratory.
- Sound interfaces are present with testing laboratories and industries.
- This is a really good project with potentially great promise that has suffered from lack of experience in the hose industry.

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.

- Recognizing that delivering hydrogen fuel to the end users is an important task and one critical to the success of the Hydrogen and Fuel Cells Program (the Program), this project has demonstrated that it aligns well with the Program and DOE research, development, and demonstration objectives. The project has the

potential to advance progress toward improving the performance of hydrogen dispenser hose under high-pressure and low-temperature operating conditions.
- Developing a delivery hose that can withstand 25,000 to 30,000 H70 fills without failure would go a long way toward enabling a robust reliable hydrogen fueling infrastructure. Also, this technology has the potential to cut the costs of these delivery systems significantly.
- Being constrained to a single supplier for high-pressure hydrogen hoses has been a continual problem for station designers. While the potential cost impact is small when compared to overall station cost, this will benefit the market.
- The lack of a reliable, cost-effective fueling hose is making an adverse impact on the industry. This effort has the potential to remove this obstruction.
- This project can deliver a strong contribution to the technical and economic progress of a critical component of the hydrogen infrastructure.
- This is not a major issue, as hose technology seems pretty good, but it is worth working on to be sure.

Question 5: Proposed future work

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

- There is a clear plan for future work, which looks good.
- The proposed future work is logical for the stage of deployment of new hose materials.
- The proposed future work is rational.
- The reviewer looks forward to the next steps in testing. The test program could include a side-by-side comparison with the existing hose manufacturer whenever possible. Performance data are very difficult to obtain, and having side-by-side comparisons would benefit both the future users and the awardee with insights into strengths and weaknesses in each design. Testing for leaching/contamination seems weak. It would be good to see a more thorough understanding of the testing and specifications that need to be met in this area—in particular, if there is a test standard or an acceptance criterion.
- The project is addressing the crimping problem in the future work proposal, which is good. The project is addressing fuel quality, which is also good; there has been an expressed concern about whether these hoses will evolve contaminants into the fuel, so fuel quality testing, with use and in time, is critical. However, it was not clear that any attention was given to particulates. It is conceivable that, with time and use, this material might fatigue and give off particles into the fuel supply. Micron and sub-micron particulate sampling really needs to be done.

Project strengths:

- This team is very strong with the polymer science, the facilities are good, and the team is clearly able to experiment with different hose configurations. In so doing, the project is clearly making progress in developing a hose with the desired properties. This is excellent.
- Hose cycle life in H70 dispensers is a tough duty cycle, and long life is essential. This project is proposing new hose liner technology that warrants continued support.
- The team has shown good adaptability and has done a very good job of addressing the concerns raised in 2015 with regard to the fittings.
- The following are project strengths: the project has coordinated well with other collaborators, has been carried out in a timely manner, is on schedule, and is making good progress.
- Project strengths include the "eyes-wide-open" approach and the willingness to find partners to help.
- The project has demonstrated that it possesses the required excellent technical and management competences to overcome the many challenges found in its path.
- This is an interesting investigation of new hose materials/manufacturing—and "manufacturability" should be a key element on which to focus.

Project weaknesses:

- The lack of a solution for the metal fitting has probably caused delays. The problem seems to have been overcome now, and a full component will be available for testing.

- The presentation needs to show/explain in more detail how to mitigate the difficulty of crimping the Ceramer coupling to the hose for high-pressure application.
- The team is new to making hose assemblies and has a steep learning curve.
- Weaknesses include the delay in addressing potential showstoppers such as incompatibility issues with PEMFCs.
- Better attention to fuel quality and the relevant standard specifications needs to be paid.

Recommendations for additions/deletions to project scope:

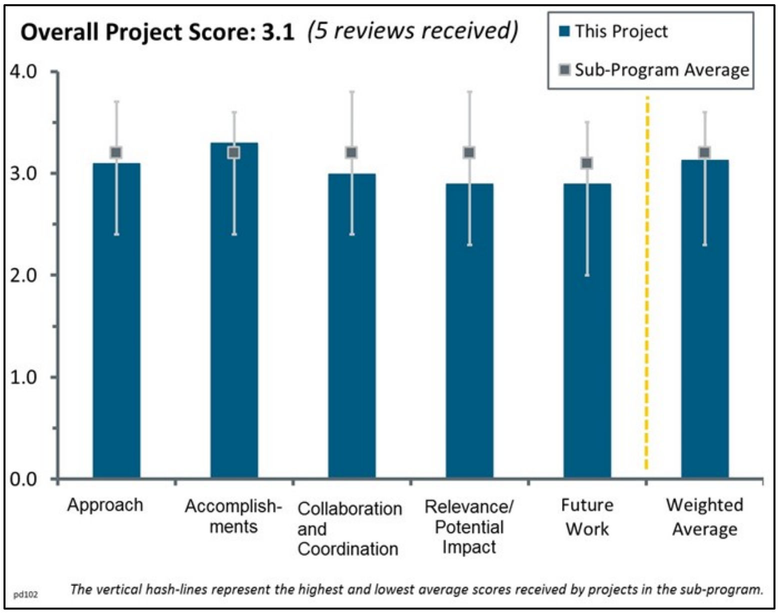
- The project is now on the right track. It is hoped they continue to have success with what used to be a serious crimping problem.
- The project should continue per its plan and determine what compounds may leach out and/or off-gas from the liner materials, and at what rate.
- The project is a very good one. However, its presentation does not clearly explain/identify the essential effort offered by this project to improve the hydrogen fuel-delivering system. It is suggested that the objectives or project scope be clearly defined and presented in such a way that the contribution of this effort can be recognizable.
- Side-by-side comparisons with the current industry standard should be made. An industry benchmark data set would be valuable for this product and for any further developments in this area.
- The project needs more funding and more partners.
- Given somewhat of a work scope change, it is not clear what the company’s plan is, i.e., whether to produce complete systems (with end terminations) or just the raw hose material. Since leaks seem associated with the fitting/crimping sections (based on other NREL research), it is not clear how we best address this.

Project #PD-102: Analysis of Advanced Hydrogen Production Pathways

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:

The objectives of this project are to (1) analyze hydrogen production and delivery (P&D) pathways to determine economical, environmentally benign, and societally feasible paths for the P&D of hydrogen fuel for fuel cell electric vehicles; (2) identify key bottlenecks to the success of these pathways, primary cost drivers, and remaining research and development (R&D) challenges; (3) assess technical progress, benefits and limitations, levelized hydrogen costs, and potential to U.S. Department of Energy (DOE) P&D cost goals of <$4 per kilogram hydrogen by 2020; (4) provide analyses that assist DOE in setting research priorities; and (5) apply the Hydrogen Analysis (H2A) model as the primary tool for projection of levelized hydrogen costs and cost sensitivities.



Question 1: Approach to performing the work

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

- Strategic Analysis, Inc., (SA) and its partner national laboratories have taken a clear and detailed approach to modeling hydrogen production. The project has done a good job predicting costs for very early-stage technologies. Projections of pioneer and nth plants are a valuable consideration in evaluating technologies.
- This is a well-established approach that provides a degree of "consistency" across many different technology platforms and feedstocks (biomass, electricity, natural gas, etc.) for estimating the cost of producing hydrogen. Given that many of these technologies are far from market-ready, it is not clear how accurate the cost estimates for "large-scale" production are.
- The assessment seems technically sound, but some of the key underlying assumptions are overly aggressive and have not been properly vetted. In addition to "today at production volume" and "future at production volume," it would be good to see the "fabricated today at today's volume" case to understand where the technology is, followed by an explanation of how learning by doing and higher production volume will reduce cost. Regarding "fermentation results, future case," the assumptions of broth density seem very aggressive given what has been achieved in the laboratory so far. Given that the corn stover concentration is based on DOE Bioenergy Technologies Office (BETO) goals, it would be material to vet this assumption with academia and industry and assess the likelihood of achieving this concentration level. Regarding solid oxide electrolyzer cell (SOEC) results, the current central case is based on assumptions of technology at scale. It is misleading to call this case "current" given that the data do not reflect current technology costs. Rather, they are based on assumptions of future potential volumes of production.
- The main sources for technical information have been research organizations rather than industry; the reasons for this are unclear. How the analyzed pathways are selected is not very transparent.
- It is not clear why the nuclear coupled thermochemical process is not selected for a review.

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.

- Completion of the SOEC (including degradation) and dark fermentation cases is a significant accomplishment. For the dark fermentation case, the high density is a big leap from current technology. Perhaps an intermediate case should also be considered. Bio-oil reforming should move quickly away from fatty acids to real bio-oils.
- The project has made excellent progress toward meeting project objectives for technologies being evaluated as defined in the presentation. It is difficult to assess how well the project has made progress toward meeting DOE goals since the results and outcomes of this project are used by DOE to define its R&D priorities and performance/cost targets. Therefore, it is difficult to assess how well the project has progressed toward meeting DOE goals, particularly from the perspective of how much impact it has on the R&D community; however, the significance and importance of this project to DOE are fully recognized.
- The project has made excellent progress toward identifying cost drivers, assessing Technology Readiness Levels (TRLs), and integrating results into the H2A model. It is unclear whether the team has already identified all technical and economic bottlenecks.
- The results seem mature and helpful for further Fuel Cell Technologies Office (FCTO) work.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- SA works to gain input from all stakeholders. There is good collaboration between SA and national laboratories.
- BETO is pursuing numerous thermal chemical and bio-based routes for converting biomass to biofuels and the idea that many of these technologies could be modified to produce hydrogen as an end product. Given these facts, perhaps it would be beneficial to evaluate a broader range of biomass-based processes for producing hydrogen than what could be called the “niche” projects being evaluated. It would provide a broader perspective of the cost competitiveness of biomass-based technologies for producing hydrogen at the projected DOE target.
- The project should ensure that assumptions are harmonized with Argonne National Laboratory’s (ANL’s) analysis work since ANL is working on the same cases to assess the lifecycle footprint of the technologies. Some of the assumptions appear different, such as the stack replacement schedule of the SOEC and the process flow of the fermentation pathway. Perhaps researchers in academia are working on similar technologies. It would be good to get their perspective on the TRL of the technologies and to compare results from this work against published literature.
- It is not clear whether there are any industry collaborations other than the one with FuelCell Energy, Inc., as no others are mentioned. It seems the analysis would not be possible without industry input. Perhaps there is a review process of the results with industry in place.

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 major benefit to DOE and the R&D community at large from this project is identifying key bottlenecks to reducing cost, which is critical for DOE and companies in setting R&D priorities.
- These results are vital for defining appropriate areas for future research.
- Analysis results could help FCTO to focus on pathways with high potential.
- It is necessary to understand the status and the potential of hydrogen production pathways but the assumptions for future progress appear highly optimistic, which makes these pathways less relevant. At this

point, given the low TRL of these technologies, these cases are not very relevant to industry, but they can be relevant to guiding DOE funding decisions if the right assumptions are made.
- It is not clear what impact this study will have on the production pathway. DOE's selection criteria are also unclear.

Question 5: Proposed future work

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

- The analysis of the FuelCell Energy work is appropriate and should be interesting.
- It is somewhat difficult to evaluate the proposed future work since the future work presented focuses solely on cost projections for FuelCell Energy's Reformer-Electrolyzer-Purifier (REP) technology. During the presentation, it was stated that SA had been awarded a new contract, so presumably there is a broader scope of work going forth, but since that work will be conducted under a new contract, future work outside of the scope of this contract is not being considered in this review.
- Molten carbonate fuel cell technology being used in the REP system is still at an early stage of R&D but shows significant promise for utility-scale operations.
- It is not clear whether any other pathways are planned or how the analyzed pathways are selected.

Project strengths:

- The project is highly relevant to future FCTO priorities in research, development, and demonstration (RD&D).
- The project team has a good understanding of processes and excellent analytical capabilities.
- There is a strong, experienced team with a proven track record.
- The project builds on proven expertise and experience.

Project weaknesses:

- It is not clear why the projected cost of hydrogen produced by solid oxide electrolysis increased from \$4.21/kg in 2015 to \$4.95/kg in 2016 for the Current Central case and from \$3.68/kg in 2015 to \$3.83/kg in 2016 for the Future Central Case. The electricity costs seem to be the same for both the 2015 and 2016 analyses. With a drop in the price of crude oil, one would assume that there would be a slight decrease in price. The factors that are leading to these cost increases are unclear. The future cost projects are based on high-volume production. It is not clear what constitutes high-volume production, either in terms of the total amount of hydrogen produced across the United States or the number of plants of a given technology required to produce the projected volume demand.
- Data and information appear to be sourced from single sources but have not been validated through collaborations with other researchers working in the same or similar fields. Technologies are assumed to operate at scale when none of their TRLs are higher than 5. Assumptions appear to be overly optimistic.
- A project weakness is lack of experimental data for emerging systems.
- Industry collaborations are a weakness.

Recommendations for additions/deletions to project scope:

- The project should ensure that assumptions are harmonized with ANL's analysis since the project team is working on the same cases to assess the lifecycle footprint of the technologies. Some of the assumptions appear to be different, such as the stack replacement schedule of the SOEC and the process flow of the fermentation pathway. Perhaps researchers in academia are working on similar technologies. It would be good to get their perspective on the TRL of the technologies and compare results from this work against published literature. In addition to the "today at production volume" and "future at production volume," it would be good to see the "fabricated today at today's volume" case to understand where the technology is and then explain how learning by doing and higher production volume will reduce cost. Regarding "fermentation results, future case," the assumptions of broth density seem very aggressive given what has been achieved in the laboratory so far. Given that the corn stover concentration is based on BETO goals, it would be material to vet this assumption with academia and industry and assess the likelihood of achieving this concentration level. Regarding SOEC results, the current central case is based on assumptions of technology at scale. It is misleading to call this case “current” given that the data do not reflect current technology costs. Rather, they are based on assumptions of future potential volumes of production.

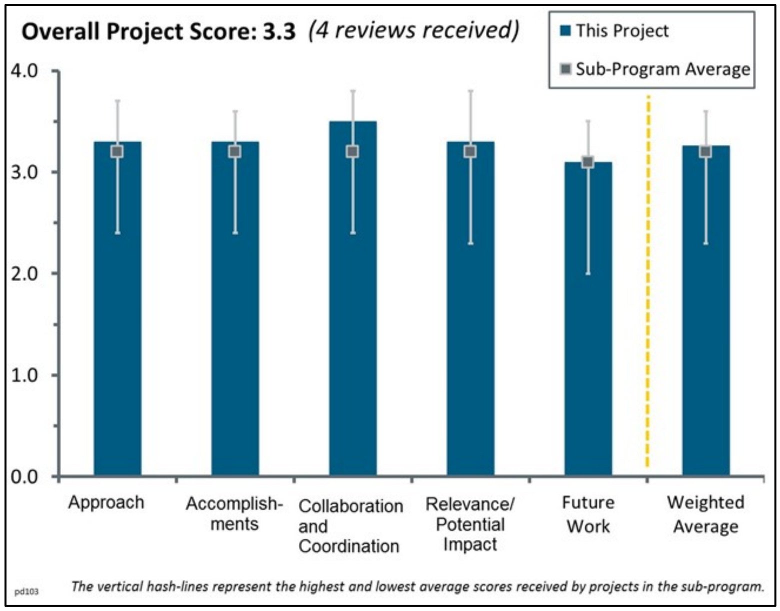
- “Future” is a somewhat ambiguous term since it could be 5 or 25 years or more before the technology is implemented at the projected scale. Given that future cost projections are based primarily on DOE R&D targets, it would be good to know at what point DOE goes back and reevaluates the current cost projections of the technologies that have been considered under this project to see whether the predicted cost reductions are on target or have been met and to determine whether there are different cost drivers that would require refocusing R&D priorities.
- Industry workshops to derive RD&D projects with industry based on the results would add value to the project.

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 (1) scale up and commercialize low-platinum-group-loading oxygen evolution reaction (OER) catalysts using the Giner, Inc., (Giner) polymer electrolyte membrane (PEM) electrolyzer platform and (2) evaluate the impact of newly developed catalysts on PEM electrolyzer efficiency and cost.



Question 1: Approach to performing the work

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

- The project is well designed and feasible. It is integrated with other efforts, including using Giner and National Renewable Energy Laboratory (NREL) standardized protocols for electrochemically active surface area (ECSA) with Hg underpotential deposition (UPD) on Ir vs. IrO_x, and transmission electron microscopy (TEM) at Oak Ridge National Laboratory (ORNL) before and after evaluation under harsh conditions.
- The project is using a good approach to replace expensive platinum-group-metal (PGM) catalysts with less-expensive catalysts with equivalent performance. The project should do a techno-economic analysis (Hydrogen Analysis [H2A] model) to determine the cost savings the project technology will achieve.
- The focus on durability test protocols is particularly encouraging.
- The project addresses the barriers of electrolyzer cost by lowering PGM loading. The project addresses the electrolyzer performance barrier and high anode overpotential. It is not clear what the strategy is to mitigate Ir dissolution and migration.

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 nanostructured thin film (NSTF) and Giner anodes demonstrated superior performance compared to the standard baseline anode. The Giner catalyst had comparable performance to the baseline with one-fourth the PGM loading. Giner and 3M successfully scaled up catalyst production. The project identified Ir migration from anode to cathode as a degradation path and potential shorting mechanism. The team has developed an Hg UPD protocol for determining surface area on oxides and metals.
- This project demonstrated progress against Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan (MYRDDP) performance indicators where the performance of a small batch of Ir/W_xTiO_{1-x} OER electrocatalysts was inferior to a large, scaled-up batch. This activity difference was due to Ir having fallen off after voltage cycles and was found more on the membrane and no longer on the carbon support and/or was aggregated in some other measurements. Moreover, the activity of the scaled-up catalyst, while excellent, was not stable on the two-days-and-longer timescale. This, however, was not a problem because another batch, synthesized in March (only discussed during the presentation, i.e., no slides), exhibited excellent performance and was stable by rotating disk electrode (RDE). However,

the project should be cautioned about use of certain materials as standards when the electrodes are fabricated by Giner and not purchased as full membrane electrode assemblies (MEAs) because the performance may suffer (e.g., Johnson Matthey Ir Black). Along this line, the reproducibility of the materials and electrodes seems poor. Also, hydrogen crossover was small when mitigated membrane decals were used. During the presentation, it was mentioned that F content in solution was measured using inductively coupled plasma (ICP) to assess stability, but the data and results were not included in the report or talk; they should be included next time.

- The materials showed good initial performance, but the durability tests show that improvement is still needed. The development of standard testing protocols was very interesting. It is not clear how the researchers will engage the electrolyzer community to try to get acceptance of their protocols. The accelerated testing is interesting but needs to be validated against real-life data.
- It would be helpful to better understand the "mitigation" process and its impact on the 3M membrane since that appears to be critical to the membrane's hydrogen crossover performance. Perhaps there is something general to be learned here.

Question 3: Collaboration and coordination with other institutions

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

- ORNL analysis has been able to identify Ir migration from anode to cathode. All partners are involved with the work, with 3M and Giner supplying catalysts, ORNL performing microscopy, and NREL studying MEA degradation. A method was developed to measure ECSA, but the correlations between SEM/TEM microscopy, measured surface areas, and performance need further investigation.
- There is a healthy "competition" between Giner and 3M for the best lead catalyst concept, and the project is leveraging expertise at national laboratories. Microscopy done at ORNL was critical to understanding aging behavior.
- The work is nicely collaborative and involves interaction with NREL, 3M, and ORNL.
- There is a strong team that has well-defined roles.

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 exhibits progress toward MYRDDP targets where Phase IIB funding (Small Business Innovation Research program) resulted in two OER catalysts, Ir/W_xTiO_{1-x} and Ir-NSTF, with much lower PGM loading, although arguably Ir may be more costly and resource constrained than Pt.
- The work is relevant and should lead to decreased PEM electrolyzer costs through lowered catalyst costs. Lowered electrolyzer cost can have an impact on hydrogen cost, especially in situations such as using stranded renewables where the electricity cost is very low.
- It is appropriate for DOE to support continuous incremental improvements in electrolysis through both fundamental and applied research.
- Low-temperature water splitting for hydrogen generation is very relevant to the FCTO. The development of the testing protocols is needed. How the project will get the protocols accepted is not clear. Getting stakeholder acceptance is beyond the scope of this work. Without a techno-economic analysis (H2A), the potential impact of the work is not clear. The project needs to do the H2A to understand the potential impact of the work.

Question 5: Proposed future work

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

- The future planned work is logical, including placing the developed electrocatalysts in a sub-megawatt stack, although “sub-megawatt” should be more precisely defined. It is less clear if there are decision points. It does not seem so based on the milestone and delivery chart.
- It is not clear what the strategy is to mitigate Ir dissolution and migration.

Project strengths:

- Giner is a leader in the electrolysis field. Other strengths include attempts to develop accelerated stress tests for electrolysis and a method to measure ECSA for metals and oxides.
- Project strengths include the ability to do scale-up and testing with a clear path to commercialization of promising leads.
- This is a strong team working on interesting problems.
- This is an interesting project and idea.

Project weaknesses:

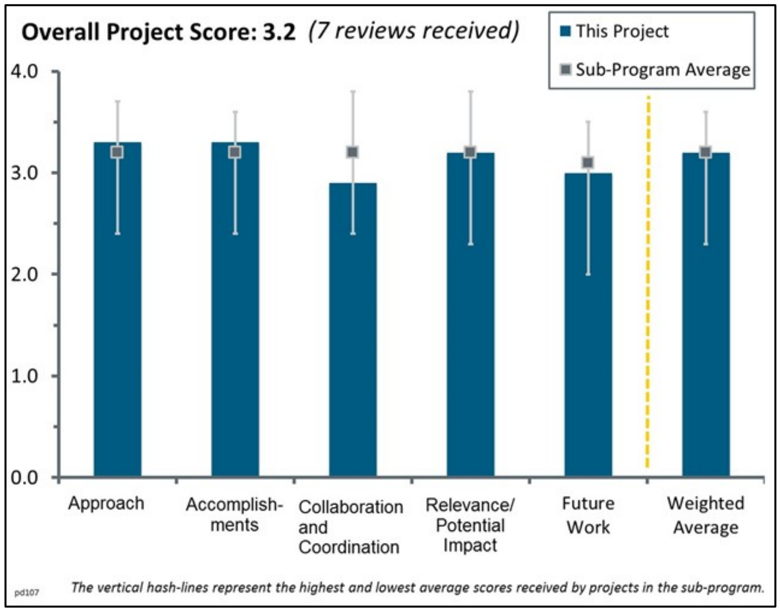
- The researchers need to work on the durability of their catalyst. They need to validate the accelerated testing protocols. They should look to the PEM fuel cell protocols for any additional durability tests. They need to do a techno-economic analysis (H2A) to determine the projected impact of their work on hydrogen cost.
- It would be beneficial to perform an H2A model analysis and compare results with other similar technologies because, notably, the use of Ir seems prohibitive in practice.

Recommendations for additions/deletions to project scope:

- Understanding the role of particle migration/sintering upon cycling (and dependence on size and initial dispersion) for these Ir systems would be helpful for this project and others with similar PGM minimization objectives. The national laboratories might be engaged for this.

Project #PD-107: Hydrogen Fueling Station Pre-Cooling Analysis

Amgad Elgowainy; Argonne National Laboratory



Brief Summary of Project:

The objectives of this project are to (1) evaluate theoretical precooling requirements at hydrogen fueling stations; (2) collaborate to acquire information on refueling operation and review results; (3) examine current precooling equipment design and cost; (4) identify major drivers for precooling cost and energy consumption; (5) analyze tradeoffs between different design concepts; and (6) vet analysis results and findings.

Question 1: Approach to performing the work

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

- The approach is based on a single refrigeration cycle that is presumed to be representative of the industry. In reality, each of the station suppliers has a very different approach to solving this problem and this program does not cover the variety of market solutions very well. The analysis of the hydrogen halide (HX) effects on back-to-back (B2B) fills is interesting and valuable. Developing an improved methodology to determine the HX design parameters and to provide guidance to the refrigeration design is valuable. Similarly, the effects on Joule-Thompson (JT) expansion and HX performance are new and interesting.
- The project is well-defined with a targeted approach involving modeling of pre-cooling that takes into account appropriate variables and different fill scenarios. The tasks are feasible and integrated well with other efforts.
- The project is clearly defined, well-designed, and presented well.
- As the principal investigator (PI) noted, it is difficult to obtain pre-cooler designs from hydrogen station providers. There are some pre-coolers with designs that differ from those presented. However, in principle the PI has a sound understanding of their design and operation. The approach is robust and comprehensive.
- Very focused approach including review with industry.
- Interesting work—the approach is pretty well explained.
- The approach seems excessively focused on exactly what is done today instead of looking at better alternatives. Seems to be that the process lends itself to optimization, and it would be important to understand why things aren’t done better. The presenter stated during the presentation that, perhaps, energy efficiency does not matter—this poses significant ramifications for future delivery technologies.

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.

- Good accomplishments and significant progress are clearly presented. This project is sharply focus on improving the performance of hydrogen refueling stations (HRS) in terms of thermal loads, cooling system size, and associated costs.
- Good in-depth analysis of refrigeration process.
- Some good initial findings -- looking forward to more results from the project.

- The analysis is articulated well with relevant results showing correlations of key operating parameters as well as scope for improvement with respect to cost and energy usage for pre-cooling. It would be helpful to point out more clearly potential benefits of this exercise on the overall cost and energy consumption for dispensed hydrogen, and how it is helping progress towards DOE goals.
- Many good calculations for a small and brand-new project. I would, however, question some of the results. It seems to me that J-T expansion heating is overestimated. Running Refprop, the reviewer obtained a maximum J-T heating of about 30 K, for a maximum temperature downstream of the expansion valve equal to 25+30=55 °C, not 70 °C as shown in the figure. Also, Figure 8 shows a very sharp change in slope for the heat exchanger cost. It seems that heat exchanger cost should continue to gradually decrease as cooling capacity increases.
- While the reviewer did not see much value in the assessment of the refrigeration loop, the reviewer perceived the work that describes the operating limits on the HX as it pertains to J-T valve placement and B2B fills is valuable and suggested that it should be the focus of this effort. The reviewer also noted that the costing studies do not appear to be consistent with the limited cost data seen in other station designs and is of less value.

Question 3: Collaboration and coordination with other institutions

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

- Collaborations with other institutions are well-coordinated and effective.
- Good coordination of efforts. It is noticed that Linde was dropped as a collaborator from FY 2015 to FY 2016. A station operator is important to have as a partner to verify realistic data and to ultimately implement any improvements resulting from this exercise.
- Good collaborations and sets of partners, but the project may benefit from further interactions with Linde, Air Products, Praxair, etc., to figure out what they envision for the future. If this is an important problem for gas dispensing, these companies may have insight on possible approaches and they may even be willing to share their knowledge.
- The biggest weakness in the program is the lack of design data and information from existing cooling systems. Without this, it is difficult, if not impossible, to provide a good assessment of the station designs. A partner who is able to bring this information to the project would strengthen the team's capability considerably.
- No direct collaboration with station technology providers and operators. It would seem they would be interested in the cost and energy efficiency of their systems.

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.

- Since HRS requires significant effort in designing a system that could deliver hydrogen fuel under low temperature and high pressure safely and effectively, this project certainly provides the good solution for that. It has a potential impact on the success of hydrogen fuel delivering systems. The project fully supports and advances the progress toward DOE Hydrogen and Fuel Cells Program goals and objectives.
- This is an important and increasingly relevant topic of study—how to reduce station costs and improve efficiency of operation.
- Compressed gas dispensing is plagued by many issues, cooling being one of them. This is relevant to hydrogen-fueled transportation at least in the near term while a better approach to refueling and dispensing gains prominence.
- The development of a tool set that allows for better design of the cooling systems remains a critical weakness in station design and a gap that needs to be addressed for next generation station designers.
- While the quality of work done is very good, its importance and potential impact is questionable. As shown in slide 3, pre-cooling cost is only 10% of the installed station equipment cost. Thus, of the total cost of

dispensed hydrogen, it is a small portion. Moreover, the project is not targeting elimination of this cost, but simply optimization of it. So, the cost savings are expected to be a small fraction of the overall cost. While any cost savings are useful, it is a question of prioritizing in light of limited funding available. The analysis done to date is useful, but it may be reasonable to expect station designers to implement improvements and optimize operations to minimize cost.

- No high impact on capital cost. Higher potential to decrease operating costs through higher energy efficiency.

Question 5: Proposed future work

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

- The proposed future work is logical and technically sound.
- It seems that an important task may be finding a plan to eliminate station cooling. It seems that this effort would be well justified to avoid all the issues presented here.
- I would like to see an emphasis for the remainder of the project to be on the effects of the HX design on the ability to conduct B2B fills. If we had a better understanding on how B2B fills and heat exchanger properties affected the refrigeration requirements for the system, this would be a big help.
- I would suggest looking at the worst-case SAE J2601 scenarios to ensure the pre-cooler is adequately sized for “peak load.” This is typically the 50 °C ambient top up fill. Otherwise, proposed future work is a good summary of action items to enhance the current level of analysis.
- Very design focused. Since Honda is a collaborating partner, the reviewer suggests checking to see if results from the European project HyTransfer or other alternative approaches like this are taken into account for future work.
- This was a bit rushed in the presentation and could be made a bit more clear, but the project seems to be on track.
- Future work in the last Annual Merit Review (FY 2015) had the first item as “Design & develop new design concepts” (e.g., carbon dioxide, R507, etc., as well as relaxing the SAE J2601 30-second window). These do not seem to have been addressed. While the project has made significant progress in improving fundamental understanding of pre-cooling options, associated costs, and efficiencies and scope for improvement, continuation of this effort is questionable. It is not clear if it would add much value given the future work plan.

Project strengths:

- This project strengths are as follows: technically well-designed and planned logically; well-coordinated with other collaborators and institutions; and has been carried out in a timely manner, on schedule, and making good progress.
- Excellent analysis of pre-cooling requirements with good understanding of fundamental concepts. Adequate attribution of practical operating parameters and tangible outcomes.
- Experienced researchers.
- Systematic approach and relevance of station cost barrier.
- Exploring in detail the issues associated with hydrogen pre-coolers, which is a relatively poorly studied area.

Project weaknesses:

- Technically, there is no weakness in the design and analysis of this HFS pre-cooling analysis.
- Industry collaboration.
- The project is not expected to make a major impact on the overall cost of dispensed hydrogen. Although potential improvements are identified, the implementation strategy is not well defined. Efforts should now focus on field demonstration and manifestation of benefits.

Recommendations for additions/deletions to project scope:

- It is suggested that:
    - Consider using double pipe heat exchanger installed between the variable area control device (VACD) and HX
    - Locate the VACD further upstream of HX, not by using straight pipe, but by using winded-coil so that the VACD can be physically installed close to HX when it is necessary due to space constraint.

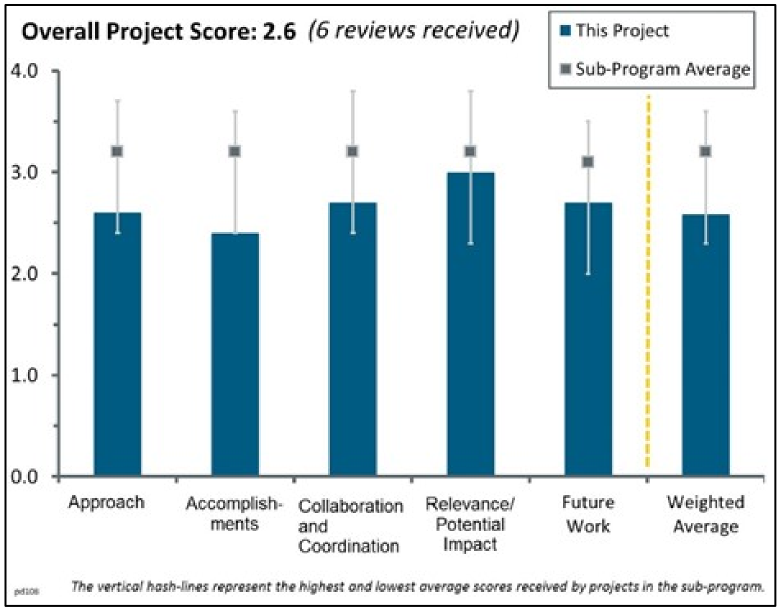
    Or item 1 and 2 can be combined to dissipate the extra thermal energy before hydrogen enters the pre-cooler HX.
- The project may be concluded with concrete suggestions to station designers and operators to reduce cost and energy consumption.
- Perform industry workshops and derive research, development, and demonstration projects with industry based on your results.
- Investigate and explain why things are done better. It is unclear if it just an effort to minimize initial cost, considering that energetic cost has little effect on the bottom line.

Project #PD-108: Hydrogen Compression Application of the Linear Motor Reciprocating Compressor

Eugene Broerman; Southwest Research Institute

Brief Summary of Project:

The objectives of this project are to (1) improve isentropic efficiency above 95% by minimizing aerodynamic losses, (2) reduce capital costs to half those of conventional reciprocating compressors by minimizing part count, and (3) reduce required maintenance by simplifying the compressor design to eliminate common wear items.



Question 1: Approach to performing the work

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

- The approach appears to be sound. The design seems to be progressing. There are many computer models and much theory, both based on simplifying assumptions. Very little validating data are shown.
- The novel compressor design is interesting. The project objectives seem ambitious, especially the reduction of cost statement. From the presentation, it seems that the project team was proceeding to build a compressor using this novel compressor design. It is suggested that the team build and test one stage first and determine that the proof of concept works before moving to building an entire compressor. The presentation made many performance claims based on theory, calculations, and predictions. The assumption of 100 bar inlet restricts the usefulness of this compressor in hydrogen station applications.
- There are three areas of concern with the approach:
  1) Major design changes, such as the switch from a sapphire to a ceramic piston—although it was probably necessary—do not engender much confidence that the team has thoroughly thought through the design issues with this machine.
  2) The team is not planning any reliability testing despite the importance of compressor reliability issues in industry. Given the novelty of the design, significant reliability issues would be expected with the first version.
  3) The footprint of the machine as laid out is likely impractical for a commercial hydrogen station. This could be remedied and is not a showstopper, as this is just a prototype, but the team should put some thought on how to shrink the overall package.
- The drive mechanism is novel, but it is unclear whether it is more efficient than existing technology. The investigators propose using superconducting magnets to increase efficiency. It is not clear how superconducting magnets would work in this application.
- It is not clear whether the cost barrier will be overcome through the proposed approach. No information is provided on the new design's effect on part-count reduction. In addition, it is not clear whether the efficiency claims can be experimentally verified until the end of Year 3. Go/no-go gate criteria have not been provided. It appears that the project will spend two thirds of the budget without any experimental verification of the assumptions that need to be proved to overcome the barriers. Thus, while the work seems technically sound, the overall approach could have been better.
- The approach to this phase of the project is fair, primarily owing to the balance of the reporting at the AMR and within the reviewer information provided. Beginning manufacturing without a proper risk mitigation plan seems unreasonable.

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

This project was rated **2.4** for its accomplishments and progress.

- This project is fundamentally a product design exercise and, as such, has multiple parts, including design of the magnet area mechanism, dry running piston section, and check valves given the high compression ratio, speeds, flow, etc. Regarding magnet assembly, accomplishments as presented display good engineering principles for magnet theory and good understanding of how the actuator should respond and act. The key area for concern is the extremely high cost per kw/kg compressed with this arrangement (fundamentally due to the power required to actuate the actuator), which far exceeds current compression technology. Further, the presentation states that the project is able to achieve 1.3 kWh/kg (using an advanced linear motor reciprocating compressor [LMRC]). This claim is presented with no base-level backup analysis and is simply unreasonable and unbelievable. Proving that significant reductions could occur should have been a go/no-go gate. Perhaps a conclusion could be drawn between the papers the project team members have read and their assumption, but it was not presented. Additionally, on the linear actuator point, the use of neodymium magnets is a cost-volatile choice. Neodymium rare earth magnets are subject to wild price swings, as the majority of this material can come from conflict zones, and magnet prices sometimes swing up to five times the normal market price. This should be taken into account for long-term cost analysis. Efficiency calculations were also extremely low, and no hard analysis was presented to explain the principal investigator's 70% possible improvement on efficiency. In looking at the piston section design, it seems that there is an unusually high dead volume at the top of the stroke of the actuator, which would severely limit the efficiency possibilities of the machine. Seal life and maintenance life values are not substantiated—data must be presented as to the baseline for these assumptions—especially because of the real fact that there is no standard for a 900B check valve/seal design for dry-running compressor technology. Additionally, it is not clear that the project has considered how brittle ceramic is or its possible effect on performance and actual function. The test loop is reasonable.
- The design seems to be progressing. The material selection is spotty. Some materials are defined by composition, form, and heat treatment (ferritic ductile iron casting ASTM A536 grade 60-40-18), some are defined by composition and heat treatment (Aluminum 6061-T6), and some are defined by composition only (Incoloy 903). Proper material definition would indicate a deeper understanding. It would have been nice to see some preliminary test data. To date, only theory and computer models have been provided, both based on simplifying assumptions.
- The project seems to be progressing according to the milestones listed. The full-scale design estimates of 930 cycles per minute (cpm) operation seem to be significantly higher than the 360 cpm (or 6 cycles per second [cps]) scheme that is listed in slide 12—thus, the full-scale design isentropic efficiency might be lower than the listed 99% efficiency at 6 cps operation. The definition of an "advanced LMRC actuator" is not given, and no justification is provided as to how this "advanced LMRC actuator" will achieve a specific energy of 1.3 kWh/kg.
- The energy usage of 9.2 kWh/kg is more than an order of magnitude above the DOE target of ~0.8. As this number does not even include the cooling load, the issue is even more problematic. When asked how the team had confidence that it could move its efficiency from 20% to 90%, it did not have satisfactory answers. It is questionable whether, in the absence of evidence to the contrary, a reasonable efficiency can be achieved. This is a serious problem and should kick off a detailed project review to determine whether, and if so how, the project can be saved.
- Increasing current observed efficiency from 20% to 90% seems a daunting task. Some of the suggested approaches, such as superconducting magnets, seem too complex and costly for forecourt use.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.7** for its collaboration and coordination.

- The project has multiple industry participants that are all pertinent to the success of the project. It is not clear whether the participants (other than SWRI and ACI) are providing only parts/design or in-kind

support as well. The issue of cost increase due to wrong materials selection (high-strength–low-alloy steel vs. superalloy) seems to indicate weakness in the project team.
- The collaboration within the team seems adequate. However, the team could benefit from additional feedback from station operators and designers. Had that collaboration existed, the advisor could have noted the low efficiency as a non-starter early on in the project.
- The collaboration is present, but moving ahead with a build of the machine without coming close to project goals of efficiency and other targets should have been more closely considered between the collaborators ACI Services Inc. (ACI) and Southwest Research Institute (SWRI). Everything is a creation on this project, and as such, there seems to be no collective plan to handle it if something does not work.
- The collaborators appear to be suppliers, not collaborators. It is not clear whether they had input into the design, selected and designed their components, or only supplied a catalogue component. Some evidence, even anecdotal, would help to show that the collaborators are contributing.
- The project needs input from a compressor manufacturer. Having a component manufacturer is valuable but not as good as a partner that makes actual compressors (Hydropac, for example).

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 project objectives are relevant to Fuel Cell Technologies Office goals in developing a low-cost compressor with reduced overhead and maintenance cost and high efficiency. If successful, the project has the potential for significant impact. However, budget issues and lack of experimental verification of various assumptions early in the project presents a risk that the objectives may not achievable.
- Because compressors are such a reliability and cost issue, the project could potentially have a great impact. This is provided that the team can overcome the very low efficiency of the machine and demonstrate adequate reliability and cost.
- If the project can turn it around and prove performance far exceeding what was presented, its relevance could be prominent. A good deal of additional analysis and design iterations must be made to reach potential cost targets.
- If successful, this has the potential of reducing the operating and capital expenditures for hydrogen compression systems.
- If successful, the project could have a significant impact on forecourt costs.

Question 5: Proposed future work

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

- The next steps are logical.
- The proposed future work is good in terms of proceeding according to the plan. It seems risky and very challenging to leave all the following to budget period 3: the fabrication of stages 2 and 3, their integration with stage 1, performing all the data analysis, and possibly resolving any potential issues that invariably crop up in any experimental/demonstration work.
- It is this reviewer's opinion that a design iteration to improve efficiency would be needed; however, the project is moving ahead to fabrication.
- It is not clear why we are proposing to build a 20% efficient compressor. Construction on the first stage has apparently gone too far to be stopped, but work on the second and third stages should be stopped and design changes made, verified, and implemented before construction is considered. There is skepticism regarding the ability of a ceramic–ceramic sliding seal to contain high-pressure hydrogen. This should be verified on a small-scale prototype before proceeding with full-scale.
- Future work information provided is too vague to evaluate with any depth, but it does not appear to address major risk issues with the project.

Project strengths:

- The concept is a strength, as it is a way to reduce moving parts and reduce footprint. The control scheme and test loop considerations are all sound and are strengths for a product.
- The project is investigating a novel design that has the potential to effectively address major compressor issues.
- The project hopes to overcome several barriers with its innovative design of a reciprocating compressor, such as through the use of a ceramic piston to reduce the coefficient of friction and maintain tight seals.
- The project presents a novel approach to compressor design.
- This is a somewhat novel approach.
- Theory and computer modeling are project strengths.

Project weaknesses:

- Fundamental concepts and analysis are missing/not considered. Cost analysis for rare earth magnets should be considered in long-term analysis. Seal clearance (in the technical backup slides) is extremely close, and for easily scratched parts (sapphire/ceramic), it could be a huge challenge to ensure keeping a seal at the high-pressure stage of compression. Low pressure could be fine. Additionally, at low speeds, this seal will have a tendency to leak faster that when at higher speeds. It seems a good deal of analysis went into the frame structure and not enough into the internal piston parts (for example, the real leak rate could have been tested statically).
- Current power consumption is greater than that of existing compressors, and ideas for improvement have not been carefully developed.
- Stage gates were not defined; experimental validation of the assumptions was not performed early in the project.
- The goal of 9.2 kWh/kg is not viable. This number should prompt a detailed project review and accounting for how or whether the project should go forward.
- There is a lack of bench data.

Recommendations for additions/deletions to project scope:

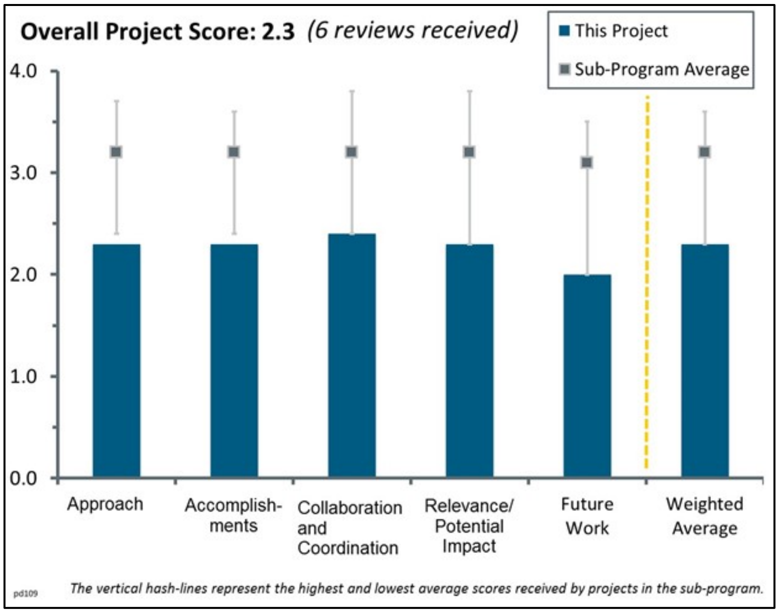
- The project should focus more on kilowatt-hours per kilogram of hydrogen rather than isentropic efficiency. The former is the metric that really matters. Investigators need to work with compressor manufacturers to validate assumptions and approaches.
- The project must regroup to analyze the piston area and seal on a smaller scale before the full build. Efficiency improvement would be a go/no-go gate to continue or, by analysis, determine the real possible improvement level.
- The project should describe the “part counts” of an LMRC vs. a conventional reciprocating compressor and provide the data that experimentally verify the life of seals (48 months) and valves (4 years).
- Reliability and efficiency testing should be added.

Project #PD-109: Steel Concrete Composite Vessel for 875 bar Stationary Hydrogen Storage

Zhili Feng; Oak Ridge National Laboratory

Brief Summary of Project:

The overall objective of this project is to develop a second-generation steel concrete composite vessel (SCCV) that will be more cost-effective for forecourt hydrogen fueling station applications. Other objectives include reducing the purchased capital cost of SCCV for forecourt hydrogen storage to $800/kg at 875 bar while meeting all other requirements, including a projected service life of at least 30 years and scalability to 1,000 kg of storage. The project will also fabricate a representative prototype mock-up, capturing all major features of SCCV technology.



Question 1: Approach to performing the work

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

- The approach seems reasonable, but the project should look at other options to ensure funds are not spent pursuing a non-optimum solution, as happened with the concrete reinforced vessel. Costs should be benchmarked with competing technologies—not just the U.S. Department of Energy (DOE) goals since other technologies are advancing quickly.
- The team's approach is to build on a previous Generation I (Gen I) steel concrete composite vessel to reduce the cost of hydrogen stored at pressures of 875 bar. The first set of analyses performed by the team was mostly focused on cost optimization. However, it was surprising that the team has not yet performed any structural analysis (e.g., finite element method analysis) or embrittlement analysis that would guide/support the cost optimization.
- The approach seems to be primarily an extension of the lower-pressure approach project. Some of the lessons learned, such as load-sharing wrapping, are being incorporated, although the movement toward replacing the pre-stressed concrete starts to point toward a reasonable design that might meet needs for both manufacturing and transportability. It would have been good to have seen more of a multivariate materials screening methodology applied to the project to ensure the best of all the appropriate materials are being utilized.
- The principal investigator (PI) was looking at a stainless layer inside the high-strength-steel shell, which would then be reinforced with concrete. Now the project is looking at no layer and steel-wire-wrapped high-strength steel. The approach seems to be all over the spectrum. The PI proposes to "replace the stainless steel inner layer with low-cost materials as [a] hydrogen permeation barrier," but there is no clue as to what that barrier might be.
- The intent of the project was to use low-cost concrete to build a low-cost vessel. At this point, the design does not incorporate concrete in any appreciable way. Barriers to the use of concrete have not been overcome.

Question 2: 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.

- Current techno-economic analysis is revealing a new direction in tank design, and screening high-tensile steels could be very beneficial across the Hydrogen and Fuel Cells Program.
- There is a certain irony to this project. It began as a steel vessel reinforced with concrete, and it has evolved into a steel vessel wrapped with steel wire—eliminating the concrete. While this is an accomplishment, it might pay to consider how this outcome might have been foreseen, thus avoiding the expense of the concrete-wrapped tank that was essentially non-productive.
- It is not clear that there is any need for a large-volume 850 bar vessel, as this would not be very helpful in cascade for an SAE J2601 compliant dispenser. It was not clear why the PI switched from the previous concept of concrete reinforcement to wire-wrap reinforcement, and achieving success with the new approach does not look any more feasible.
- Given a start date of October 2014, it seems that the main accomplishment by now is the cost optimization beyond the Gen I vessel. Given that the project has only another one and a half years remaining, it is not clear how the team will be able to assess fatigue life, assess hydrogen embrittlement, and design and build the Generation II vessel.
- There are other pressure vessel technologies, such as composite overwrapped pressure vessels (COPVs), that provide a much more efficient method for hydrogen storage and transport.
- Although the project is still proceeding, it has not validated its original premise for use of concrete. It would be better to end the project and let other projects carry the work forward.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.4** for its collaboration and coordination.

- The collaboration between tank designers, cost analysts, and materials experts is a very strong aspect of this project.
- A big team is involved; however, the activities of the different team members are not yet clear, given that most activities have focused on cost optimization.
- Reliance on ASME codes will be the downfall of this project and any innovation on steel tanks for hydrogen service. Already there are European Pressure Equipment Directive tank designs that are welded steel, are 50 bar working pressure, and contain ~375 kg in 90 m^3 transportable vessels (the largest tanks that can be shipped by regular lorry over most roads) that are priced at $320/kg. Collaboration with Pressure Equipment Directive standards at the Compressed Gas Association level has been blocked for years, and the United States has weak standards for welded tanks. Tanks built for 50 bar service in the United States to ASME standards will be at least 50% thicker wall and have 50% greater materials costs, but these tanks will still beat DOE cost targets.
- Other collaborators are listed, but the work seems to be focused mostly at Oak Ridge National Laboratory.
- It is not clear that adequate interaction with fuel station providers is sufficient to warrant production—i.e., it is not clear that there is a need for these vessels.

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.

- If the goals are reached by the end of the project, the potential impact would be strong. It is just not clear that this will be attainable in the remaining timeframe.
- This project could potentially provide a vessel that could meet the current cost targets while being manufacturable and transportable, thus supporting the growth market for hydrogen stations across the nation.

- The work is relevant, but DOE should make sure that competing technologies are not on target to equal or surpass the cost/performance of this material.
- The pressure rating of the vessels is not particularly useful for fueling. The pressure rating of the vessel is roughly equal to that required for fueling, leaving little usable quantity. Hydrogen fueling stations do not have a need for such large vessels at this high of a pressure. The cyclic life is not likely to be sufficient for a fuel station. There is virtually no mention of cycle life or a plan to address testing cycle life. It will likely be impossible to fill the vessel while it is being used because it will be producing a large number of cycles, even if partial. It is not clear that the "cost" would end up being a real sale "price" until someone is willing to build and sell for the cost listed. There would be other corporate overhead costs and the profit margin to apply.

Question 5: Proposed future work

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

- The focus on health monitoring and inspection is good and could be cross-cutting. It would be good to see more of a multivariate screening of materials.
- The focus should be on the use of Type 4 polymer-lined tanks for high-pressure hydrogen storage. Steel liners are not the best for high-pressure storage. The PI proposes to "replace the stainless steel inner layer with low-cost materials as [a] hydrogen permeation barrier," but there is no clue as to what that barrier might be.
- Weaknesses include the lack of complete structural analysis, fatigue life assessment, and hydrogen embrittlement assessment.
- This work should not proceed until this approach has been compared to other storage technologies to ensure that money is not spent on another suboptimal approach.
- The use of concrete has not been proven to provide value; therefore, the project may as well be stopped. There are other projects already working on high-pressure wire-wound vessels of the same type.

Project strengths:

- The project has proven that concrete is not going to be a viable pathway.
- The project builds on previous experience.

Project weaknesses:

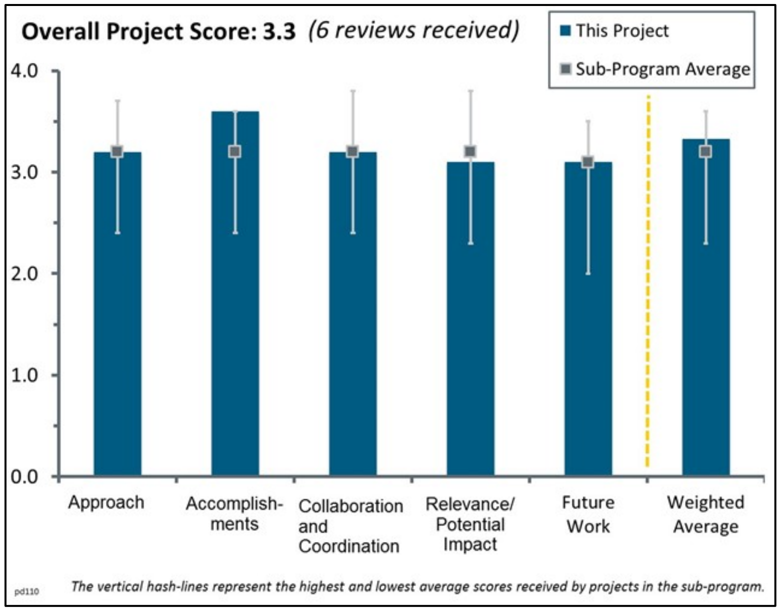
- ASME codes are not being aggressively revised to support hydrogen infrastructure needs—in fact, the team seems to be dragging feet.
- Failure to benchmark with competing technologies is a project weakness.
- Concrete has not proven to be a viable material, but the project is continuing.

Recommendations for additions/deletions to project scope:

- The project should coordinate with codes and standards to confirm that cylinder lengths will not compromise station design with setback distances. An independent benchmark analysis of this and competing technologies should be added.
- The project should eliminate the remote sensor technology cost optimization and focus on fatigue life and hydrogen embrittlement assessment and proof of concept.
- The project should stop progress and not proceed to the manufacture stage. There is already another project designing comparable vessels of the type to which this project has evolved.

Project #PD-110: Low-Cost Hydrogen Storage at 875 bar Using Steel Liner and Steel Wire Wrap

Amit Prakash; Wiretough Cylinders



Brief Summary of Project:

The overall objective of this project is to develop a pressure vessel with a capacity of 765 liters to safely store hydrogen at 875 bar that also meets the U.S. Department of Energy (DOE) storage tank cost target of <$1,000/kg hydrogen. The vessel must have a lifetime that exceeds 30 years/10,000 pressure cycles, have a safety factor of 3 on burst pressure to operating pressure, deliver hydrogen that meets SAE J2719 hydrogen purity requirements, and have a design consistent with relevant ASME codes.

Question 1: Approach to performing the work

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

- The overall approach is well planned and should lead to the development of a high-pressure vessel for hydrogen storage that satisfies DOE targets.
- Use of commercial cylinders should minimize costs. Project is well defined and described. Investigators are looking at several methods to improve design and performance.
- There seems to be progress in pursuing an alternative technology to composite wrap. It would be good to see a direct comparison of cost between wire and composite to understand the full, long-term benefit.
- The project seems to be moving along with the ASME stamp of approval for vessel design. The focus on fatigue crack growth rate (FCGR) is appropriate to answer many of the anticipated issues with the vessel and will provide a good basis for future design efforts. It is suggested that the FCGR studies be extended across both temperature and pressure cycling regimes, although doing so may be a difficult task.
- The approach based on assessing FCGR in the liner material starting from a flaw that is 3% of the thickness is a proper one. Autofrettage is used to reduce the tensile stresses, but stresses become positive upon the application of the load, so the purpose of exploring negative R ratios for liner fatigue is not clear.

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 accomplishments and progress of this project to date have been outstanding. The team has been able to approve an ASME code case for a Wiretough design, perform detailed fatigue tests, and plan for different design improvements.
- The project has progressed well and seems to be on target for producing workable cylinders. ASME certification is a great accomplishment.
- The ASME certification of the project results is a good progress indicator. The results shown on slides 9 and 10 constitute significant progress toward the analysis reported on slide 11. For the results shown on slide 11, it would be good if the team showed the magnitude of the hydrogen pressure reported in the first column next to the magnitude of hoop stress. The calculation of the cycles to failure also requires

knowledge of the threshold stress intensity factor range. It has not been reported what this threshold value is.
- These pressure vessels are way too heavy and impractical. Composite overwrapped pressure vessels could provide a much more efficient method for storage and transport of high-pressure hydrogen.

Question 3: Collaboration and coordination with other institutions

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

- Continued collaborations with the Oak Ridge National Laboratory (ORNL) on the fatigue studies will definitely benefit the project. Also, a number of other collaborators are involved in the different tasks.
- There is good work with ASME to certify cylinders. The project appears to be utilizing ORNL's and Sandia National Laboratories' resources effectively.
- Collaboration with Dr. Kevin Nibur is a unique strength for the project. Dr. Nibur is a world expert on experimental measurement of fatigue crack growth rates.
- Collaborations between Wiretough, ORNL, N&R Associates, CP Industries, Structural Integrity Associates, and Hy-Performance Materials Testing seem to be adequate to meet the project demands currently. The project might need a new partner that could possibly address cyclic fatigue across the actual pressures and temperatures.
- The stainless steel liner is a concern for hydrogen embrittlement.
- There is no collaboration with fuel station providers to understand whether the resultant product will meet industry technical needs, or whether there is a market for the vessels.

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 successful, the project will have a significant impact on reduced-cost storage of hydrogen at high pressures.
- The project has the potential to decrease costs for station storage.
- The project aims to deliver a safe and reliable storage vessel at 875 bar hydrogen pressure on the basis of wrapping the cylinder with ultra-high strength fibers. The reviewer cannot assess the project's effectiveness in achieving its objective because there is no comparison between cycles to failure in the presence or absence of wrapping.
- The tank design shows potential to meet the cost targets. However, it is not certain that the principal investigator is considering installation in the model since it is not part of the target metric, but installation should definitely be considered when understanding costs.
- The pressure rating of the vessels may not be particularly useful for fueling. A statement is made that the pressure rating might be in the 10,000–15,000 psig range. To be useful, the pressure must be at least 14,000 psig, and preferably 15,000 psig. The cyclic life is not likely to be sufficient for a fuel station. There is mention of cycle life and material testing, but no details on final pressure rating, cycle range, and cycle count of final design. It is not clear that the "cost" would end up being a real sale "price" until there is someone willing to build and sell for the estimated cost. There would be other corporate overhead costs and a profit margin to apply.

Question 5: Proposed future work

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

- The project appears to be on track for successful completion.
- The hydrogen embrittlement and fatigue performance is great.
- Future plans are adequate. However, some focus on crack initiation and not just pre-existing defects would strengthen the design and safety of the vessel.

- There still seems to be a number of challenges remaining for this project to be successful; it is not certain that the researchers can address everything. The project should focus on a few of the challenges that would provide useful data to DOE for future efforts in the materials design space.
- The approach to reduce the yield strength of the liner seems to be moving in the right direction, but the gain due to autofrettage needs to be ascertained given that the compressive stresses will also be lower in that case.
- The project needs to perform actual cycle testing on the prototype vessel. The project should complete a cost analysis with detail earlier in the project than shown.

Project strengths:

- The project is pursuing the metallurgical testing for the base materials. The project is trying to demonstrate a new technology for vessels and is making progress.
- There is a good blend of practical and theoretical work to optimize cylinder performance and costs.
- The credentials of Dr. Saxena and Dr. Nibur are project strengths.

Project weaknesses:

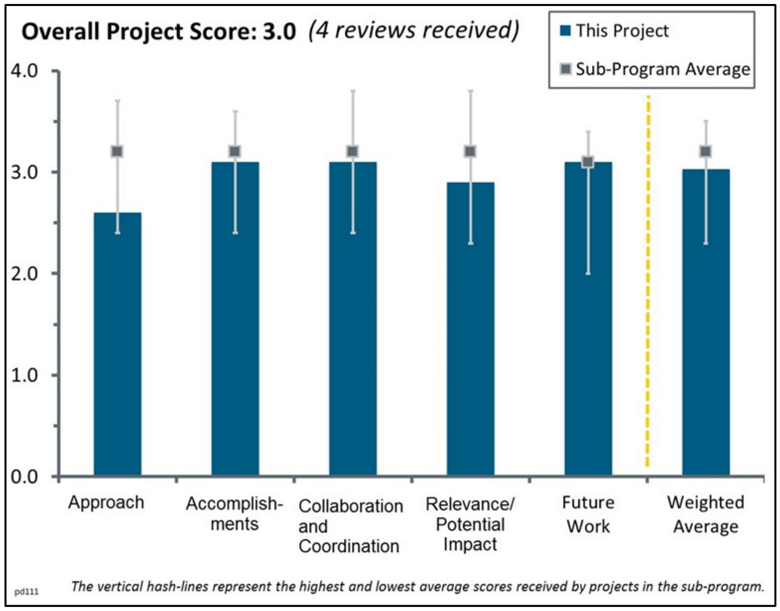
- It is not clear how much the autofrettage affects fatigue crack growth. In fact, the elaboration on negative R ratios seems to be out of place because fatigue crack growth under hydrogen pressure takes place under positive hoop stresses (slide 11) and hence positive R.
- There is insufficient discussion about fatigue life at a given pressure cycle range to understand whether these vessels will meet the intended service. There is no support for the basis of meeting the storage tank cost of less than $1,000/kg, other than statements that it will be met.

Recommendations for additions/deletions to project scope:

- The project should compare projected costs to competitive technologies in addition to DOE goals. DOE should set a recommended standard pressure cycling regime (cycles/day and pressure swing) to be used to estimate cycling effects on vessel lifetime.
- The effect of the axial stress needs to be investigated. In fact, possible failure scenarios due to axial stress need to be envisioned and outlined.
- The project should perform some crack initiation studies as well.
- There should be additional support for the contention that the sub-$1,000/kg storage tank cost goal can be met. Better define cycle range and number of cycles are needed. There should be a step to actually test and demonstrate the resultant vessel to these design parameters. This does not appear to be included.

Project #PD-111: Monolithic Piston-Type Reactor for Hydrogen Production through Rapid Swing of Reforming/Combustion Reactions

Wei Liu; Pacific Northwest National Laboratory



Brief Summary of Project:

Bio-oil reforming technology advancements are being pursued in this work. Pacific Northwest National Laboratory (PNNL) is working to (1) reduce the capital cost of plants through minimized unit operations, smaller pressure swing adsorption, and process simplification; (2) increase energy conversion through in situ CO_2 capture and in situ heat exchange between reaction and regeneration; and (3) increase operating flexibility and durability through reduced operations and maintenance requirements.

Question 1: Approach to performing the work

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

- It is good to see the focus on kinetics and heat and mass transfer. The project should accelerate this work and develop a basic process model to predict hydrogen production and bed temperatures as a function of feed and regeneration conditions. (Leveraging Dason Technology here would be a good idea.) Also, efforts should be focused on understanding the impact of coking and sulfur on catalyst performance and ability to regenerate; these responses have the potential to be killer variables. The techno-economic analysis shows a key feature of this project is a substantial reduction (50% assumed) in bio-oil feed to achieve the same hydrogen production. It is not clear how to assess the work being done against that goal. Also, it would be good to see the explicit relationship between regeneration conditions and hydrogen production cost.
- The project team is taking a good approach by starting with looking at materials innovation for both the sorbents and the catalysts, followed by demonstrating the reactor innovation with actual bio-oil, and then integrating the system and developing an innovative process.
- The early focus on identifying reforming catalyst materials, followed by optimizing reactor and process systems, is reasonable. However, the project should also provide the assumed mass and energy balance for the sorption and regeneration reactor systems. The stated improvement in smaller bio-oil usage per unit hydrogen produced may be explained by the fact that a significant percentage of the hydrogen is coming from steam. As such, it is not clear if the calculated cost reduction from this work accounts for the higher energy consumption due to steam production and high-temperature regeneration. The Hydrogen Analysis (H2A) model analysis is overly simplistic. It is not clear what the basis was for the assumed over 50% reduction in future bio-oil feedstock cost compared to the H2A default. Similarly, it is not clear why, in slide 4, the future capital and operations and maintenance (O&M) costs—the two identified barriers in the Fuel Cell Technology Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP)—are assumed to be constant relative to the H2A default.
- In principle, the proposed approach is interesting and attractive: addressing bio-oil conversion issues by utilizing coke formation to its advantage. However, there are significant operational challenges, especially with operating a dual bed swing reactor system at high temperatures. Thermal management and cost of materials are some of the key factors. The investigators should refer to previous efforts to develop similar technologies for natural gas reforming, none of which have been successful; bio-oil is even more challenging.

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.

- Impressive progress has been made on identifying lead catalyst and sorbent materials and proving out the catalyst/monolith system (albeit on a simulated bio-oil, which avoids coking and sulfur complications). The reviewer looks forward to seeing the results with both catalyst and sorbent loaded onto a monolith.
- Accomplishments and progress are excellent on the sorbent and catalyst portions of this project, which are key aspects to the success of this work.
- Good progress has been made in developing novel catalyst/CO_2 sorbent materials. The monolith base has benefits with respect to pressure drop, catalyst distribution, and uniform gas flow. The critical step is operating a dual bed system continuously with thermal stability and consistent output. More time should be devoted to operational aspects; if successful, catalyst/materials can be further improved later. Based on prior experience with similar systems, the viability of this approach in the long run is questionable. While switching between reforming and regeneration steps, there will be some residual gas left in the system that will need to be purged to avoid contamination. This step can cause instability and added cost that should be addressed. It is essential to have mass and energy balance for the cyclic process to know how much coke must be deposited for adequate heat generation. The cycle time may need to be adjusted accordingly to make sure the system is thermally balanced, including the purge step.
- The catalyst performance and stability results look promising. In addition, the reported catalyst improvement over packed bed by use of a monolith reflects nice progress. That said, the project team should carry out additional parametric tests to pin down the mechanism for this observed improvement, namely the quantitative impact of changes in weight hourly space velocity or steam-to-carbon ratio.

Question 3: Collaboration and coordination with other institutions

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

- A very strong team and collaboration are covering the main aspects of this work: materials innovation (PNNL), catalysis (Washington State University), monolith support (Cormetech, Inc.), and process design (Dason Technology). There is a very good mix of industry, national laboratories, and academia.
- There is a good mix of industrial and academic partners.
- Selected partners bring necessary skills and capabilities. Nevertheless, a major industrial partner with significant operational experience could provide valuable feedback on critical challenges of high-temperature cyclic reactor operation. This is a deficiency in the current team.
- Overall, team collaboration is reasonable, although the project could benefit from partnering with a feedstock/bio-oil supplier.

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.

- This project will support DOE's goal of reducing distributed hydrogen production costs, and the H2A model analysis presented by the project team showed that this technology could potentially meet the production cost targets.
- Reducing the cost of bio-oil conversion to hydrogen is important for cost-competitive renewable hydrogen production. As projected in slide 4, the proposed method can result in a significant cost reduction. However, the assumptions need to be verified under realistic conditions. For example, there is a need to explain and verify a 50% reduction in bio-oil usage, as it contributes significantly to the projected cost reduction. The investigators should make sure that some of the critical issues on approach and progress are adequately addressed and properly accounted for in the cost calculations, for example, purging step operation and associated equipment, as well as high-temperature switching valves and other materials.

Although some CO_2 is removed in situ, based on the results presented and configuration shown, this step does not eliminate water–gas shift and pressure swing adsorption equipment. Thus, capital cost reduction is not obvious.

- It would be good to understand the potential for this technology vs. electrolysis in forecourt applications. The project should consider adding quantitative technology goals that target key barriers, in addition to 80% conversion efficiency and hydrogen production cost.
- The project is fairly aligned with the MYRDDP and aims to address key barriers to commercialization. However, the benefit of this approach over steam methane reforming is not obvious and needs to be made up front, including quick greenhouse gas (GHG) life-cycle analysis.

Question 5: Proposed future work

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

- The proposed work looks good and is aligned with the approach presented for this project. A key aspect for the success of this project is the demonstration of the integrated reactor system, and the team is planning to complete this task.
- The stated milestones and the proposed approach to achieve them are aligned with the overall objectives. The project needs to make the transition from simulated to real bio-oils before the end of the project period.
- Proposed future work addresses key areas but fails to adequately address cyclic operation in detail. Investigators should carefully consider thermal management, purging requirements, high-temperature switching valve selection, and durability of the high-temperature components.
- It would be good to see a vision for (1) how to deal with bio-oil composition variability and (2) how to scale up to approximately a ton of hydrogen per day.

Project strengths:

- The project team has strong catalyst material screening and conceptual reactor design capabilities.
- Project strengths include knowledge and expertise in catalyst and sorbent development.
- The partnership with Dason Technology is a strength.

Project weaknesses:

- It is ambitious to start with bio-oil as the feed.
- Not enough consideration is given to potential non-matching conversion and kinetics of the competing reactions for both reforming and regeneration conditions, which are likely to have significant impact on overall system design and O&M costs. Although not the primary goal of the project, there was not much discussion or analysis of the in situ CO_2 capture portion of the system.
- Project weaknesses include the team's experience with high-temperature cyclic operation and its challenges.

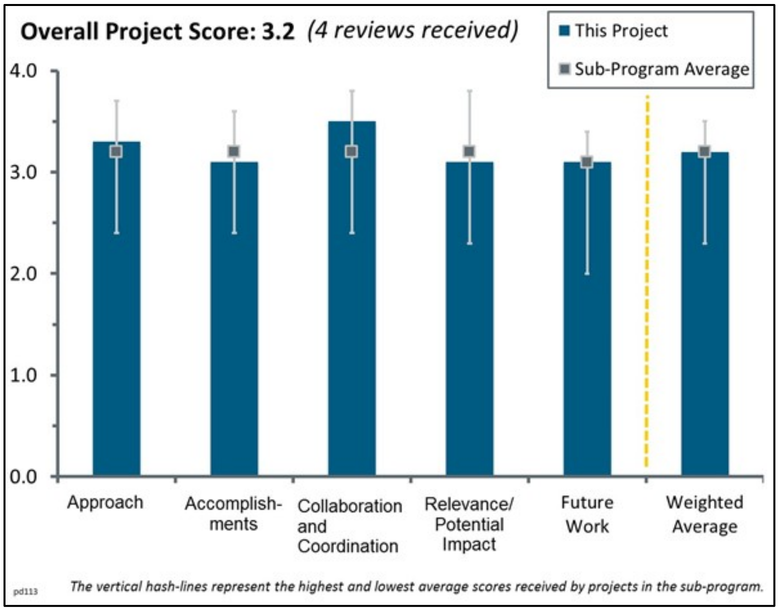
Recommendations for additions/deletions to project scope:

- The role of coke burn-off must be better understood. Questions include how much coke is ideal and whether the feed composition can be related to the amount of coke that will be deposited; the latter will be critical for bio-oil since the composition will vary considerably depending on how it is made. The project should consider modeling (or even testing) the use of bio-gas instead of bio-oil.
- The project should address the challenges of matching the reaction kinetics of the various reforming and regeneration reactions within the temperature and pressure operating envelope. The project team needs to address the likely sensitivities of the reforming catalyst performance with respect to temperature and the likely seasonal and regional feedstock variabilities. The 2006 reference to the National Renewable Energy Laboratory report and cost numbers looks outdated. There has to be a more recent and relevant reference.

Project #PD-113: High-Efficiency Solar Thermochemical Reactor for Hydrogen Production

Tony McDaniel; Sandia National Laboratories

Brief Summary of Project:



The objective of this project is to develop and validate a particle bed reactor for producing hydrogen via a thermochemical water-splitting cycle using a non-volatile metal oxide as the working fluid. Sandia National Laboratories (SNL) will demonstrate eight continuous hours of "on-sun" operation, producing greater than 3 L of hydrogen by the end of the project. Fiscal year (FY) 2016 objectives are to (1) discover and characterize suitable materials for two-step, non-volatile metal oxide thermochemical water-splitting cycles, (2) construct and demonstrate a particle receiver-reactor capable of continuous operation at 3 kW thermal input, and (3) conduct full techno-economic, sensitivity, and trade-off analysis of a large-scale hydrogen production facility using a plant-specific predictor model coupled to the Hydrogen Analysis (H2A) model.

Question 1: Approach to performing the work

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

- The SNL team has outlined an aggressive but reasonable approach for meeting project objectives by the end of 2016. The SNL team made the correct decision to focus on the demonstration of hydrogen production to meet U.S. Department of Energy (DOE) requirements. SNL continues to improve on timeliness and effectiveness of communications and coordination with the entire project team on component design and fabrication. The team also continues to improve on materials characterization (e.g., through a "virtual lab" using the stagnation flow reactor to investigate materials kinetics and thermodynamic properties). Although finding optimal reaction materials will require more time and resources than the current effort allows, it is anticipated that these investigations will continue in the future.
- The team has identified key barriers in developing this technology. Objectives are being met in a timely manner and directly address the pathway barriers. Further work is still required to finalize models and conduct the complete techno-economic analysis.
- The team made a good decision to settle on CeO_2 and focus on demonstrating the key aspects of the reactor design.
- The researchers have a very large collaboration that combines experimental work, theory, and techno-economic analysis. They need to state how they will characterize the materials in terms of redox potential, cycle life, mechanical strength, etc., or at least put in a reference where the characterization procedure can be reviewed. They need to define their target metrics in terms of materials performance—moles hydrogen/gram material, cycle time, cycle life, etc. This is missing and is very important to assessing progress and feasibility.

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.

- Excellent progress has been made in completing and validating the Cascading Pressure Receiver-Reactor (CPR2) design, in designing and fabricating reactor components in preparation for testing, and in combining modeling and experimental investigations to accelerate material discovery for materials of reaction. The project team continues to devise novel and creative testing methods (e.g., stick slip measurements to determine flow rates of the reaction particles) in preparation for full-scale testing of the CPR2. Materials discovery work has yielded promising results, but it will take extended investigations beyond the scope of this project to find an optimal material of reaction having the thermochemical and performance parameter values desired for solar-to-hydrogen (STH) efficiency >20%.
- The work in question has made significant progress in demonstrating the target technology. Obviously, great strides have been made in the solar receiver technology. Specifically, many materials have been analyzed for effectiveness. The study could still benefit from a better understanding of the problems regarding the synthesis of $CeFeO_3$. This may prove to be a critical issue because $CeFeO_3$ seems to be the preferred material for this technology.
- While the team is taking a rigorous approach to modeling the process, the lack of techno-economic analysis at this point in the project is disappointing. The techno-economic analysis should have been an earlier objective, even if subsequently revised, because it helps to identify the critical technical challenges, which might have changed the project strategy.
- It is good to see that the researchers have validated the theory by finding and testing some potential compounds. Whether the compounds perform as well as predicted is not stated in their slides, however. This project is very well funded, so the progress on constructing the test system is expected. They reported screening 50 new compounds, but it is unclear whether these compounds were the same ones reported in 2015, and the results of that screening were not shown. It was also unclear whether the compounds identified in the density function theory (DFT) modeling performed as predicted. This finding should have been reported because it would validate the DFT modeling task. In addition, how the screening was done is not discussed. For example, they do not disclose how many cycles compounds were tested for or how well the experimental work compared to what was predicted. This is extremely important. They are spending a lot of effort on DFT modeling to direct their discovery. They need to show that the modeling is accelerating the discovery, which from the data is not known. It is difficult to see what was changed and why on the MATLAB work from FY 2015. They were debugging the model last year and are now ready for exercising it. This was done on a sub-contract, which the presenter said they were not happy with and that they are changing contractors. Based on the results presented, a change in sub-contractors is a good decision, and the principal investigator/project manager should be commended for making the change. There do not seem to be any journal publications, which given the nature of the work, the funding level, and the length of time the project has been active is surprising.

Question 3: Collaboration and coordination with other institutions

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

- An excellent project team has been assembled for this project, which leverages materials experience and reactor design capabilities at SNL Livermore and Albuquerque, respectively, and academic materials discovery capabilities at several key universities, as well as access to DOE Office of Science facilities. The interaction with the German Aerospace Center will allow significantly more progress to be made in this effort than would be the case if the U.S. team were working alone. The new “virtual lab” capability for the stagnation flow reactor will facilitate further collaborations with project partners. It would have been useful to hear if any progress has been made (as in the photoelectrochemical [PEC] community) in coordinating across the research community to establish conventions for analysis, best practices, and key measurements, and in coordinating and communicating materials discovery approaches, testing protocols, and reporting standards.

- There seems to be sufficient collaboration between various groups. The groups also include both research and corporate aspects, which inspires confidence that production goals will be met. There is some indication from the presenter that there was a previous problem with the timeliness of work. This issue was also stated to be resolved, but careful team member selection should be maintained in the future.
- The project has a lot of collaborations.

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 supports the Fuel Cell Technologies Office (FCTO) goal of reducing the cost of hydrogen production from renewable resources to < $2/kg, as well as the objectives to verify the competitive potential for solar thermochemical (STCH) cycles for hydrogen in the long term and to develop this technology to produce hydrogen by 2020. Meeting the cost goal will depend not only on the successful completion of this project, but also on technology development beyond the scope of the project and the FCTO (e.g., lowering the cost of the heliostats to the DOE Solar Program SunShot target).
- The project goals align well with DOE targets for water-splitting technology. While the work is only laboratory scale, at least some thought should be given to large-scale operational considerations such as heat recovery and equipment size for full operations.
- This technology is in its infant stage, so it has potential to make an impact, but until it is further developed, the impact is unclear. The system is extremely complicated and still has many technical challenges to be solved before its impact can be assessed.
- This technology does not have a convincingly reasonable potential to achieve $2/kg hydrogen. Multiple breakthroughs are required (e.g., materials, reactor design, and thermal management).

Question 5: Proposed future work

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

- Information provided on future work addressed the remainder of FY 2016 and included plans for an on-sun test, full techno-economic analysis, and publication of results. The planned future work is totally reasonable and as much as the remaining time allows. Additional discussion of next steps after the conclusion of this effort would have been helpful, even though they are, of course, contingent on the results of the 2016 tests and analysis. Continued cost analysis should take place. Heat recovery in the receiver-reactor will need to be more fully addressed, and additional materials discovery work will be needed to identify the optimum materials of reaction. Going forward, coordination with the University of Colorado and others on materials of discovery work would be beneficial.
- The approach taken to plan the project timeline, as shown in the Gantt chart, is appreciated. The project should develop a concept for particle illumination at scale and determine the implications for time on sun. The project should identify key challenges associated with heat management and determine implications for economics.
- The work is promising, but foresight should be given beyond laboratory-scale demonstration, when possible.

Project strengths:

- The project team and progress to date are excellent, with the project team focused on demonstrating hydrogen production by the end of FY 2016. The coordination between materials discovery work and component and reactor design and analysis is noteworthy and should serve as a model for future efforts of this kind.
- The project has demonstrated significant work in materials studies as well as manufacturing techniques. Further, the modelling work is well supported, and the amount of collaboration for the team is good.
- This is a well-funded, long-term project that has a very strong team.

Project weaknesses:

- No significant weaknesses were apparent; however, updates to H2A were not provided or discussed. No information was given on recent publications, patents, or collaborations with the greater research community on establishing testing protocols and coordination of analyses.
- Careful selection of team members needs to be maintained consistently to ensure timely work responses. Thought should be given to how large-scale operations will function, especially with regards to heat recovery and equipment size.
- The response to the second comment listed on slide 15 was poor. The team should provide data to address the challenging technical issues—redox material performance, circulation of high-temperature materials, high-temperature structural materials challenges, radiative heating challenges, and material durability challenges (added here because the active materials must go through significant stress in this process). The team should address these issues with data. The team also states that STCH has the potential to be more efficient than PEC and photovoltaic (PV) plus electrolysis. For low-temperature electrolysis, this may be true; however, high-temperature electrolysis has the potential to be more efficient than STCH. The comparison to low-temperature electrolysis is not fair since PV plus electrolysis is at a high Technology Readiness Level (TRL) compared to the low TRL of STCH. In addition, as PV technology improves, which it will, the PV plus low-temperature electrolysis may become more efficient than STCH. Finally, while STCH may have the potential to be more efficient than PEC, PEC is so much simpler that it may be preferred over STCH. The team should not disparage other technologies to try to justify its research.

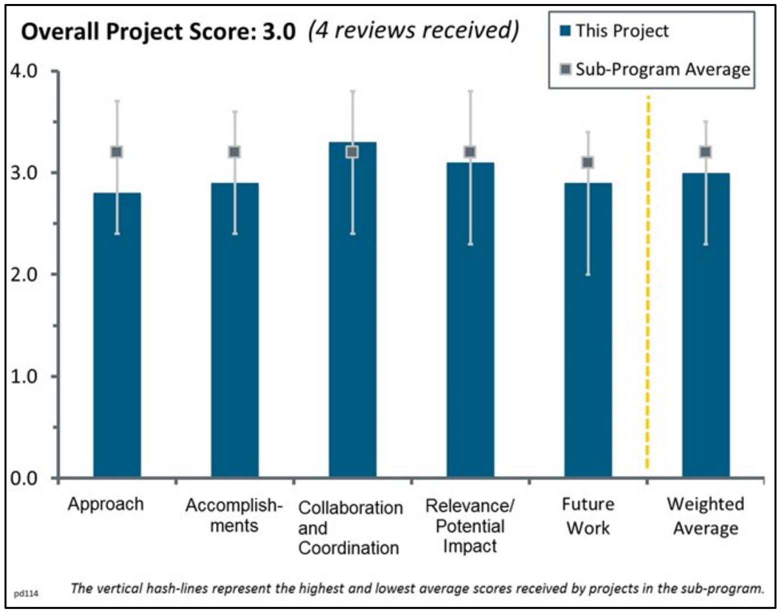
Recommendations for additions/deletions to project scope:

- The project should work to indicate how effectively a column of particles will support a pressure differential. The project should identify the critical particle properties (e.g., size, packing characteristics) and determine how the pressure drop depends on column height.
- The project should continue to update H2A of costs for hydrogen production and compare it to updated analyses of PV electrolysis and high-temperature solid oxide electrolyzer cell electrolysis. The project should also encourage the materials community involved in high-throughput computational materials screening to coordinate and collaborate on establishing materials discovery approaches, testing protocols, and reporting standards.
- The project should define target metrics in terms of materials performance—moles hydrogen/gram material, cycle time, cycle life, etc. This is missing and is very important to assessing progress and feasibility.

Project #PD-114: Flowing Particle Bed Solarthermal Reduction–Oxidation Process to Split Water

Al Weimer; University of Colorado

Brief Summary of Project:



The overall objective of this project is to design and test the individual components of a novel flowing particle solarthermal water-splitting system and show a pathway to a system capable of producing 50,000 kg of hydrogen per day at a cost of less than $2/kg. Further objectives include (1) identifying and developing high-performance active material formations; (2) synthesizing flowable, attrition-resistant, long-use spherical particles from low-cost precursors; (3) demonstrating high-temperature-tolerant, refractory, non-reactive containment materials; (4) constructing a flowing particle redox test system and testing components of the system; and (5) monitoring progress toward cost targets by incorporating experimental results into frequently updated and detailed process models.

Question 1: Approach to performing the work

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

- The comprehensive approach is laudable. However, the effort is spread across an extremely wide range of objectives. Prioritizing the list of objectives is suggested. Then the project should concentrate resources on those near the top of the list that can be realistically addressed in the remaining project time. A specific recommendation is to de-emphasize improvement of the redox material since the hydrogen production target has been met and focus on other critical aspects—especially since the stated objective is to move this technology from Technology Readiness Level (TRL) 2 to TRL 3.
- All of the barriers presented as a part of the approach are clear and directly related to water-splitting. However, there are a great many barriers listed, and the project should take care that there is sufficient time to address all of the barriers. Further analysis and details may be required for details of particle flow and entrainment in the system. Hydrogen Analysis (H2A) results and key input parameters—not just sensitivity results—should be supplied if possible.
- The project approach is to test and validate the performance of components of the project's reactor design to move the technology concept from TRL 2 to TRL 3. This involves synthesis of reactive particles, design and demonstration of high-temperature containment materials, performance and attrition resistance testing of materials of reaction and construction, iterative updating of an Aspen process model and H2A, and collaborations with partners for identification and design of materials of reaction (University of Colorado) and "on-sun" testing (National Renewable Energy Laboratory [NREL]). Challenges and barriers were adequately addressed.
- The project approach was outlined and is logical; however, a project schedule for meeting project goals and U.S. Department of Energy (DOE) requirements was not given. A timeline for achieving all the go/no-go decision points and the DOE requirement for eight hours' continuous on-sun operation and production of greater than 3 L of hydrogen is needed in order to determine whether the project approach is appropriate. Preparing for on-sun testing and demonstration of hydrogen production requires working on many technical

fronts simultaneously, and it is not clear that all technical issues will be addressed sufficiently to meet the requirements in the project time remaining.
- The approach of experimental work coupled with theory closely resembles work done by Sandia National Laboratories. They should collaborate. The researchers need to define their anticipated system operating parameters such as cycle time for the materials. It is not clear whether this will this be 24/7 operation or only when there is sun. If it is 24/7, it is not clear whether they will use thermal energy storage. They plan on depositing SiC onto the reactor using atomic layer deposition. Given the proposed size of the reactors, they need to present compelling evidence (examples) that atomic layer deposition (ALD) can be economically done on a system of this size, shape, etc.

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 project has an excellent approach to modeling and extensive laboratory work with clear forward progress in several areas. Given the wide range of barriers addressed, some priority or indication of the primary barriers was expected, as well as detailed progress showing the scale and effect of those improvements.
- The modeling of the multi-phase reactor is a good start to show the viability of the approach. It is not clear where this reactor is in the concept shown on slide 5. The materials require extremely high temperatures (1500°C) for long periods of time (60 minutes). This seems like it may be a problem for the system. The targeted cycle time is not clearly defined and needs to be defined. The researchers have screened many materials using theory. They have identified around 15 materials for further testing. The results of the testing will be interesting. It is unclear whether the coating of the very large reactors using ALD is economically feasible; it is not clear whether there are examples of this. In fiscal year (FY) 2015, the heat exchanger was a major emphasis; in FY 2016, this was essentially not mentioned. The reason for this was unclear. Solid–solid heat exchangers are very challenging, especially at the large flow rates required. The researchers indicated that particle size is important and the desired size is 1 mm. In their fluidized bed system, it is likely that attrition will occur. It is not clear whether they are testing their materials' durability under the very challenging conditions they will be operating. The principal investigator said that the cycle time was 40 minutes to oxidize and 40 minutes to reduce for a cycle of 1 hour 20 minutes (though slide 17 shows a cycle time of 1.5 hours). Using the average production (482 micromoles hydrogen/gram, slide 17) and assuming 50,000 kg hydrogen production per day, this means the system is moving approximately 2,900 tonnes of material per hour. This seems like a lot of solid material to move. If the cycle time can be decreased to 15 minutes, the amount of material decreases to ~540 tonnes. It is recommended that the researchers improve the kinetics to decrease the cycle time.
- The comparison evaluation of vacuum cascade and recycled inert gas sweep was interesting. It was not clear that the transport membrane for inert gas sweep would be capable of performing at the temperatures required or would be available for future scale-up testing. Lower reduction temperatures may be needed for use of inert gas sweep (if that is selected over the vacuum cascade method). Significant progress was made in modeling for coating stability of barrier material coatings on SiC, with candidate materials identified, and in synthesis of spherical particles of reactor materials. Materials discovery work continues to make progress and yield interesting results. The comparison between Sr- and Mn-doped $LaAlO_3$ (SLMA) perovskite, hercynite spinel, and ceria was interesting. A more detailed analysis of relative advantages of these classes of materials and the prioritization of peak production rate, cycle time, and production for system efficiency and production volume would be helpful.
- The H2A analysis in 2015 indicated that heat exchanger effectiveness was by far the most critical factor in the final economics. This technical challenge seems to not have received attention. Furthermore, its impact on the economics no longer appears in the H2A tornado diagram. This should be explained.

Question 3: Collaboration and coordination with other institutions

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

- The synergy and interactions between the Office of Energy Efficiency and Renewable Energy (EERE) project team (University of Colorado and NREL) and the National Science Foundation (NSF)/DOE project team are excellent. The participation of CoorsTek, Inc./Ceramatec, Inc., in the project provides valuable industrial input into development and vetting of reactor system components.
- There is a good balance of national laboratories, companies, and foreign collaborators.
- Several new collaborators appear to have been added from previous years, addressing concerns about having large-scale engineering companies involved in the project.
- They have added some collaborators (CoorsTek, Inc./Ceramatec, Inc.) that are good additions to the team.

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 supports the EERE Fuel Cell Technologies Office (FCTO) goal of reducing the cost of hydrogen production from renewable resources to <$2/kg and the objectives of verifying the competitive potential for solar thermochemical cycles for hydrogen in the long term and of developing this technology to produce hydrogen by 2020. Meeting the cost goal will depend on the successful completion of this project and on technology development beyond the scope of the project and FTCO, e.g., lowering the cost of the heliostats to the DOE Solar Program SunShot target.
- The project directly supports the plans and targets of the Multi-Year Research, Development, and Demonstration Plan.
- This technology is in its infant stage, so it has potential to make an impact, but until it is further developed, the impact is unclear. The system is extremely complicated and still has many technical challenges to be solved before its impact can be assessed. Renewable hydrogen production from water splitting is relevant.
- It is not convincing that this technology has a reasonable potential to achieve $2/kg hydrogen. Multiple breakthroughs are required (e.g., materials, reactor design, and thermal management).

Question 5: Proposed future work

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

- All future work is in line with project barriers and goals. There are a great many barriers and issues to be addressed. Sufficient time and expertise will be required to complete all of these tasks.
- The project team has outlined future work in reactive and containment materials development, reactor design, and modeling and prediction in a logical manner, with milestones and go/no-go decision points identified. In the future, activities and plans for scale-up of fabrication technologies to apply materials of containment coating to full-size components should be addressed. Also of interest would be information on heat recuperation/recovery requirements for an inert gas sweep system (if that is what is selected) and heat exchanger requirements necessary for reactor success.
- It would to beneficial to see a Gantt chart describing the timeline that will achieve on-sun production in 19 months (before the end of the project).
- The researchers need to reduce the cycle time and the average production of their materials in order to decrease the solids handling required.

Project strengths:

- The project has an experienced team collaborating and leveraging resources from DOE and NSF. Excellent work has been performed on the development and production of materials of reaction and construction.
- Strengths are the extensive collaborative teams and the strong experimental and modeling work.

- The project's vision and collaboration are strengths.

Project weaknesses:

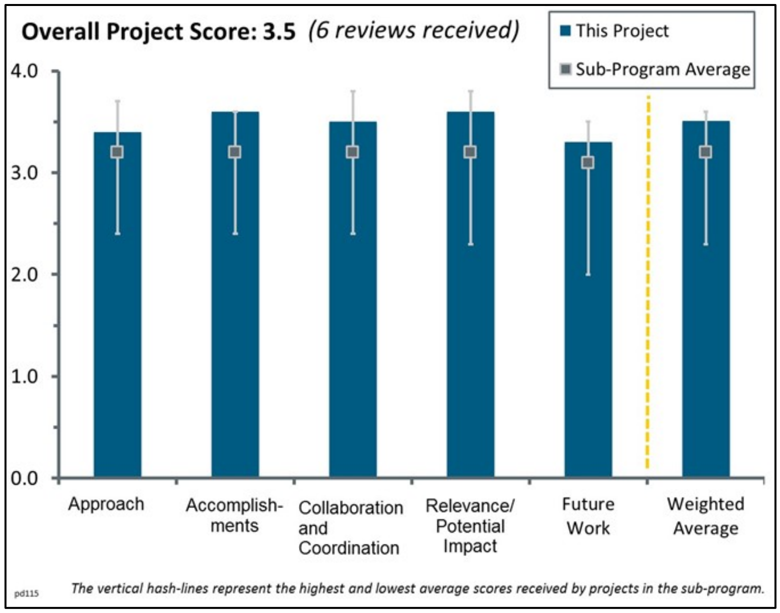
- The presenter would benefit from describing the connection of the disparate parts of this research in such a way as to give a complete understanding of the system and its operation. This information would be best presented early in the discussion. More H2A details indicating what the key baseline parameter values are should be provided. There are a significant number of barriers to address in this project. Further work to examine and report the details of the particle fluid dynamics is encouraged.
- Breadth and depth are project weaknesses.
- No schedule or strategy was discussed for meeting project requirements in the time remaining.

Recommendations for additions/deletions to project scope:

- The project should engage the Saudi Basic Industries Corporation (SABIC) more and get its perspective on the challenges associated with very large high-temperature moving bed reactors.
- The project should encourage the materials community involved in high-throughput computational materials screening to coordinate and collaborate on establishing materials discovery approaches, testing protocols, and reporting standards. The project should also identify an eventual downselect strategy for materials of reaction.
- The project should continue to update H2A analysis of hydrogen production costs. The project should identify an overall solar-to-hydrogen efficiency metric for a given reactor system/materials combination and compare the efficiency of the reactor system with photoelectric-powered water electrolysis and high-temperature solid oxide electrolyzer cell electrolysis.

Project #PD-115: High-Efficiency Tandem Absorbers for Economical Solar Hydrogen Production

Todd Deutsch; National Renewable Energy Laboratory



Brief Summary of Project:

The long-term objective of this project is to develop highly efficient, durable photoelectrochemical (PEC) reactors that can operate under moderate solar concentration and generate renewable hydrogen for less than $2/kg from PEC water splitting. The objectives for the current year are to (1) push boundaries on achievable semiconductor PEC solar-to-hydrogen (STH) efficiencies, (2) benchmark STH efficiencies for multi-junction (tandem) PEC devices, and (3) improve material durability through approaches such as stabilizing surface modifications.

Question 1: Approach to performing the work

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

- Barriers are addressed with careful analysis of reported efficiencies (as published in *Energy & Environmental Science*) as well as effects due to optical absorption by water. The project is well designed and includes the use of inverted metamorphic multijunction (IMM) cells and graded junctions so that lattice matching is not required between the components in the tandem design (which is included in a non-provisional patent). The project is feasible, assuming that in time the expense of III-V compounds drops greatly. This work is highly integrated with other efforts as noted on the Energy Materials Network slide and extensive collaborations list.
- It is a successful tandem junction solar cell approach based on an IMM cell of $GalnP_2$/InGaAs. The investigators grew a very high-efficiency tandem junction that produced a high-voltage output that drove the electrolyzer. In summary, it is a successful engineering device.
- The approach is a reasonable response to DOE objectives. The project continues to exploit the National Renewable Energy Laboratory's (NREL's) leadership in the III-V semiconductor field. Techno-economic analysis aside, deterioration of the photoelectrodes has always been the Achilles heel of semiconductor photoelectrochemistry, so the project should pay more attention to it.
- The approach to the project and tasks within are clearly planned out in a manner to achieve Hydrogen and Fuel Cells Program milestones and targets. Partner involvement is clearly defined, and the pathways to addressing specific targets are well addressed.
- This approach is feasible only if solar concentration can be used. The principal investigator (PI) stated, "Solar concentration is the major lever." The approach is based on the assumption of scaling solar intensity by 100x to meet cost goals to reduce material usage. To this point in time, the project has focused on other aspects than materials suitability at required solar concentration. This could become a significant roadblock to success if unanticipated barriers are encountered that require more than engineering solutions.

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.

- There was huge progress against DOE performance indicators with all milestones completed or on-track for completion. There was a series of recent world-record STH efficiencies using the buried-junction concept and IMM cells to alleviate lattice-matching problems.
- The project has shown significant improvements in STH efficiency per DOE targets. No guidance is given for the duration of the test or ability to sustain this efficiency.
- Modeling efforts have provided a realistic view into how the PEC water electrolysis cell of the future will be configured. Several insights and developments, such as electrolyte depth, back reflective contact, concentrated sunlight, circulation of electrolyte, and IMM, have all gone together to make >20% efficiency cells appear to be in the realm of possibility.
- The 16.3% efficiency is indeed impressive, and it shows the soundness of the approach. However, this world record should be considered along with durability. It is unclear whether stabilization of the p-$GalnP_2$ surface for 60 hours through MoS_2 is a successful milestone or a promising future approach. Further, the prospects of improving the efficiency of the device are unclear. It seems that the team needs a better junction to do that; it is unclear whether there is a plan for this.

Question 3: Collaboration and coordination with other institutions

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

- NREL has put together a good team with the University of Nevada, Las Vegas (UNLV); Lawrence Livermore National Laboratory (LLNL); the University of Hawaii; Stanford University; and others.
- The project partners and their involvement are shown as highly engaged. Tasks are segmented to appropriately leverage the strengths of each contributing institution.
- The large group of engaged collaborators establishes the credibility of these results.
- This work is highly collaborative.
- It seems that UNLV is an important contributor to the project, but its role should be better defined through a well-thought-out plan, i.e., how microstructural characterization can help the project achieve higher efficiencies. The way the collaboration was presented made it seem that materials are chosen first and then tested at UNLV. For instance, on slide 24 it is stated that new materials will be investigated with no reference as to how the new materials will be chosen in relation to promising microstructural features.

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 project is extremely relevant and provides a sound approach toward achieving the DOE targets.
- DOE has many goals and objectives, and it is hard for any one project to satisfy all of them. At some level, PEC hydrogen has to be compared with all other means of producing hydrogen using renewable energy.
- This work is potentially impactful and supports great progress toward the Multi-Year Research, Development, and Demonstration Plan targets given the series of new reported world records (16.3% STH, not on slides); addition of the window layer, which is new; etc. However, it is unclear whether this technology path will prove fruitful because of costs.
- The project meets or approaches near-term efficiency targets. Durability is still nonexistent. It is difficult to see how $/kg targets are achieved with no method demonstrated that shows an ability to incrementally show progress toward increased run time.

Question 5: Proposed future work

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

- The future work is planned in a logical manner with decision points including the use of ambient pressure x-ray photo-electron spectroscopy (XPS) built by the Heske Group at UNLV and GW approximation theory measurements with the Ogitsu Group at LLNL. However, there are some significant barriers to the goals, including (1) the cost of the wafer in the context of the new techno-economic analysis, (2) champion electrodes have been hard to reproduce, (3) the need for a thin water layer, and (4) the mention that 10x concentration resulted in >100 ºC operation for 12 hours per year from JCAP-North work, and so one cannot likely go much higher in optical concentration. Notwithstanding, the durability is being addressed and alternative pathways to mitigate risk are present, including down-selected materials, metamorphic geometry, protection layers with the Jaramillo Group at Stanford, and investigation of work using increased pressure to mitigate bubble formation.
- Future efforts are detailed well with focus on continued improvement toward DOE targets. Barriers are acknowledged, but the specifics of what are required to meet the $/kg targets are still significantly far away on all aspects.
- It is good that durability will finally be getting more emphasis. The various modeling exercises where the lifetime of an array is put at 5–20 years are somewhat disconnected from where the technology presently is (which seems to be hundreds of hours).
- Materials and device lifetime studies of solar concentration were raised as a need in the previous year’s review. The reliance on one class of materials is a potential weakness.
- The proposed work is directed to efficiencies greater than 15% and durability over 875 hours. Although this year’s accomplishments ensure better results, it is not clear that the suggested three encapsulation pathways (see bottom of slide 24) will yield better durabilities. In other words, the mechanism-based approach to understanding degradation is missing.

Project strengths:

- NREL and collaborators demonstrate unrivaled competency in developing $GaInP_2$-based multijunction photoelectrodes and likely PEC water-splitting technology in general.
- The group uses capillary mass spectrometry to evaluate the 1:2 ratio of hydrogen to oxygen and aims to grow materials on silicon substrate to mitigate some of cost because the current substrate is prohibitively expensive.
- Progress on multiple fronts has been made. The new world record for solar PEC efficiency is 16.3%.
- The project has come up with a world-record STH efficiency, which is excellent.
- The collaborative nature of the project and contributions from UNLV and LLNL are strengths.
- The team and focus on fundamental research are strengths. Detailed plans for current and future work are strengths.

Project weaknesses:

- It would be essential to determine what is the actual device (or interfacial) lifetime of materials at the full (100x) solar concentration intensity stated to be essential for meeting deliverable STH. It is unclear whether there is a different intensity inactivation curve for each material. It is unclear whether it is interface specific. It is unclear whether the lifetime is predictable from fundamental physical and chemical properties of materials. The decision to eliminate the (lower cost) nitrides materials subtask because of inadequate performance further reinforces the dangerous reliance on one class of ternary III-IV materials, which has not been tested at 10x or 100x solar intensities.
- In the broad scheme of things, it continues to look like a very expensive way to produce renewable hydrogen. Setting efficiency records is good in that it captures headlines, a non-trivial consideration when funds are being distributed among disparate agency branches and technologies. Nonetheless, such accomplishments will invariably be followed by demands for scaled-up demonstration projects. It is unclear how much longer NREL will be allowed to work with 0.25 cm^2 electrodes.

- The high cost of material prohibits use of significant material for stability tests under 10x and larger optical concentration. Also, more needs to be known about stability, which is part of the scope of work.
- The project still has not addressed the photocorrosion aspect, which is critical to the progress.
- The lack of a mechanism-based approach to understanding photocorrosion is a weakness. It is unclear why the electrolyte is in contact with the tandem junction.
- A weakness is the project's inability to show any durability for this technology.

Recommendations for additions/deletions to project scope:

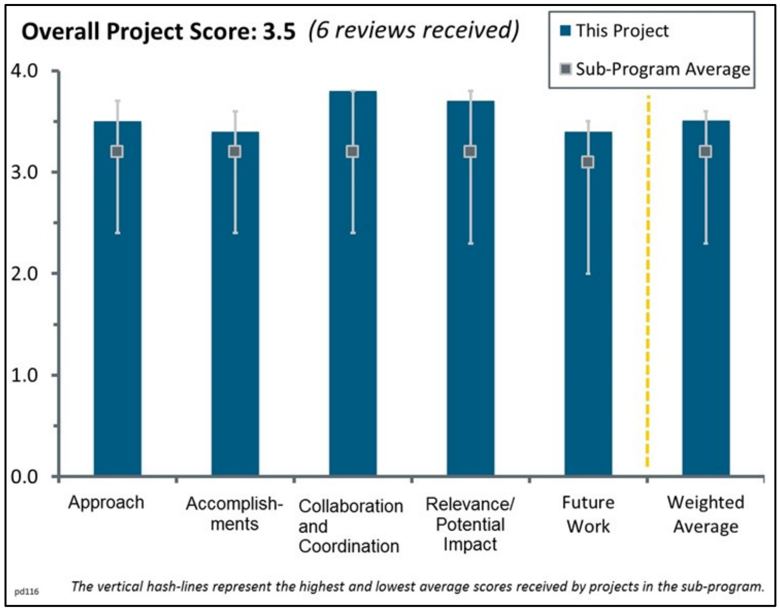
- The entire PEC hydrogen research area needs to carve out a niche for itself that defines a possible set of parameters in the future world energy economy that would make photoelectrolysis the preferable approach to renewable hydrogen. In other words, PEC hydrogen has to elbow its way between hydroelectric- and wind-powered electrolysis, maybe even certain types of biomass conversion, and ultimately solid-state photovoltaic arrays coupled to dark electrolysis. All of these technologies can be done presently on a massive scale if a demand for renewable hydrogen were to arise.
- The project should use a voltmeter and an ammeter to assess the actual short-circuit condition.
- The project will benefit if the PIs put more focus on the lifetime of the device at this time.
- It is stated that the approach used minimizes interface defects, but no proof was given. However, it is stated on slide 15 that this task will be undertaken with UNLV in the near future.

Project #PD-116: Wide-Bandgap Chalcopyrite Photoelectrodes for Direct Solar Water Splitting

Nicolas Gaillard; University of Hawaii

Brief Summary of Project:

The long-term objective of this project is to identify efficient and durable copper chalcopyrite-based materials that can operate under moderate solar concentration and are capable of generating hydrogen via photoelectrochemical water splitting at a cost of $2 per kilogram or less. The Hawaii Natural Energy Institute (HNEI) will (1) develop new wide-bandgap (>1.7 eV) copper chalcopyrites compatible with the hybrid photoelectrode design, (2) demonstrate at least 15% solar-to-hydrogen efficiency, and (3) generate 3 liters of hydrogen under 10 times concentration in 8 hours.



Question 1: Approach to performing the work

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

- The project addresses barriers, including the need to widen bandgap to make a tandem using a sulfur/selenium mixed material with single phase by X-ray diffraction (XRD). The University of Nevada, Las Vegas (UNLV) identified an oxygen vacancy and they are exploring if this is a major issue. The project is well-designed using photovoltaic (PV)-grade materials that are bandgap tunable with cost-effective processing. The project is feasible and the techno-economics are promising. The group accomplished the go/no-go decision for year 1 already, via four pathways (three different materials; one of them measured using outdoor testing in Hawaii and got 0.6 V (V*oc*) and decent fill factor with rather long-term stability). This work is integrated with other efforts as it is largely collaborative, including with the National Renewable Energy Laboratory (NREL) and the University of California, Irvine (two other presenters of photoelectrochemical [PEC] work at the Annual Merit Review [AMR]).
- The project goal is to identify low-cost materials for tunable bandgap absorbers and investigate their suitability for preparation of photoanode and photocathode interfaces using catalysts and/or passivation of the electrolyte interface. Copper chalcopyrites and copper indium gallium (di)selenide (CIGS) alloys will be evaluated for optical absorption properties of relevance to use in PEC tandem cells.
- CIGS has been around for some time but the investigators have found some productive avenues to pursue. Widening the bandgap with the idea of eventually producing a tandem device is worthwhile. They are using the same partners as NREL to modify the electrode surface to slow deterioration.
- Efforts appear to be well-coordinated and supported by a solid team that has done a good job of addressing barriers. Progress on a durability barrier is still way behind and it is difficult to see that any success will be accomplished here since 2015 milestones are still only 25% complete.
- The approach aims at PEC water splitting at low cost and solar-to-hydrogen (STH) efficiencies greater than 10% by using wide-bandgap copper chalcopyrite based materials. The approach is focused more on the photovoltaics aspects such as optical absorption than surface chemistry and catalysis, which are given less attention. It was also stated that the Materials Genome Initiative will guide the choice of new wide band gap materials and then be tested at UNLV, but it was not shown how the process of down-selection will take place.

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 against performance indicators is excellent including meeting the go/no-go decision in several ways. It was unclear which demonstrations used buried junctions, PV-bias, and electrocatalysts (and what they were); this should be made clearer next year. This work is also demonstrating progress toward DOE goals by notably working on the protection issue.
- The principal investigator (PI) has synthesized a range of copper chalcopyrites and CIGS alloys and evaluated optical absorption properties of relevance for use in PEC tandem cells. They have identified compositional requirements for achieving wider spectral range and met appropriate goals. The team has moved on to examine how to stabilize the interfaces and how to avoid interfacial problems. Data appears to show identical sub-band-gap absorbance and scattering for a wide range of different materials. Actual absorbance spectra appear to have been normalized (no data for absorbance [Abs] were given). Impurities were identified in the synthesis and work towards removal has progressed. Heterogeneity at the compositional level may contribute to spectral band gap structure and to sub-band gap absorbance scattering and will be investigated.
- The evolution of bandgaps presented on slide 8 over the duration of the project shows cohesion and concerted effort. The photocurrent density of 10 mA/cm^2 seems to be a promising result. Oxygen and sulfur identification of impurities by X-ray photoelectron spectroscopy (XPS) (see slide 10) is an important accomplishment but it was not mentioned how this information will be used to achieve better efficiencies or even guide choice of new materials. Studies on corrosion resistance are significant (see slides 14 and 15) but again they should focus on mechanistic understanding rather than on observational conclusions.
- Manufacturing and efficiency targets are being met. It is difficult to see a pathway to achieving durability targets within this program since the current trajectory looks like another miss in 2016. All other goals look to be in a good position to be met for the balance of the program.
- Gallium, aluminum, and sulfur have been identified as potential dopants to adjust (widen) bandgap. Most of the work was done on sulfur, which has been used in that regard on copper indium selenide PV cells for some time. Nonetheless, HNEI continues to accrue improvements in open circuit voltage and conversion efficiency. Use of MoS2 and Pt-TiO2 as surface modifiers provided significant improvements in durability although there's a long way to go to reach DOE cost targets.

Question 3: Collaboration and coordination with other institutions

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

- Team interaction appears to be well-managed. Contributors are being leveraged well to play to their strengths. This has been key in meeting the majority of targets thoroughly and in a timely manner.
- This project has a huge list of active and influential collaborators.
- The project demonstrates effective collaboration among partners in identifying contaminants and preventing electrode corrosion.
- The project enjoys a wide range of effective collaborations.
- It seems that UNLV is an important contributor to the project, but its role should be better defined through a well thought out plan, i.e. how microstructural characterization can help the project achieve higher efficiencies. The way the collaboration was presented, it seems that materials are chosen first and tested at UNLV next. Perhaps the collaboration between UNLV and Stanford can advance the identification of the source of impurities at the interface.

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 work is in alignment with the goal of producing cost-reduced hydrogen efficiently. The goal of producing a cheaper chalcopyrite material achieved and provides a path to meeting long-term goals.
- The work is excellent and complementary to the NREL work.
- Project relevance fits well within the scope of the entire PEC program; however, the reviewer is concerned about the fit of the PEC program with DOE hydrogen.
- It is an engineering-driven project that would have more impact if there was more focus on catalysis and surface chemistry.

Question 5: Proposed future work

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

- The future work is planned in a logical manner with decision points but, as in any PEC approach, there are many barriers to the goals, which are addressed directly (i.e., bandgap, purity, stability, and benchmarking). The group has also explored several alternative pathways to mitigate risk, e.g. the Ogitsu Group at LLNL did modeling to identify a window layer that has a good energetic match and identified ZnOS; corrosion protection with Jaramillo Group at Stanford because they had done physical vapor deposition, (PVD) but this material is rough and so now they are doing MoS2 by atomic layer deposition (ALD) and demonstrated a doubling of the lifetime (they also looked at Pt/TiO2 [50 nm] and got 250 hours stability)
- Intriguing and innovative work with dopants, surface modifiers, and tandem cell fabrication was outlined.
- Proposed future work is set up to address project barriers.
- Although a solid approach toward future developments is described, the specifics of the required fundamental science are missing. For instance, on slide 19 it is stated that the optimum protective material for durability will be identified but no specific pathway is outlined.

Project strengths:

- The project represents a good set of back-up photoelectrode materials should the III-V semiconductor tandem cell work become stymied for some reason. The chalcopyrites might actually have a better chance of reaching long-term cost goals for renewable hydrogen.
- The project demonstrates excellent recent progress.
- The project presents well thought-out, systematic study on the variation in the elemental composition of the film.
- The collaborative nature of the project and contributions from UNLV and Lawrence Livermore National Laboratory are strengths.
- The project enjoys strong team collaboration.
- The project has an excellent team integrated well within the user community.

Project weaknesses:

- This is significantly farther down the power curve (literally) than the GaInP2 photoelectrodes. The lower efficiency may cancel out the advantage of lower material cost.
- The work has too much emphasis on efficiency. This does not mean a lot if you can't get the device to run for any reasonable period of time.
- The project does not address whether the proposed MoS2 or TiO2 films could provide long-term protection even when pin holes are removed.
- There is a lack of a mechanism-based approach to understanding bottlenecks.

Recommendations for additions/deletions to project scope:

- Oxygen was found in the bulk of the selenium/sulfur semiconductor. Its origin should be identified and its concentration controlled. Its presence in small amounts might be beneficial.
- The project will benefit from more coordination on fundamental science between UNLV, Stanford, and LLNL. The below answer by the project PI to reviewers' comments on page 17 indicates that the focus of the project needs to be expanded:

  > *Our project aims to develop wide bandgap chalcopyrite photocathodes. A mechanical stack approach will be used to pair these electrodes with existing high efficiency PV drivers to form a complete high-pressure electrolysis device (proof of concept). However, our technoeconomic analysis indicates that this approach is not economical for large scale PEC hydrogen production. To be economically viable, a commercial device should be made of two absorbers monolithically integrated on the same substrate, with hydrogen and oxygen evolved on opposite sides of the device. For this reason, our team has chosen to study some key components of the monolithic structure (e.g. In2O3:Mo as intermediate transparent window layers) to identify possible pitfalls.*

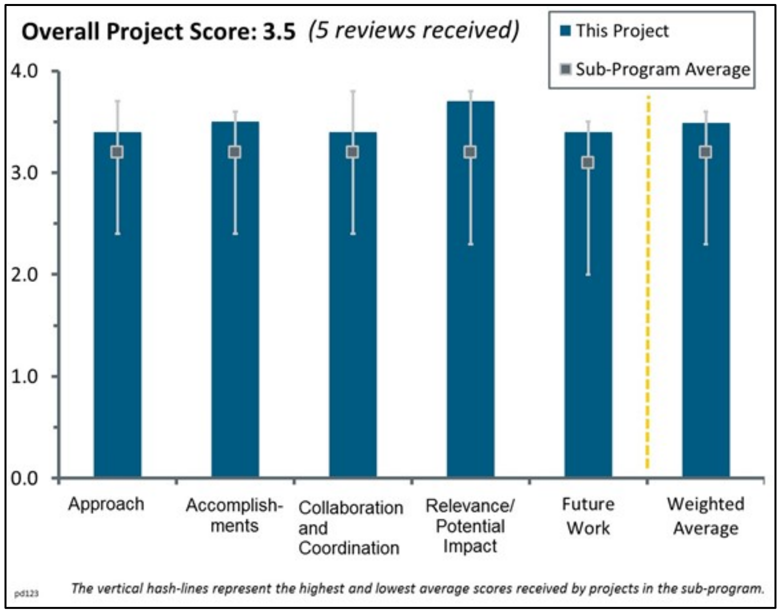
  Indeed, this answer shows that the project is mainly focused on PV and its direct link to hydrogen production is not clear.

Project #PD-123: High-Performance Platinum-Group-Metal-Free Membrane Electrode Assemblies through Control of Interfacial Processes

Katherine Ayers; Proton OnSite

Brief Summary of Project:

Anion-exchange-membrane (AEM)-based electrolysis enables the elimination of most expensive cell materials. This project will further AEM electrolysis through three objectives: (1) demonstrate feasibility of non-platinum-group-metal (non-PGM) catalysts in AEMs, (2) enhance membrane and ionomer stability to achieve long-term cell operation, and (3) demonstrate 500 hours of stable operation at <2 V for a fully integrated AEM cell at 500 mA/cm^2.



Question 1: Approach to performing the work

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

- Barriers are addressed, including increased polyphenylene oxide (PPO) membrane stability through the use of hydrocarbon cation spacers to decrease benzyl attack. This cannot withstand hot press conditions like cation exchange membranes, so spray deposition of catalysts on the membrane was used to circumvent this. This is a well-designed project, especially given that electrolysis is the most mature technology to bridge hydrogen to market. The project is feasible using Ni-based oxygen evolution reaction (OER) and hydrogen evolution reaction (HER) electrocatalysts, AEMs, and stainless steel flow fields, which are cheaper when at scale than polymer electrolyte membrane (PEM) Ti. This work is integrated with other efforts at Proton OnSite and collaborators at Northeastern University, Pennsylvania State University (Penn State), and the University of New Mexico.
- The project addresses barriers of electrolyzer cost, durability, and performance. Cost is addressed by investigating PGM-free catalysts for HER and OER in alkaline-exchange membrane electrolyzers. Durability is addressed by enhancing alkaline membrane durability. Though Los Alamos National Laboratory has shown aryl ether backbones can be susceptible to ether cleavage, the dimethyl substituted aryl ethers pursued here should be kinetically stabilized because of steric hindrance provided by the methyl groups. The choice of longer side chains for the ammonium group provides greater hydroxide stability. The sacrificial support method should enable a porous electrode with efficient mass transport. The approach to address catalyst, membrane, and ionomer should lead to an optimized MEA.
- This project has a structured approach that should lead to good results. The team is also working on novel tunable membranes for use in non-PGM electrolysis systems. Technical targets seem to focus solely on catalyst activity without paying much attention to durability. To make a viable system, both are needed. That the addition of carbonate to the system has such a profound and unexplained effect is a bit of a concern. This issue warrants more attention.
- The project is following a logical approach. Alkaline electrolyzers are lower-capital-cost; however, they are lower-efficiency. This means that this technology may be applicable for smaller systems in which capital costs dominate, not the larger stations in which electricity costs dominate. It would be good if the project identified its target market for the project’s applications. The project should include analysis conducted using the Hydrogen Analysis (H2A) model to be able to track the impact of the project work. The project needs to include work on more traditional baseline catalysts to show improvement.

- It is not clear how choices were made for particular catalyst and polymer membrane compositions. Addition of potassium carbonate has a huge impact on membrane electrode assembly (MEA) testing, but there is no apparent plan to probe why.

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.

- While IrO_x is not a great OER catalyst in base, the new Ni-based OER electrocatalysts had similar surface areas, electrical conductivities, and OER onsets to IrO_x (a decent standard). The project also demonstrated progress toward DOE goals including membrane stabilization (and 400 mV better performance) with use of carbonate and demonstrated five-year stability at 2 V, including addition of cross-linking in the AEM to enhance performance and properties.
- Progress has been excellent so far in the year since this project launched. The team has already down-selected OER and HER catalysts based on technical milestones and is progressing on membrane stability.
- The project has identified PGM-free catalysts with OER activity of 20 mA/cm^2 at <1.55 V with 0.1 M KOH. HER catalysts with less than 200 mV overpotential at 20 mA/cm^2 have also been identified. The project has shown a full PGM-free electrolyzer cell with stable operation at 500 mA/cm^2 for five hours. On slide 10, the project states it will use ammonium groups with longer spacers to improve stability, but the cross-linked structures utilized have the ammonium group directly attached to the backbone, which should result in ammonium groups, and the data shown on slide 26 show problems with stability. Stability is still an issue.
- The initial performance tests are very interesting. The durability tests will be useful to determine whether the catalyst and membrane performance is good enough. The PPO-MEA + catalyst tests do not show good stability, even at the low current density. However, it is very difficult to determine from the data whether the degradation is due to the catalyst or the membrane. It is hard to determine the accomplishment significance since there is no associated H2A analysis to show progress on reducing the cost.
- Heavy use of acronyms and industry parameters made it difficult for a non-expert to critically assess.

Question 3: Collaboration and coordination with other institutions

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

- The team appears tightly integrated and focused on results. Penn State is working on the ionomer, with the University of New Mexico working on high-surface-area fabrication of catalysts developed at Northeastern University.
- The project is highly collaborative.
- The team is strong.
- There is a nice set of academic collaborators, each with clear responsibilities, but it is not clear how integrated the academic teams are with each other or with Proton OnSite.
- Collaboration among partners is good. All partners have an active role. Collaborations with those outside the project are not apparent.

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.

- In order to see hydrogen production at large scale, it will be necessary to reduce the capital cost of electrolyzers significantly. Developing non-PGM systems is critical to that goal. This project, though focused initially on laboratory-scale systems, could have the potential for scale-up to industrial sizes.
- Alkaline electrolyzers have a lower capital cost than PEM electrolyzers, so companies that are very concerned with initial capital costs may prefer this approach. In addition, for lower production rates, capital cost may surpass electricity costs for the largest part of the hydrogen cost.

- Proton OnSite is the leader in PEM electrolyzers for hydrogen and is now taking the same prominent position in the AEM electrolyzer space. Progress is excellent.
- It is appropriate for DOE to support an AEM alternative to PEM through both fundamental and applied research. The project has a good set of quantified goals.
- Alkaline electrolyzers have the potential to significantly reduce the capital expenditures for electrolyzers. The project addresses major cost components for alkaline membrane electrolyzers.

Question 5: Proposed future work

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

- The planned future work is logical with decision points (some materials were already down-selected), including scale-up of cross-linked PPO membranes, mechanical reinforcement to improve durability of membrane and water transport rate and PPO with >25 mS/cm, tests on effects of carbonate (K_2CO_3) using half-cell and full-cell electrochemical impedance spectroscopy (EIS)measurements, and catalyst performance and interfacial stability.
- The project has a clear timeline for addressing milestones.
- It is recommended that two components be added to the future work. (1) The project should add a lifecycle cost assessment evaluating the trade-off in life-cycle cost between capital outlay and system efficiency. The motivation for this analysis is that a non-PGM system is likely to be less efficient than a PGM system. On a life-cycle basis, the cost of electricity is the most important factor in the cost of hydrogen. This consumption is directly affected by efficiency. The question is how much efficiency loss capital cost reduction can buy. At several reviews, the principal investigator has said that her customers care about capital outlay, but it is not clear to what extent. (2) The effect of carbonate on the system is profound. It is important to understand the mechanism by which carbonate improves the system stability and performance. It is unknown whether the addition of carbonate is viable in the long term. Is it not certain that carbonate can be engineered out.
- The plan to perform more durability testing is good. The team needs to include cycle testing as well as lifetime. It is recommended that the project compare its data with some more traditional baseline alkaline electrolyzer catalyst data (such as a Ni catalyst).
- The proposed work addresses mass transport in the electrolyte layer, and optimizing the hydrophobic properties is needed. Transport issues are likely to be important.

Project strengths:

- The project is actively focused on capital cost reduction from the ground up. The team is taking an almost blank slate approach to what it means to build an electrolyzer.
- Alkaline electrolyzers have clear entry points into the market with increased materials development and could conceivably be plug-in replacements into PEM cells/stacks.
- The project has a good team and is doing a thoughtful, methodical approach.
- Proton OnSite is a leader in the electrolysis field.

Project weaknesses:

- The approach may not be helpful for DOE to achieve its long-term, high-volume production (>1000 kg hydrogen/day) cost targets.
- There are questions about carbonate.

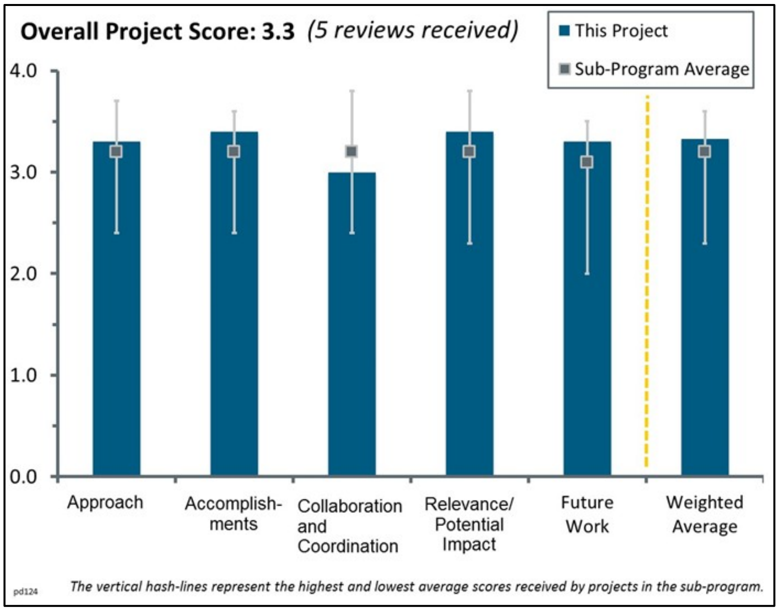
Recommendations for additions/deletions to project scope:

- It would be interesting to see H2A model analysis at lower hydrogen production levels, such as 100 kg/day. There are some very old H2A model analyses at this production capacity that can be considered. The project should consider testing against baselines other than Ir. The required cost for the carbonate addition is unknown.

Project #PD-124: Solid-Oxide-Based Electrolysis and Stack Technology with Ultra-High Electrolysis Current Density (>3A/cm^2) and Efficiency

Randy Petri; Versa Power Systems

Brief Summary of Project:



This project is researching and developing solid oxide electrolyzer cell (SOEC) technology capable of operating at ultra-high current density with reasonable efficiency. Project objectives are to (1) develop a solid oxide electrolysis cell platform capable of operating with current density up to 4 A/cm^2 at an upper voltage limit of 1.6 V, then demonstrate cell operation with high current density of more than 3 A/cm^2 for 1,000 hours; (2) design a solid oxide electrolysis stack platform capable of operating with 3 A/cm^2 current density cell technology at an upper voltage limit of 1.6 V, then demonstrate stack operation with high current density of more than 2 A/cm^2 for 1,000 hours; and (3) complete a solid oxide electrolyzer process and system design that accommodates the ultra-high operating current density platform.

Question 1: Approach to performing the work

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

- Details on specific approaches were not provided, but the results suggest that the technical approach was solid. There is a more global question, to be addressed in future work, of whether operating the SOEC at high current density is actually preferable. Ultimately, stack size and cost (which are lower at high current density) need to be traded against power requirements (which increase with higher current density) and stack lifetime (which is likely reduced at higher current density). The analysis to be performed by the University of California, Irvine (UC-Irvine) should shed some light on these trade-offs.
- It is not clear why someone would want to operate at >1.6 V. The advantage of SOEC over low-temperature electrolysis is increased efficiency. At the high current and voltage, the efficiency is low. It would make more sense to operate closer to thermoneutral and higher efficiency. The project really needs to do an analysis using the Hydrogen Analysis (H2A) model to show that, should the project be successful, it will reduce the cost.
- The innovation being pursued by this project is unclear, beyond testing existing materials at higher voltage and the design of the appropriate SOEC system.

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.

- Versa Power Systems made considerable progress toward its stated goals. The demonstration of SOEC stack operation at such high current density—with minimal degradation—is an important accomplishment.
- The project appears to be ahead of schedule.
- Getting to >3A/cm^2 in the single-cell testing was impressive. The degradation rate is too high. The project really needs to do longer durability and cycling tests at the high rates. Operating an SOEC at or above

1.6 V causes severe irreversible degradation due to microstructural changes both in electrodes and at the active interfaces. The researchers are seeing that in their system. In addition, they are generating a good deal of excess heat. Their stack model shows a large temperature gradient that will impact stack durability, especially for cycling. The advantage of operating at relatively high voltage is not clear.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- It seems the collaborations were primarily in the past on other projects. The project does not seem to be collaborating with others. It is recommended that the project bring on a partner to aid in the analysis. The Pacific Northwest National Laboratory (PNNL) has developed a lot of solid oxide materials, seals, and coatings and has plenty of experience doing failure analysis. The project should consider reaching out to PNNL. The National Renewable Energy Laboratory (NREL) has done techno-economic analysis for Versa Power Systems in the past and could help the project team understand the value, or lack thereof, of operating at high voltage in terms of hydrogen cost.
- It was not apparent that there has been any collaboration to date on this project (aside from Versa Power Systems' parallel work with Boeing and the Solid State Energy Conversion Alliance), although this could be related to the proposed project plan. It seems likely that this will change in the future since UC-Irvine is tasked to complete a techno-economic analysis. NREL's role on the project was not clear from the presentation—perhaps NREL is supporting the techno-economic analysis.
- It is not clear how NREL is involved or whether other partners are still engaged.

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.

- Solid oxide electrolysis represents an important approach for hydrogen production in water-abundant locations, especially if integrated with "free" sources of power (e.g., wind and solar) or "free" sources of heat and steam (e.g., nuclear power plants).
- Solid oxide electrolyzers have the potential to lower hydrogen cost since they use electricity more efficiently than low-temperature electrolysis.
- It is appropriate for DOE to support SOEC development. There is a good set of quantified technology goals in support of higher-level cost and efficiency goals.

Question 5: Proposed future work

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

- The future work outlined in the presentation is fine. It is recommended that UC-Irvine's techno-economic work be initiated and completed as soon as possible so a decision on the target current density for the SOEC stack can be made. Based on these techno-economic results, the technical targets for the work might be revised. That said, any work that enables higher current density to be achieved with lower degradation will still be germane for SOEC stacks operating at lower current density. Thus, the work proposed toward improving SOEC stack performance need not be delayed.
- The clear project plan and timeline are appreciated. The project should consider engaging academic or national laboratory expertise to develop an approach to stabilizing Ni loss from the cathode.
- The project is doing the techno-economic analysis at the end of the project. The researchers should be doing the analysis now and using it to direct their work. They need to do durability and cycle testing.

Project strengths:

- Versa Power Systems is a world leader in solid oxide fuel cell (SOFC)/SOEC technology. The company has a stack platform with high technical maturity and has demonstrated world-leading SOEC performance. The results obtained to date on this project are outstanding from a technical perspective.
- This is very interesting work, and the project has made good progress.
- The project has made excellent progress.

Project weaknesses:

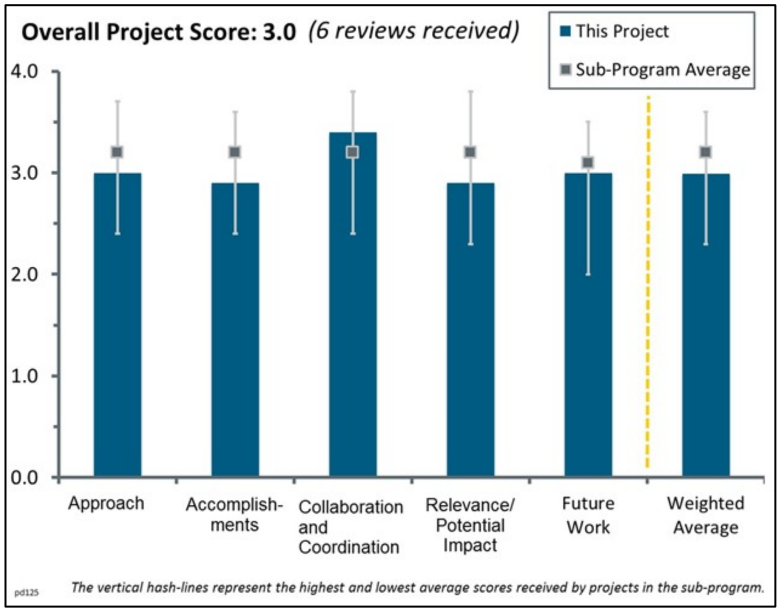
- A techno-economic assessment is needed to determine the optimum current density for SOEC stack operation.

Recommendations for additions/deletions to project scope:

- It is recommended that the project perform durability testing and cycling testing, as well as examine the interconnects for corrosion.

Project #PD-125: Tandem Particle Slurry Batch Reactors for Solar Water Splitting

Shane Ardo; University of California, Irvine



Brief Summary of Project:

This project aims to experimentally validate a laboratory-scale particle suspension reactor as a scalable technology for photoelectrochemical (PEC) hydrogen production. The novel approach entails stacking the two slurry-reactor compartments in series instead of the more typical parallel, side-by-side arrangement to realize the tandem efficiency advantage and shorten the mass transport distance so that fewer pumps and pipes are needed. The project will perform numerical device-physics modeling and simulations of particle-slurry tandem solar reactors as well as design, fabricate, and experimentally test this reactor concept.

Question 1: Approach to performing the work

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

- This type of photoreactor is likely viewed by many as the contrarian approach to solar water splitting, so starting with an increasingly sophisticated modeling effort to show that >1% efficiency is even possible is a worthwhile approach. A good deal of theoretical modeling was done in the early days of photoelectrochemistry, so it is appropriate to do some upfront modeling of particle slurries as well. Actually, these types of systems received much attention as water purification/pollutant removal devices in which the redox transfer agent was instead the targeted pollutant. There may be some chemical/civil/ environmental engineering studies that would be of benefit, at least in terms of what reaction rates have already been achieved. Trying to apply a tandem configuration to particle slurries will not be easy but is novel nevertheless.
- The Type 2 system (suspended particles) has been considered in prior techno-economic analysis evaluations for hydrogen production. This project aims to model the standard configuration and some variations on this configuration. The project's value will be in providing a further assessment of this system's limitations in terms of geometry, plumbing, diffusion, etc. The project could provide further boundaries for defining its possible contribution to a real Type II system if the costs can be achieved. The U.S. Department of Energy (DOE) Office of Energy Efficiency and Renewable Energy (EERE) is fully responsible for selecting projects based on real or anticipated contributions to achievable designs—those with the greatest potential for translation to practical applications.
- The general modeling and experimental approach to evaluating the proposed "new reactor design" of overlaying the bags is fine and should accomplish project objectives. Given that particle (i.e., photocatalyst) development is not a focus of this project, it is unclear how much of the boost in efficiency and projected cost reduction can be attributed to the "new design" and how much would be attributed to having better-performing photocatalysts. (The considerable number of materials contributors suggests that the project is targeting the best available photocatalyst technology, but even that may not be adequate to meet performance targets.) It would be beneficial for judging the potential for this technology to significantly reduce cost if the benefits could be quantified on a "photocatalyst"-independent basis.
- The iterative research and development approach, between computational and experimental, presented by the project lead seems a very effective way to achieve the main goals and objectives of this project.

- This is a “bag approach” aiming at >1% solar-to-hydrogen (STH) efficiencies at reduced cost. However, there are a number of issues that need to be further explored before the approach can be deemed sound and promising. Among these issues are the degree of particle suspension and its effect on quantum yield, as well as the stability of the particles.

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 project lead presented significant progress on this work, including the electrolyte stability demonstration through the project’s modeling efforts, the electrocatalyst deposition through bipolar electrochemistry, and the synthesis and characterization of the photoelectrodes.
- Progress appears to be adequate to date relative to milestones based on the percentage completed reported, especially for a new startup project. However, insufficient information was presented to evaluate whether projected completion percentages were truly representative of whether the milestones were on schedule. For example, the D1.1.1.1 milestone to reduce pipes and pumping energy demand by 80%, which is due in less than two months, is reported to be 50% completed; however, there was no information presented to support this level of projected completion.
- The effort just started late last year, and the calculations successively incorporate more and more dynamic aspects of the ultimate particle slurry configuration. Therefore, at this point, the best that can be said is that the project is making progress, but many more aspects need to be built into the model to make realistic conclusions. There is some concern that the model will become so complicated that simplifying assumptions may have to be made, which will then cast a shadow on the final results. It looks like the project already has candidate oxygen- and hydrogen-evolving particles identified.
- Early-stage results (nine months) are approaching targets.
- The accomplishments so far are only numerical related to transport issues among the various “bags.” It was mentioned that Rh-modified $SrTiO_3$ and $BiVO_4$ were chosen, but this choice seems rather ad hoc with no clearly underlying relation to the milestones and targets of the project.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration on this work is outstanding. The primary project team is collaborating with a significant number of experts in particle material synthesis.
- The project team has found it necessary to go outside the DOE PEC program, even internationally, to get help on some aspects of the work. That shows some initiative on the team’s part.
- Excellent leveraging of expertise is available through the Joint Center for Artificial Photosynthesis; however, it is not clear what impact these collaborations have had to date on advancing the project. There is a wide and diverse range of materials contributors. It is not clear what, if any, impact these contributors will have on the project. The presentation gave the impression that materials development for photocatalysts is not a focus of this project.
- The names of the collaborators and partnering institutions are impressive. However, the presentation did not make clear what the collaborators’ contributions are. For instance, the role of Professor K. Domen in this project could not be identified.
- There are many prospective collaborators, but few real partners were listed. Engagement of others that could offer experience with this Type 2 system would further benefit task completion.

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.

- Solar water splitting could be a potential enabler within the Fuel Cell Technologies Office’s main goals to achieve low-cost renewable hydrogen production.
- It is a worthwhile project, providing some balance within the PEC program. The issue is how the entire PEC program stacks up against all the other approaches to renewable hydrogen.
- This comment is directed at EERE, not the principal investigator. DOE needs to establish its priorities in terms of Type 2, Type 3, Type 4, or other configurations. The funding of research on Type 2 PECs that is likely not to lead to a practical implementation is not advised. EERE is fully responsible for selecting projects based on real or anticipated contributions to achievable designs—those with the greatest potential for translation to practical applications.
- It is not clear what impact the new design concept being pursued will have on enabling tandem particle slurry batch reactors for meeting DOE hydrogen production cost targets, given that the project’s success lies in identifying materials with relative high solar efficiencies, which is outside the scope of this project.
- The project is still at an initial stage. Further developments, in principle experimental, are needed before the relevance of the project can be ascertained.

Question 5: Proposed future work

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

- The proposed experimental work for fiscal year 2017 will be essential for the validation of the modeling work. Future work is very well defined.
- The project is well planned.
- Proposed work is based on meeting project milestones. Task D.1.1.1 is a go/no-go decision point that should demonstrate certain, but not all, proof of concepts related to the new design. It is not clear what the benefit of task M.1.1.3 is—this task leads to an “improved model” by considering more process parameters—especially since it is not clear how the improved model will benefit the experimental development. It is not clear how the project progresses, as a material that operates at a rate consistent with an efficiency of >1% STH cannot be identified.
- Since the project just started up, most of the work statement is future work.
- The approach continues to rely on an incremental addition of device physics to the numerical transport model. This is not a sound approach. The project needs to focus first on how a transparent membrane is going to remain transparent over a long time (not just a few days) or how the particles are going to remain stable over that timeframe, etc.

Project strengths:

- The project provides some balance to the PEC program, where low-cost and likely lower-efficiency STH materials and configurations are employed to eventually produce large-scale photoreactors. The tandem adaptation is a novel idea that may enable a significant jump in efficiency.
- Proposed “stacked bag” reactor design does appear to have some benefits over “side-by-side bag” design in terms of better utilization of solar flux. The benefits in this design with regard to fewer pipes are not as clear.
- The presented theoretical calculations shed a clear light on the project’s feasibility.

Project weaknesses:

- The particle slurry reactor represents a challenging modeling task whose ultimate conclusions may or may not be credible. Most of the experimental work is still ahead.

- It is difficult to evaluate how the results presented to date demonstrate progress toward meeting milestones and deliverables, especially since this is only a two-year project.
- Project weaknesses include a lack of important understanding of particle stability, membrane integrity, and particle quantum yield.
- Feasibility of the efficient ion transport aspect is still not clear.

Recommendations for additions/deletions to project scope:

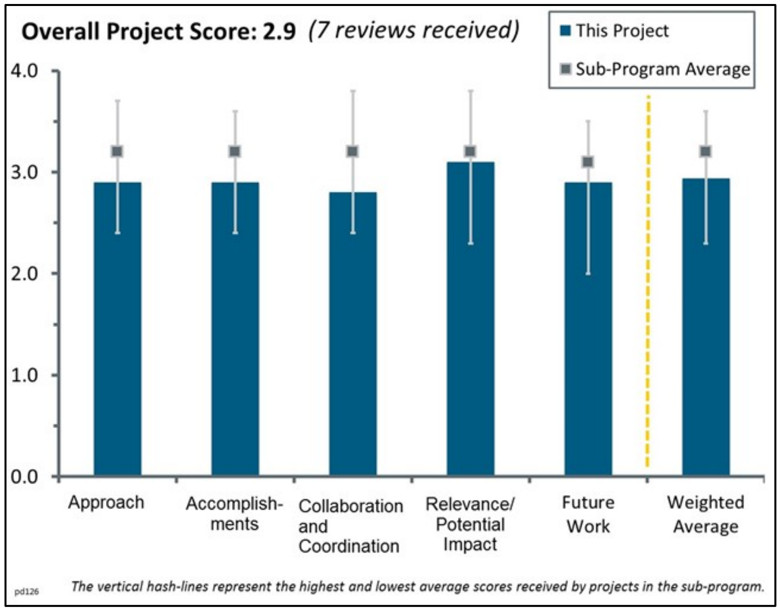
- A reference point for "80% less pipes, 80% less pumping" is needed. If 3 L of hydrogen is a deliverable, then 1.5 L of oxygen should be also—or at least the project should demonstrate somewhere along the way that it is splitting water and making the gases in a 2:1 ratio and not forcing something in the photoreactor to function as a sacrificial reagent. The top layer of the tandem slurry photoreactor will have to be optically transparent to long-wavelength sunlight. Even if the bandgap of the semiconductor is large, there are still scattering effects that have to be minimized to provide any radiation to the lower chamber.
- A comparison of the overall gain in solar efficiency and decrease in parasitic energy consumption for this proposed "stacked bag" vessel compared to the conventional "side-by-side bag" vessel using the same photocatalyst should be conducted. Such a study would provide a basis for evaluating the merit of the proposed reactor design concept. Right now, it seems that identifying/developing a better catalyst would have significantly more impact than developing this new stacked bag design.
- Experiments should be conducted.

Project #PD-126: Compressorless Hydrogen Refueling Station Using Thermal Compression

Kenneth Kriha; Gas Technology Institute

Brief Summary of Project:

This project is demonstrating the technical and economic feasibility of the thermal compression concept for 700 bar hydrogen fueling stations. Compared to conventional liquid hydrogen fueling stations, thermal compression is expected to minimize energy loss and eliminate the need for compressors and refrigeration chillers. Investigators will use modeling to establish and optimize the concept design, evaluate cryogenic pressure vessel options, and compare costs of traditional and proposed stations.



Question 1: Approach to performing the work

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

- The project approach is excellent; the flow chart describing the modeling and demonstration, and the integration are laid out clearly. The project is addressing important barriers, especially the reliability and cost of hydrogen compression.
- The baseline proof of concept for this project is sound and well-thought-out at this stage. Based on the project results, it will influence future work. The testing approach is right-sized and appropriate at this stage of development. The proper steps to achieving this concept were well-thought-out (although practicality is another consideration).
- The technical approach seems comprehensive. However, perhaps a more practical approach would have been to evaluate the economic feasibility of this concept earlier in the project. Granted, the thermal compression would eliminate the compressor; however, the increase in the number and complexity of the pressure vessels seems to indicate very early in the project that demonstrating economic feasibility will be a challenge. The high-pressure storage is among the most expensive items in a hydrogen station. The addition of the cryogenic service to high-pressure storage would be assumed to be more expensive than the existing high-pressure storage today. In the reviewer’s experience, the “smaller-and-more-vessels” approach seen on slide 10 of the presentation typically results in higher costs, not in reducing material costs as claimed. While the elimination of the chilling system would be a cost savings, it provides a means of controlled cooling of the gas. It is not clear how the project team means to control temperature of the hydrogen gas in accordance with SAE J2601 with this system.
- Several major barriers have been identified. The following are some thoughts:
    - The design of an ASME cryogenic vessel capable of the thermal and pressure cycling is mentioned, but this issue should not be minimized since this concept is impossible without that capability. There are major technical hurdles on this item.
    - The amount of piping and valving that needs to be both cryogenic and high-pressure is substantial. It is not clear that this heat leak has been factored into the analysis.
    - There may be several potential hazards that need to be addressed, including potential high-pressure back flow and leakage due to cold-temperature equipment cycling.
- So far it has been mostly modeling, with many test cases and results. However, there is no recommended approach yet, just generalized trends such as desiring more, smaller bottles. At the go/no-go point, there should be one recommended design, and that recommendation should state exactly how much hydrogen is

lost and at what step in the process. It seems likely that anything with more than 10% hydrogen losses will be too expensive. The test demonstration in Period 2 should consider using at least two or three bottles to simulate the cascade operations. It is not clear what the source is for all the thermal energy needed to heat up the hydrogen. Slide 6 could have shown some basic energy balances to verify.

- The approach for this project seems to be disconnected, as it does not seem to capture a good deal of the experience from previous cryo-compressed work at Lawrence Livermore National Laboratory (LLNL) by the same team. A tremendous amount of time was wasted on creating an Excel spreadsheet model that did not function well and had to be recoded in Fortran. Having recoded the model, it seems that the project is making adequate progress but is very much behind schedule. The U.S. Department of Energy (DOE) should reassess this project and possibly tune it toward more of an analysis project, screening the materials and determining an optimum design prior to moving forward toward a demonstration that looks to be in jeopardy of not being completed by May 2017.
- The concept is extremely interesting, but it is too early to determine the validity of the modeling or whether the thermal swings are too challenging for a single cryogenic vessel.

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 project has made excellent accomplishment toward the overall project—the key was developing the model and programming it in Excel/Fortran. The limits of the model variables have been defined, and preliminary data has been generated. Some limitations of the model have also been identified, though the reason has not been described. The project has identified that the vessel cost using ASME-allowed materials will be above the 2020 DOE cost target, while a "low-cost" steel could be a potential candidate. However, in the absence of clear understanding of the properties of the "low-cost" steel, it seems premature to make any decisions. In some instances, the boil-off percentage exceeds 100%, which seems to suggest more hydrogen is boiling off than was originally put into the dewar. It is not clear if the >100% boil-off is a bug in the software, was calculated on a different basis, or has some specific implications.
- A good modeling approach has been made, and software has been developed to design the system. The project's initial first analysis came up with using 25–30 300 L cryo vessels. The following should be first-level questions and considerations: what footprint this would take at a station and whether it would even be possible at any level; whether it would work in terms of operating expenditures (OPEX) without a boil-off recovery compressor, as the boil-off losses are huge and unpredictable (this would be considered from a cost analysis perspective, taking into account the number of vessels); and the fact that, combined with the space consideration, a 700 bar cryo tank design with up to 300 L water volume with *foam* insulation and minimal losses is a *very* long way away (i.e., this is long-term and high-risk).
- The early modeling work and concept layout reflect very encouraging progress. The absence of thermal data associated with cryogen vaporization is baffling. Future reviews need to present a convincing slide that accounts for various ambient conditions. Early economic analysis is interesting, but there is some concern that pre-mature economic evaluation may lead to an early death of the concept.
- The project has made good progress on the modeling but not much progress on the hardware. The hardware is the key challenge and needs most of the additional work. Also, it is not clear that the fills are J2601-compliant since the graphs show non-full fills.
- Not much seems to have been done in the area of reducing the risk of finding suitable materials for these vessels that have so many thermal cycles at high pressure. There should be more specific information than simply citing "low-cost steel." More details should be provided as to how and when ASME will allow these material specifications and for what thermal cycling conditions. Several test cases were run using the model, and sensitivity analysis gives trends on what variables are important.
- No complete vessel designs were described, although several parameters were screened. Tremendous modeling, design, and materials challenges lie ahead for this project, and accomplishments do not seem to support meeting the go/no-go.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- The listed collaborations seem appropriate and relevant. Not much was said about what Royal Dutch Shell has contributed.
- It is a bit unclear who was doing what work. However, all team members seemed to have good knowledge and responded to questions and concerns well.
- It too early to be critical on this point. However, the project needs more collaborators. In particular, the team should collaborate with experts in modeling, vessels, heat exchangers, and material thermodynamics. Also, perhaps the concept could benefit from employing ceramics for the purposes of strength or insulation. Lastly, the team should be encouraged to search for other work that has explored the concept or portions thereof. Time and energy can be saved by building off others' research. The concept seems too practical to believe others have not investigated.
- Several collaborators were mentioned, but it was not very apparent what they are contributing or have contributed thus far in the project.
- The project partners have been identified. It seems there is an overlap of several activities, e.g., station design and cost analysis are being done by several partners. Thus, while the partners may all contribute to a given activity, it is not clear who is leading it.
- It would be beneficial to have partners with more substantial cryogenic and liquid hydrogen handling experience. They may have some insight that could help the overall system.

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 technically and economically viable, the concept will have a huge impact on all fuel cell applications. The elimination of high-pressure pumps, compressors, and receivers will have an impact on capital outlays and eliminate significant OPEX. The concept also reduces the technical competency required to manage and maintain the system in commercial applications.
- The relevance to being able to recover some of the energy required for hydrogen liquefaction is promising. Theoretically, it should be simpler and less expensive. However, adding many storage bottles—each with its own sensors, valves, and relief valves, plus a control system—is complex and may be expensive and difficult to operate. Diagrams show five active valves per cryogenic vessel (CV), not including relief valves. This could require several hundred cryogenic valves, which is expensive. It is not clear how they are actuated or how much area is required for all these bottles.
- If the number of vessels at high pressure can be significantly reduced, it could be a long-term approach to high-volume station design. Many barriers exist (lack of material for cyclic conditions at cryogenic pressures, insulation to limit boil-off, footprint, and cost).
- The project is relevant to Hydrogen and Fuel Cells Program (the Program) objectives and has the potential to make progress toward lowering the cost of hydrogen compression. The key challenge to the concept appears to be the limited material selections for sustaining the CV operation over its service life.
- If successful, this technology could provide substantial ability to improve fueling capability. However, the capital cost does not appear as if it would scale well for larger stations. The capital cost might appear almost linear as the system scales in size.
- While this technology could ultimately deliver some advantages for large-scale storage at stations, it does not seem certain that this project will identify the designs that will demonstrate those impacts. The Gas Technology Institute might consider making this a pure analysis project prior to moving toward a demonstration that seems likely unachievable, given the schedule.

Question 5: Proposed future work

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

- The team has a good go/no-go gate based on cost analysis vs. the baseline, and it is sound and reasonable within the funding period.
- The proposed “future work” reflects the standard format expected in the Program’s Annual Merit Review presentations. The team should be encouraged to focus more on proving the concept and less on analyzing the economics or physical footprint. If the concept is viable, many organizations will provide the funds to streamline and remove costs.
- The proposed future work is reasonable. The challenge in the “demonstration” phase is not clear: it is obvious that vaporizing liquid hydrogen at constant volume will raise the pressure, and the duration over which this pressure increases is a function of the rate at which the hydrogen is heated. Therefore, the value of data generated needs to be brought out.
- The proposed future work is correctly identified, but it is not evident that the work can happen within the schedule of this project.
- Demonstrating a single vessel may not be indicative of a successful larger future system since the demonstration will not address cycling of vessels or the balance of system issues (piping, valving, and heat leaks). Also, the test vessel should be 1000 bar, not 700 bar, in order to do complete fills. If not already done, an analysis should be completed for the heat leak of the entire system (piping, valves, etc.), including during dormant periods, to see the impact of overall losses.
- It is suggested that this concept’s economic feasibility be moved ahead in the project timeline to demonstrate that this system has a potential cost advantage over today’s conventional station design.
- There should be a firm proposed design that has estimated hydrogen losses characterized for a variety of usage scenarios before the go/no-go decision is made.

Project strengths:

- Theoretically, this approach should require less energy than hydrogen compression. Some exergy is recovered from the liquefaction process. Thermal compression may reduce station maintenance costs and downtime due to compressor failures.
- This is an interesting and unique concept, and there is good collaboration between partners. The approach is novel. Technical back-up material was presented well.
- The concept has few moving parts, lowers required operating competency, and utilizes ambient energy to capitalize on cryogenic density.
- The project has proposed a simple and innovative concept to compress hydrogen and has developed a model to evaluate a host of process and station parameters to optimize cost and hydrogen boil-off losses.
- The project demonstrates a compressionless system. The concept can eliminate the need for dispenser cooling for a fuel station.

Project weaknesses:

- The major weaknesses of the project include lack of a suitable vessel and the inability to scale well past 400 kg—there is not much in the way of savings per kilogram for larger stations since equipment need is virtually linear. Also, it is not clear whether the losses are acceptable based on perception, cost, and safety.
- Weaknesses include the high boil-off rate, the unpredictability, and the associated hydrogen OPEX this boil-off would affect. The large number of vessels may not be a weakness but simply a result.
- The design requires a large number of yet unproven cryo pressure vessels with associated valves, sensors, and controls. There are potential high hydrogen losses.
- The value of the demonstration task is not clear. While it may provide some thermodynamic data, it would have been better if the demonstration was designed so that it could validate assumptions used in the model.
- There are not enough collaborators. There is potential for economic analysis to stifle a great idea.

Recommendations for additions/deletions to project scope:

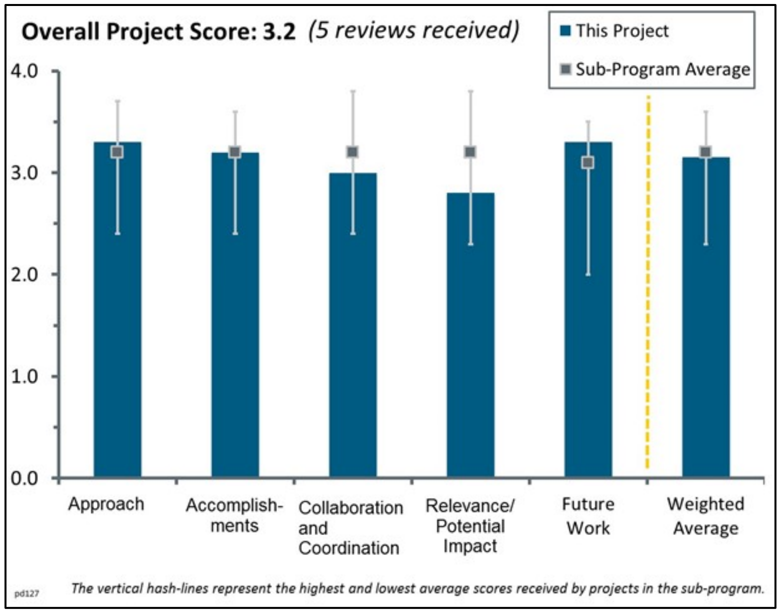
- The project should look into whether the excess hydrogen at 150 K can be economically recovered, to be either compressed by a smaller compressor or re-liquefied and returned to the storage tank. Venting cold parahydrogen seems to be a waste. The model input variables should be separated by what can be controlled by design (number and sizes of vessels, etc.) and what the requirements may be (fueling profile). Then the project should consider worst-case requirements. For example, the project should consider what happens if 10 cars in a row come in and need to charge only from 400 bar to 700 bar. In that scenario, the lower-pressure bottles might have to be vented at a higher pressure, resulting in a higher percentage of hydrogen loss.
- Boil-off rate should be considered in cost analysis. The project should also consider how to reduce the footprint to make it reasonable/similar to existing technology. In addition, the project should consider liquid hydrogen setback distances and the effects that cryogenic liquid hydrogen at 700 bar would have on these at actual sites. This would feed into the footprint discussion. Finally, the project should consider additional thermal modeling to understand possible reduction of the footprint.
- The team can consider replacing the demonstration task with machine learning aspect described on slide 12. The ability to evaluate a host of variables and determine the most sensitive input parameters would be beneficial for any future modeling activities for any other concepts. The team can consider leveraging machine-learning capabilities within the group of collaborators or in the DOE national laboratory network.
- Modeling, vessel options, and demonstration should be added; economic analysis should be pushed to the next phase.
- A separate project is recommended to resolve the pressure vessel issue.

Project #PD-127: Sweet Hydrogen: High-Yield Production of Hydrogen from Biomass Sugars Catalyzed by In Vitro Synthetic Biosystems

Y-H Percival Zhang; Virginia Tech

Brief Summary of Project:

This project addresses the Fuel Cell Technologies Office objective of developing cost-efficient advanced biological generation technologies to produce hydrogen. Investigators are using enzyme cocktails to catalyze the production of hydrogen from renewable sugars (e.g., biomass sugars or starch) and water. This approach is expected to yield high-purity hydrogen at high rates through low-carbon production using local resources.



Question 1: Approach to performing the work

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

- The approach to the work seems reasonable and aims to address barriers.
- The overall approach seems reasonable for a project at a very early Technology Readiness Level. Much of the science seems very interesting and is novel.
  - The use of starch vs. monomeric sugars is very good in this context, although it does add to the complexity of the system. It should be noted, though, that this in vitro approach will likely be effective only for very clean sugars/starches and so will have somewhat limited greenhouse gas benefits when compared to a lignocellulosic system.
- It is unclear why expressing multiple enzymes in one *E. coli* host is useful for this project, other than to minimize the volume for very small-scale assays. The benefit of this will quickly disappear entirely once the system begins to scale.
  - The work on cofactor replacement is interesting and ultimately very necessary for an in vitro system to function and is one of the most innovative areas of the proposal.
  - Overall, the argument that an in vitro system is the best approach for hydrogen production is not entirely convincing, given the complexity of the hydrogen evolution machinery (eight accessory enzymes), but if robust variants can be found for all elements of the system, this limitation has the potential to be overcome.
- With the synthesis of the enzymes and identification of challenges/solutions with the cofactors, the project appears to be off to a promising start in terms of meeting the hydrogen volume objectives. It is still not clear how the project will go about meeting the cost targets, as it is not clear that all of the capital costs have been fully considered. An industry partner might be helpful for testing the commercial viability of the approach.
- From a practical point of view, this approach based on in vitro enzymatic reconstruction of a complete degradative conversion of cellulosic feedstock to hydrogen is among the most risky, high-cost, and impractical approaches conceived.

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 protein overexpression seems to be working very well, and the enzyme production work so far is solid. However, the price and scale at which this work can be accomplished is questionable. Specifically, the bridging electron mediators seem to be fairly expensive. Also, there was not much information given to determine how *long* certain hydrogen productivity rates could be achieved—whether it was just a few moments or sustained.
- Many of the preliminary results are promising, but sustaining peak hydrogen production will be critical for this project to be successful. If there are currently no milestones around sustained production vs. peak production, it may be useful to add some. Additionally, running the system in semi-continuous mode, even at very small scale, seems much more useful than scaling to 1 L.
- There are insufficient quantitative results to assess progress towards achieving benchmarks. Techno-economic analysis (TEA) is qualitative, based on early-stage pre-technologies, and highly approximate.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- The work is shared among a small set of collaborators who contribute useful expertise. The extent to which the principal investigator (PI) utilizes that expertise is not known.
- There is reasonable collaboration between the University of Georgia and Virginia Tech, but the role of the University of Georgia is unclear beyond supplying *Pyrococcus furiosus* enzymes.
- If it were economically viable, the team should be expanded to include a national laboratory and industrial partner at this stage.
- Very little information was given on collaboration details.

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.

- This is a discovery-based fundamental research project that has some overlap with the Office of Energy Efficiency and Renewable Energy mission for hydrogen production. The value in this approach appears to be primarily in demonstration of in vitro biochemistry of isolated enzymes and genetic tools for overexpression.
- Generally speaking, making hydrogen from biomass seems like a non-optimal use of the carbon in biomass. The one real advantage of biomass as a feedstock, aside from the fact that it is renewable, is that it contains carbon in organic form, which can be transformed relatively easily into easily transportable liquid fuels and chemicals. Because biomass is highly oxygenated, making relatively reduced hydrocarbon fuels necessitates the input of reducing power or the rejection of CO_2, but this problem is even larger with hydrogen production, in which all carbon must be rejected as CO_2. Many technologies are looking at making organic molecules from sunlight/CO_2/H_2O because of how central these carbon energy carriers are to the U.S. economy. However, given the state of technology for electrochemical water splitting for hydrogen production and the massively falling costs of renewable electricity from wind/solar, it is hard to see what role biomass-to-hydrogen could play in the renewable energy future.

Question 5: Proposed future work

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

- Decreasing enzyme production costs in an *E. coli* host is not terribly useful. As the PI noted, this would not be the ultimate expression host for the process, and so this optimization of multi-enzyme expression plasmids will not be useful for the ultimate process. The TEA discussion around expression in alternative hosts is convincing; maybe a single proof of concept in a filamentous fungi and then extrapolation to the rest of the enzymes would be sufficient. Cutting this scope would call for more focus on the enzyme engineering/enzyme stability/cofactor replacement. In addition, it is unclear what the scale-up past 10 mL really shows, considering how many elements of this system are still in development. A focus on the other tasks with the resources dedicated to this scope would potentially accelerate the crucial unknowns in this project, such as cofactor switching/enzyme stability.
- The scale-up is appealing if tied to more information on how long the project can achieve its productivity rate.

Project strengths:

- Enzyme production appears to be going well.

Project weaknesses:

- TEA efforts appear to be lacking and could better assess the practical needs for impact.
- More detail concerning hydrogen production rates is needed.

Recommendations for additions/deletions to project scope:

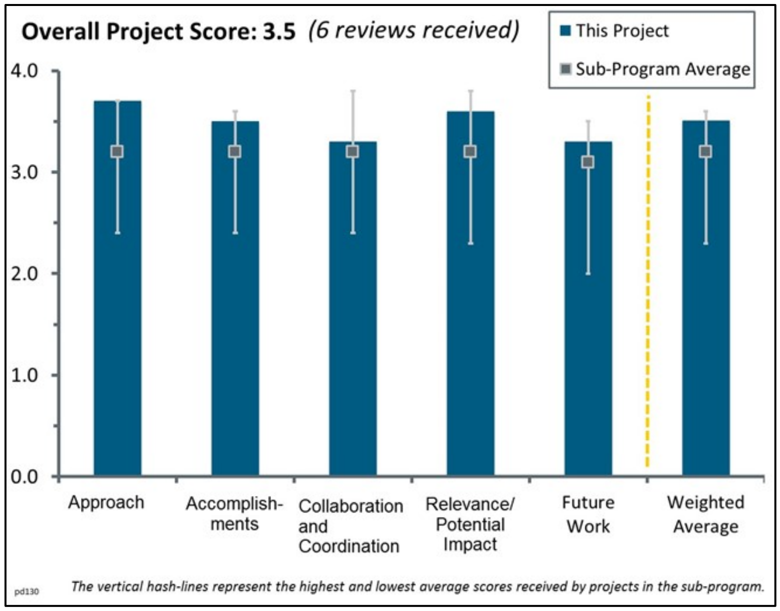
- More detail concerning hydrogen production rates is needed.
- DOE should assess whether this research project will benefit its biological hydrogen program.

Project #PD-130: Improved Hydrogen Liquefaction through Heisenberg Vortex Separation of Para- and Orthohydrogen

Christopher Ainscough; National Renewable Energy Laboratory

Brief Summary of Project:

This project aims to further the first concept in history that directly uses ortho–para conversion to aid in cooling hydrogen. Researchers will develop vortex tubes for hydrogen liquefaction, moving them from Technology Readiness Level (TRL) 2 to TRL 4 such that the technology can be commercialized to units that are 5–30 metric tons per day (MTPD) in size. Exothermic ortho–para conversion results in significant refrigerant use, whereas the vortex concept leverages catalysts for reverse endothermic reaction. The vortex motion cools parahydrogen for subsequent liquefaction. This concept is expected to improve liquefaction efficiency by >40% by minimizing refrigerant use.



Question 1: Approach to performing the work

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

- This initiative represents a new and original approach to liquefaction of hydrogen. It could simultaneously make liquefaction more efficient, reduce the scale of balance of plant and therefore the amount of investment needed in the plant, and increase the flexibility to where liquefaction plants may be located. This would permit locating liquefaction near sources of alternative energy (solar, wind, hydroelectric, etc.).
- A novel concept is proposed that is focused on an important issue with hydrogen liquefaction. If successful, it could have significant impact on the economics of hydrogen production.
- On the Overview slide, it is not clear what is meant by lower liquid hydrogen delivery cost ($4–$15/kg). It is unclear whether this is the reduction of the current cost. Nearly all liquid hydrogen in North America is delivered for $4–$7/kg. A specific cost-per-kilogram savings target should be provided in the overview. For example, a reference to how much reduction in specific power (in kilowatt-hours per kilogram) could be provided, along with specific reduction in dollars per kilogram based on expected power cost. It is also not clear what the total reduction in capital is expected to be. Making this information available will make the value of the project clearer.
- The approach to vortex tube work seems good. The project uses modeling first with testing to validate the model. More details on specific test parameters should have been given. More details on modeling assumptions should be given. The link between the vortex tube work and the work with the refrigerant composed of helium, hydrogen, and neon is unclear. Characterization of the mixed gas refrigerant is also valuable. The cycle as drawn does not work. With 25%–75% hydrogen gas coming out at 30 K and 14 psi, it is unclear how the remaining gas gets liquefied. There is not enough energy for a final Joule–Thompson (JT) expansion.
- The approach to liquefaction is highly original and seems brand new. However, it is unclear whether the approach is feasible. The aspects of the vortex tube operation and how the hydrogen in the periphery may heat up and convert to orthohydrogen by interaction with the catalyst are clear. It is, however, unclear how cold hydrogen in the center of the vortex tube may convert to parahydrogen so rapidly and without being in contact with a catalyst. If it does not convert to parahydrogen, it is not clear that the approach still makes sense.

- It is not clear how lowering the cost and achieving commercialization of units that are 5–30 MTPD can be achieved when the anticipated TRL of the vortex tubes is expected to be 4 at the end of the project. A stage that will bring the TRL of the vortex tubes to at least 7 is missing. Maybe the stated goal should be realistically redefined or clarified.

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.

- It seems like a good deal of good work has happened in just seven months. This includes two different thermodynamic models, a computational fluid dynamics model, and two different types of tests.
- The Status of achieved modeling and analytical and experimental development are outstanding.
- Excellent progress has been made in the limited time available, with a focus on the key aspects of the technology and an appropriate go/no-go decision point. Since the success of the concept is ultimately dependent on the net energy usage, the amount of cooling required for stream E on slide 7 (returned to the source) is critical. A mass and energy balance should be presented for the entire system, not just the vortex tube.
- The analysis and initial experiment at Washington State University (WSU) seems appropriate. However, the helium–hydrogen–neon refrigerant task seems to have little relevance to the overall effort. It is unclear that this task is needed to solve the main issues with the technology.
- It is too early in the process for much input, but the project should be encouraged to get to actual vortex tube experimentation quickly. Pre-work is good, but the project needs to demonstrate actual technology.
- This is the first year of the effort, and it is perhaps too early to comment on progress.

Question 3: Collaboration and coordination with other institutions

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

- The team has all the necessary technical capabilities. In addition, having Praxair as a partner provides the required commercial input.
- Collaborations seem to be balanced between academia and industry.
- The concept was developed by WSU, which has a well-established industry partner, Praxair. If subsequent development is successful, it might be useful to introduce more collaboration.
- Working with Dr. Leachman at WSU and leveraging his hydrogen test apparatus is a good partnership. More details on Praxair involvement would help. The Praxair facility in Tonawanda, New York, has good mixed-gas characterization capabilities and may be able to assist in that task.
- It seems that Praxair is having minimum participation other than to review the relevance of the approach. This relationship should be enhanced, and Praxair should contribute to all aspects of the problem, including cost analysis and comparison with other existing approaches.
- There is only one partner other than the prime technology provider. Input from additional industrial gas companies that currently operate liquid hydrogen plants would be useful, in addition to at least another organization that can provide feedback on the core technology. In addition, potential collaboration with the magnetocaloric liquefaction project could be useful. It is possible that each technology could benefit from 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 **3.6** for its relevance/potential impact.

- The problem of small-scale liquefaction is important for future hydrogen-fueled transportation, and it seems that this concept is scalable to small sizes, even though the analysis presented seems directed to large sizes.

- Liquefaction plants are large and expensive, and there are few in the United States and Canada. This technology could revolutionize the scale-up of infrastructure to help meet hydrogen supply needs as automotive demand materializes. If the proposed benefits are realized, another aspect of revolutionizing infrastructure would be the reduction in capitalization necessary to build the hardware.
- The project clearly addresses a key barrier to a liquid hydrogen pathway in terms of high energy use and cost. It is well aligned with DOE goals and targets.
- The project's success could be critical to achieving DOE goals.
- This is solid, basic research that could lead to potential improvements in technology to lower the cost of liquid hydrogen production. There may be challenges and it may not work, but it is worth continuing.
- Increasing the figure of merit (FOM) of hydrogen liquefaction is an important goal. If successful, the vortex tube cooling has the potential to do that. The project emphasizes scaling up, but the technology may also make small-scale liquefiers competitive. More details on the relevance of the mixed-gas refrigerant would help. It is unclear whether the refrigerant is to be used for isothermal cooling at 20 K.

Question 5: Proposed future work

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

- The future work plan adequately covers the key aspects and is appropriately focused on the go/no-go step. It is critical to understand the potential impact of the technology on the net energy usage and cost of a liquefaction plant. Hence, the planned techno-economic analysis that will indicate the impact of the proposed work on the overall cost is essential and appropriate.
- The scope of the proposed future work seems to be appropriately defined.
- The proposed work appears to address research goals. However, the technology may have application in solving different problems that are not in the scope defined for the present work.
- It seems that a good idea for a future task would be comparing this approach to liquefaction with today's power plants under a variety of performance conditions. For example, it would be good to know what happens if no ortho–para conversion occurs in the middle of the tube and the result is 50% parahydrogen at the cold end. It is unclear whether this will destroy the concept. It is unclear how this would affect the results, considering that this conversion would have to occur outside the tube.
- The project should focus on technology of liquefaction, not location of hydrogen plants. Location of plants will be dependent on the market and feedstock costs, not on the location of wind power. Distribution costs from small, remote plants will likely outweigh the potential for savings.
- The proposed go/no-go test is for 500 g/hr flow of hydrogen with 5% para–ortho conversion. However, more than composition should be measured; the temperature drop and the warm gas bypass rate should also have some metrics.

Project strengths:

- The FOM computation indicates good promise for improvement of liquefaction efficiency over standard methods.
- A new technology to lower the cost of ortho–para conversion by performing at a higher temperature is interesting and potentially valuable.
- If successful, the vortex tube has the potential to reduce hydrogen liquefaction costs and meet DOE targets. The mixed-gas refrigerant work is interesting and will provide new data on possible refrigerant mixtures.
- This is a novel concept with high potential impact. The project has a solid technical background, as well as good planning and focus.
- Modeling, analysis, and experimental capabilities are project strengths.
- The approach is highly original.

Project weaknesses:

- This is not strictly a project weakness, but it would be helpful if comparisons to current liquid hydrogen production methods were expanded upon, and an effort made to predict the potential for cost savings. In addition, the benefits of a smaller balance of plant could be expounded upon in greater detail.

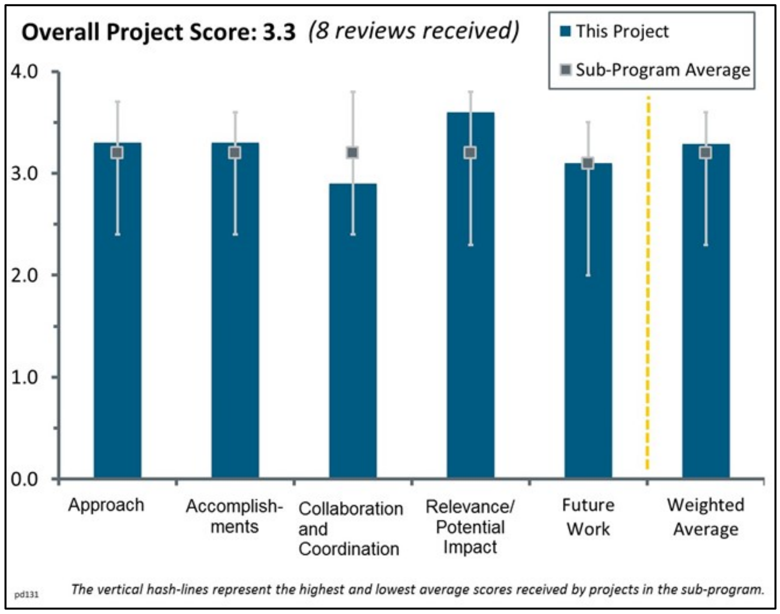
- Scale-up must be validated. The project needs to develop specific cost savings per kilogram to compare to existing liquefaction (e.g., specified power and capital reductions) to show value. In particular, the cost savings of the new technology might be overwhelmed by the worse economics of small-scale production; i.e., it is best to focus on large plants.
- The system requires approximately 40% of the flow to be diverted back to the compressor, increasing compressor size and operating cost. The cycle as shown requires two cold compressors. The project may want to add a recuperative heat exchanger and use warm compressors. It is unclear that the vortex tube will work as advertised.
- There is potential over-optimism regarding the commercialization impact.
- It is unclear that the approach is feasible.

Recommendations for additions/deletions to project scope:

- Apparently the technology could play a role in boil-off recovery from existing storage systems. This capability would be of great value to liquid hydrogen users with large storage systems and the need for long product “hold” times. It may prove that some combination of vortex separation and magnetocaloric cooling can yield even greater system efficiencies and cost reductions for production. Magnetocaloric cooling might be a good “front end” for the vortex separation. Initial development should proceed as planned to validate these technologies, but some thought should be given to seeing whether the two processes are complementary in some way.
- The project should consider cooperation with a magnetocaloric project. Focusing on liquefaction technology exclusively is recommended, regardless of the location/type of feedstock or power. The liquefaction technology is the breakthrough, not the location of small plants, etc. It will likely be easier to transport renewable power than ship liquid hydrogen over long distances.
- The proposed process is not entirely clear. It appears the project is using liquid hydrogen plus a catalyst to convert warm equilibrium hydrogen to cold equilibrium hydrogen, removing the exothermic heat of conversion of 25% of the gas. Then the vortex tube converts a certain mass fraction of that back to normal hydrogen at warm temperatures, and the endothermic heat of reaction is used to cool the remaining hydrogen. It is unclear whether the advantage is just being able to remove less heat of conversion at higher temperatures to create para, then reconverting the para back to ortho, removing a higher heat of conversion now at a lower temperature. It is unclear that if there were no ortho–para conversion back and forth, the vortex tube would act as an isenthalpic expansion process to cool the gas. It is unclear whether the vortex action helps make it closer to isentropic instead. It is unclear why the cold gas flowing through the center of the tube does not exchange heat with the warm gas flowing around the outside of the tube. More details should be given in future presentations, including mass and energy balance of the vortex tube.
- The project should focus on the vortex tube operation and performance and eliminate side tasks that do not contribute to this effort.

Project #PD-131: Magnetocaloric Hydrogen Liquefaction

Jamie Holladay; Pacific Northwest National Laboratory



Brief Summary of Project:

The Pacific Northwest National Laboratory (PNNL) magnetocaloric hydrogen liquefaction system is expected to be considerably more energy efficient than the Claude cycle. At 30 tonnes per day (tpd), the latter shows 40% efficiency, while the former is projected to be 70%–80% efficient. In this project, investigators will demonstrate the PNNL system liquefying ~25 kg/day with a figure of merit (FOM) >0.5 (as compared to the Claude cycle system's FOM of <0.3). The project will also identify a pathway to a larger-scale system with an installed capital cost of less than $70 million.

Question 1: Approach to performing the work

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

- The approach, successively testing different prototypes, is strong. The project starts with an existing Generation (GEN) I testbed, then designs a new GEN II prototype to build and test. The team then applies these lessons to the final GEN III system. If all that is successful, then this project may have a huge impact on future hydrogen liquefaction.
- It is good that previous project results (i.e., from Prometheus Energy Group, Inc., [Prometheus]) are followed up. The approach seems systematic. It is unclear how deeply involved large gas companies are, or whether they are interested in the technology.
- The project appears to be very successful in proving the technology and refining the efficiency of the dual active magnetic regenerator (AMR) design; however, there was little to no discussion about the complete liquefaction system. It was unclear how the ortho-para (O-P) conversion will be performed, what the overall energy consumption will be, and how many dual regenerative AMRs will be required for a 30 tpd liquefaction plant.
- The capability of 100°C temperature reductions by the proposed magnetocaloric cooling system will be a breakthrough in hydrogen liquefaction, if achieved. The introduction of this technology would reduce hydrogen liquefaction costs at a time industry needs to increase production. There is no question that this research directly addresses important barriers of the cost of producing liquid hydrogen.
- Magnetic refrigeration is not a new concept. Development of this concept was previously funded by U.S. Department of Energy (DOE) at Prometheus with Dr. Barclay, who is a consultant on this project, as the principal investigator (PI). It is not explained how the proposed approach is different, whether it is an improvement over the previous work, or what specifically was changed. While the concept is interesting, the approach needs to identify key novel features properly and explain why these would lead to a successful development given the critical barriers.
- The project is focused on creating a magnetocaloric liquefaction machine that operates from ambient temperature all the way to 20 K. This seems rather difficult to accomplish. Magnetocaloric materials operate best near a phase transition temperature, especially if this is a second order transition. The authors would, therefore, need to select a cascade of materials with appropriately located phase transition temperatures to make this work. It is unclear whether these materials have been selected and properly characterized. It is also unclear whether the performance predictions take this into account.

- There is no mention of either O-P conversion or the energy required to make this conversion, so it is not clear whether that conversion is included in the 6 kWh/kg target for liquefaction energy or in capital cost estimates. Barriers are addressed, but there appear to be too many for one project.
- The demonstrated approach and projected targets are sharply focused on critical barriers.

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.

- Achieving a world record on ΔT for He is very impressive.
- There are a lot of accomplishments and good early results. It is good to see that the existing machine was refurbished so rapidly and that it produced many good results.
- Given that the project is 10 months into a four year effort, the progress is good, but it is important to keep in mind that in previous funding DOE spent $2 million on the technology that facilitated much of the progress in this project. By the next DOE Hydrogen and Fuel Cells Program (the Program) Annual Merit Review, it is hoped that the project will have progressed on technology development and will also have provided equal attention to the balance of plant to enable reviewers to fully appreciate a production design that accounts for all energy demands (O-P conversion, cooling, heat transfer fluid management, etc.).
- It appears there was a lot of progress in the first part of the project, but the starting point based on past work was unclear. Characterizing the temperature spans and heat lift in the GEN I system was a good accomplishment as well as figuring out to add the bypass flow. The propane liquefaction demonstration offers a preview of the potential for other gasses. In addition, a good start on the GEN II design was accomplished.
- The project seems to be on schedule.
- While a lot of details are provided, a more concise description would be helpful to clearly understand what is the background material (e.g., from PD-019 from fiscal year 2011/2012) and what is accomplished in this project. It is unclear whether this project is a continuation of the previous project. It would be useful to elaborate lessons learned from the previous work and see whether and how they are guiding the project plans. Based on the description provided, significant progress seems to have been made toward the goal. However, since it was previously attempted to develop this concept at Prometheus with DOE funding, it is critical to reference the work and to specifically point out differences, improvements, etc., and whether there were any shortcomings or hurdles before, which are being addressed in this work.
- The project has demonstrated liquefaction with propane, which is good for demonstrating initial promise, but propane may be a relatively easy fluid liquefaction compared to the liquefaction of hydrogen. While the magnetocaloric cooling properties were explained, the "bypass" design, a key element in the system to achieving the large temperature reductions, was not covered as clearly. The project has just begun, so accomplishments with hydrogen are limited at this point.
- The presentation was not concise or clear as to progress and barriers. There was too much technical detail in some areas, but not enough explanation of how the pieces fit into the "bigger" picture. The main benefit of the liquefaction technology is stated as reducing the compression energy for the Claude cycle to get to liquid nitrogen (LIN) temperatures. However, if this is the case, the technology might be better proven by making other cryogenic products first, instead of going to the more difficult step of liquid hydrogen. For example, the technology might be more cost effectively used to liquefy air, which is a much larger industry. While the test on propane was interesting, liquefaction of air would be a more impressive accomplishment to test the technology.

Question 3: Collaboration and coordination with other institutions

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

- The project has good coordination with Ames National Laboratory and its regenerator material group. It appears the materials database shows many magnetocaloric materials over the necessary temperature span to choose from. It is unclear what happened to past collaborators in this area such as John Barclay and Prometheus.

- The project partners appear to be appropriate and well-coordinated.
- There are a limited number of participants, but the partners are experts.
- For the work performed to date (i.e., refurbishment of Prometheus equipment), the collaboration appears to be well-targeted. Going forward, the team needs to expand to include individuals and organizations that can help determine whether the technology can be scaled up in a manner that retains the stated targets of $70 million in capital and 6 kW/kg. This support has to come from organizations that have experience in process and plant design and safety standards, and the support has to account for maintenance and operations.
- The project has good collaboration efforts with partners having necessary skills and capabilities. It would be helpful to have an industrial partner to assess commercial feasibility of the technology, if successful.
- The project should include a potential future user (e.g., Linde Group or Praxair) in the team to provide industry perspective and cost evaluation. Magnetic refrigeration is commercially available for ultra-low temperature. The project team could investigate whether any of these magnetic refrigeration companies would agree to contribute to this project.
- There are no industrial gas companies collaborating on the project, nor anyone actively liquefying hydrogen for validation of technology. Including these partners is recommended, especially since it is not clear that the project participants have any direct experience with liquefaction of hydrogen.
- It is unclear how deeply involved, or even interested, gas companies such as Linde Group, Air Liquide, or Air Products and Chemicals, Inc., are.

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.

- Active magnetic refrigeration has great potential to reduce liquefaction costs and reach DOE goals. The preliminary cost analysis shows the feasibility.
- This work directly supports DOE Program goals for development of a national hydrogen infrastructure. The technique, already demonstrated to have import for the propane industry, may have similar implications for production of liquid nitrogen, liquid helium, and other cryogenic fluids.
- Hydrogen liquefaction is an important area, and any reduction in energy usage and cost for this step would be helpful. The proposed approach is claimed to have a potential major impact on the overall energy and cost. The proposed new concept would replace liquid nitrogen cooling section, including associated compressors. This is a part of the overall liquefaction system. It would, therefore, be helpful to illustrate what portion of energy and cost will be impacted and to what extent, along with cost/benefit analysis, to clearly point out the benefit of the work.
- The project has good relevance as liquefaction (whether on site or not) is sure to play an important role in the future of hydrogen-based transportation.
- If the project objectives can be achieved in a functional plant, the relevance of this project is very meaningful.
- There is no question that the success of this project could be critical for achieving DOE market deployment goals.
- If the projected values can be reached, this will be an important step to reach the DOE target.
- It could be a breakthrough technology, but at the current development stage, it is unclear whether the cost estimates can be credibly assessed. For this reason, it is not clear whether it will be able to support DOE efforts.

Question 5: Proposed future work

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

- The proposed future work is excellent. More details should be given on the GEN II prototype. It is unclear whether the GEN II prototype is just one stage of a multistage device for GEN III. The temperature span is

also unclear. The work done on manufacturing different materials for the regenerator heat exchanger use is critical.
- The future work described is comprehensive and covers all essential features.
- Proposed future scope seems appropriate to meet the project objectives.
- The authors should develop a systems model that may predict liquefaction efficiency based on what materials are being used at what temperature levels and how these will perform. This is key to determine how likely this approach is to reach the efficiency and cost targets presented here.
- The plan for a GEN II system seems to be the correct path to take, but more needs to be accomplished in defining a full-scale system (30 tpd) and evaluating its financial viability.
- The challenges faced by this project are significant and range from manufacturing the base material up through the process cycle itself. It is likely best for the project scope to be narrowed to some specific near-term (and simpler) objectives, which can then lead to a future hydrogen liquefaction system.
- The project should incorporate gas companies unless Emerald has the potential to industrialize such a technology.
- At this juncture, the proposed work has just begun. While the proposed work looks logical, this reviewer is not sufficiently expert to comment on the proposed development plan.

Project strengths:

- The investigators have made good progress in a short time. They have identified key issues and have designs and mitigations in place. The idea of magnetic refrigeration has great potential to simplify and reduce the cost of hydrogen liquefiers.
- This could provide a technology breakthrough, if successful, and significantly lower cryogenic processing cost.
- The project undertakes a novel concept compared to state of the art for hydrogen liquefaction. The project has strong team expertise and fundamental understanding of technical details as well as existing equipment, materials, and background work to build on.
- The project has high relevance and has made excellent progress with many accomplishments in very little time.
- The project has a strong knowledge and expertise base as well as appropriate partners.
- Optimizing the AMR technology and demonstrating design improvement in the GEN II stage is a project strength.
- The project directly addresses issues of cooling efficiency for hydrogen liquefaction. The development of this capability requires advances in several areas: extending the magnetization cooling properties with layered materials optimized by the use of different rare earths; demonstrating the “bypass” technique; and expanding the technology capability of rotating disk atomizations to work with rare earth alloys.

Project weaknesses:

- The project needs more, and better, partners within the cryogenic and liquid hydrogen industry. The project needs to address O-P conversion. If it is included in process calculations and capital, the project should provide the details. The project has too many risks and assumptions for one project and should be narrowed. There are many intermediary steps between proving the refrigeration capability of this technology down to liquid hydrogen temperatures. It would be best to work on those intermediary steps first and prove it to be a viable refrigeration technology at warmer cryogenic temperatures first.
- The project is ambitious in that it will pioneer several advances simultaneously. The PI had difficulty explaining the various aspects of the “bypass” innovation that was critical in achieving the cooling gains advertised. It remains to be seen whether progress will be straight forward in achieving cooling at the lower temperature regimes required for liquefaction of hydrogen. A lot rests on the cooling effectiveness of the multi-layer magnetic regenerators for the GEN II system.
- The lack of a concise description of a new concept is a weakness. The lack of focus and the absence of go/no-go decision points are weaknesses considering that this project is high risk.
- There is no set plan to fully quantify the long-term goal (capital and performance) for a 30 tpd design.
- These ideas have been proposed for years, but practical designs have not been achieved.
- The feasibility of the approach remains questionable.

- The lack of industry collaboration is a weakness.
- Magnetic liquefaction has been promised to work “tomorrow” for the past 40 years.

Recommendations for additions/deletions to project scope:

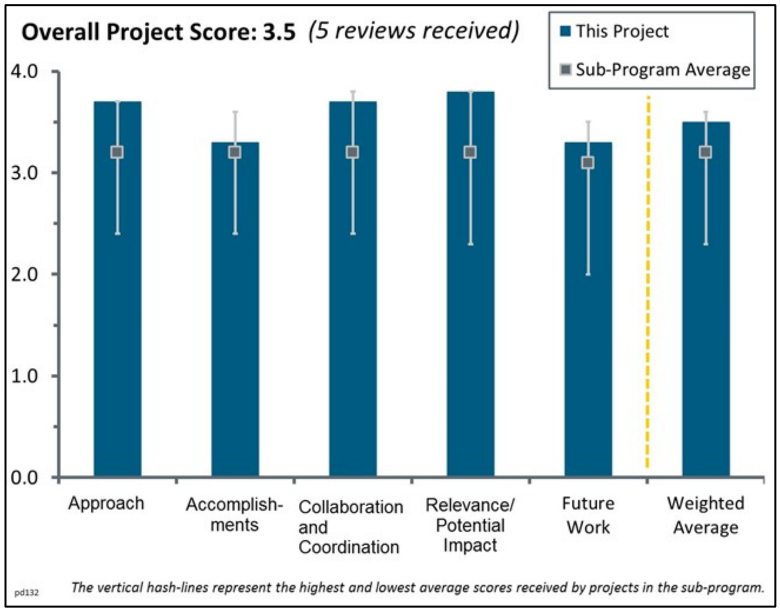
- Consider collaborating with the vortex tube liquefaction project. Maybe magnetocaloric hydrogen liquefaction can do refrigeration to -320°F, and the other project can refrigerate to -420°F. The project should delete any consideration of assistance from liquefied natural gas liquefaction. This is known technology and obfuscates the benefit of the magnetocaloric hydrogen liquefaction technology. For the next step, focus on demonstrating better performance than traditional refrigeration to -320°F LIN temperatures, rather than liquid hydrogen.
- There are several areas that might be noted. This technology stands to be synergistic with the vortex flow technology, and this synergy should be pursued. This technology, or some element of it, may prove useful in recovering cold gaseous boil off from storage vessels. More important, this technology does not simply apply to liquefaction of hydrogen, but would have application in the production of other cryogenic fluids.
- The investigators should clearly identify key challenges of magnetic refrigeration that were not attempted or satisfactorily solved before and focus efforts on those. A critical path analysis would be helpful to present with well-defined go/no-go decision points.
- It seems that, given the low level of technological maturity, it may be premature to do a cost analysis, as the analysis is likely to change as the system definition improves.
- The project should include more collaborators. Inclusion of an assistance PI responsible for evaluating the viability of a full-scale plant is also recommended.
- The project should perform industry workshops.

Project #PD-132: Advanced Barrier Coatings for Harsh Environments

Shannan O'Shaughnessy; GVD Corporation

Brief Summary of Project:

Plastic and elastomeric seals are integral to all areas of hydrogen compression, storage, and dispensing (CSD), and seal failure is a major contributor to hydrogen compressor maintenance, adding significant downtime and cost to operation. This project is developing two types of coatings to improve seal life from <1,500 hours to >8,000 hours: flexible barrier coatings that mitigate hydrogen ingress into the seal, preventing premature failure; and low-friction coatings that reduce wear of rigid seals, extending seal life significantly. Using polymer vapor deposition will result in uniform coatings that are conformal down to the nanoscale.



Question 1: Approach to performing the work

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

- The technical approach of the principal investigator (PI) was soundly presented as a way to improve seal performance in non-lubricated compressor seals. The work has primary relevance to moving seals. The approach to application of coatings to a variety of elastomers and rigid seal parts is good. Uniform deposition and depth of impregnation is a key challenge for most coating technology.
- Great work on polymer layer impregnation with physical vapor deposition (PVD); this looks very promising. The vacuum tumbler is great idea on how to develop a new manufacturing method.
- This project is focuses well on its target—producing a low-cost, low-friction, high-sealing material for compressors. They are spot-on in their approach.
- The approach is defined well and well thought-out, considers key barriers and issues with the state of the art, and demonstrates good understanding of fundamental problems. The science behind the proposed solution is explained well and covers the necessary details. The efforts are focused on solving the specific issue with compressor seal failure.
- GVD Corporation (GVD) understands the problem well and has demonstrated that their well-thought-out approach can overcome the barriers.

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 vacuum tumbler is great idea on how to develop a new manufacturing method.
- For a project that is only one year along, their progress is excellent. They have optimized the chemical vapor deposition (CVD) process and are on track for scale-up.
- For the one style of coating and deposition presented, the PI had good, presentable results. Something missing was a risk mitigation plan if this particular coating and deposition did not work as planned and alternate depths, materials, or processes of deposition could be used to plan for unforeseen real world

results. Further, it is not clear if there are other applications and if they would be effective in other industries.
- GVD is making good progress toward overall project and DOE goals. Their accomplishments have been demonstrated by their good barrier coating technology, which shows significant reduction in helium and hydrogen ingress via permeation through elastomeric seal materials.
- A systematic development and testing plan is in place with attention to detail. Given that the team has already spent little more than a year on the project, slides 14 and 15 do not appear to reflect adequate progress. It is unclear which portion is background GVD work and which is performed under this contract. It would be helpful to more clearly point out the accomplishments during the project time period.

Question 3: Collaboration and coordination with other institutions

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

- The team has assembled the right set of talent in their collaborators—an advance seal manufacturer, a manufacturer of compressor equipment, an industrial user, a hydrogen fuel system (HFS) designer, and a national laboratory—to understand and perform fundamental permeability measurements; this is excellent.
- GVD has demonstrated good collaboration with other partners, with whom well-planned coordination as well as participation has been a key for their success.
- This is a strong team of collaborators, and their work plans are sound.
- Nice collaboration with industry partners that really need some help with seals to support linear hydrogen compressor technology.
- The partners selected are appropriate to cover all aspects of technology development and commercialization.

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 relevance of this work is spot-on. Hydrogen fuel cell (HFS) reliability is by far and away the biggest Achilles heel for the early deployment of HFS; this goes right to customer satisfaction. Compressor failure is the biggest factor in HFS reliability, and this project addresses that issue.
- This work on PVD layers for polymers promises a potentially major impact on reducing compressor maintenance costs.
- The project aligns well with the Hydrogen and Fuel Cells Program and DOE research, development, and demonstration (RD&D) objectives, and has potential to advance progress toward DOE RD&D goals and objectives. The project has a significant impact on the success of delivery hydrogen fuel to the end users. Besides the fact that it could reduce the cost of delivered fuel, the safety as well as environmental impacts are also important factors to consider.
- With a good risk mitigation plan, this project could have significant impact on station reliability and stations' operating expenses.
- The project addresses an important, known practical issue with the high cost of hydrogen compressor maintenance. It is well-aligned with the DOE goals and program plans. A techno-economic (cost/benefit) analysis showing the potential impact of successful implementation of the proposed solution would be helpful.

Question 5: Proposed future work

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

- The vacuum tumble PVD system will hopefully demonstrate seals with new properties optimized for high pressure hydrogen service.

- The only thing preventing this project from being rated "good to excellent" is identifying the metrics for what is acceptable compressor testing wise; permeability is straightforward and good. The PIs must specify how often they will inspect seal wear, and what patterns they will look for. This type of testing approach was not presented.
- The proposed future work covers essential aspects necessary for commercial implementation of the technology. It may be helpful to conduct accelerated durability testing of the coatings in a simulated environment before testing in a compressor under actual operating conditions. Also, a cost-benefit analysis would be helpful.
- The proposed future work is effective. However, it lacks a risk management strategy for scaling up the design to accommodate large-scale operation with high-throughput manufacturing. Mitigating risk is particularly crucial in making sure that the project is executed smoothly and effectively as planned.
- The proposed future work is good; however, this project really needs to start concerning themselves with possible contaminant poisoning of the fuel, through both outgassing and friction degradation. Concentration levels on the order of parts-per-billion (ppb) are required to maintain the fuel quality needed for fuel cell electric vehicles. Also, maybe this is a bit unfair, but the fuel quality specifications are being revisited and may go lower than currently published standards (SAE J2719, and ISO 14687). This is a serious enough omission in their future plans that it cost them an entire rating point.

Project strengths:

- The project enjoys good scientific and technical background for the proposed solution, excellent capabilities and qualifications, and appropriate partners.
- The project is technically sound and well-coordinated with well-known collaborators who are considered experts in the field.
- This work is very relevant, and employs a good approach to basic science. This style of station presents an opportunity for reducing operating expenses.
- The team is excellent, as are the facilities and program plan.

Project weaknesses:

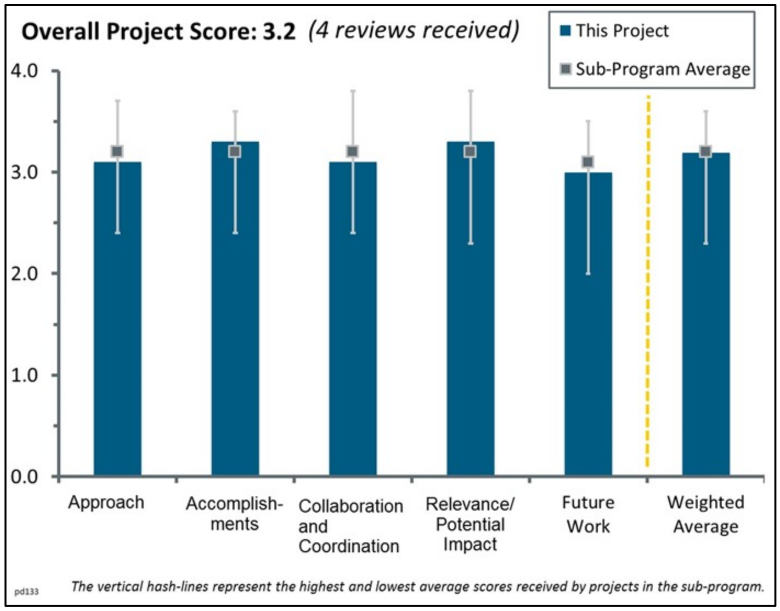
- The project lacks risk mitigation plans, should this style of coating and deposition not work.
- The project lacks a risk management strategy, which could cause many failures such as project delay, costly repair, and unsafe operation.

Recommendations for additions/deletions to project scope:

- This project really needs to start concerning themselves with possible contaminant poisoning of the fuel, through both outgassing and friction degradation. Concentration levels on the order of ppb are required to maintain the fuel quality needed for FCEV. Also, maybe this is a bit unfair, but the fuel quality specifications are being revisited and may go lower than currently published standards (SAE J2719, and ISO 14687).
- It is highly recommended that a risk management strategy be added to the project scope to ensure its success.
- The PIs should create alternate methods to achieve project goals/have a risk mitigation plan.

Project #PD-133: Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) – Consolidation

Christopher Ainscough; National Renewable Energy Laboratory



Brief Summary of Project:

This project aims to reduce at-the-pump hydrogen prices by decreasing the cost contribution of fueling station capital, specifically compression costs. Currently, compressors for large stations can cost up to $1 million. The project will design and demonstrate a hydrogen station based on a tube–trailer consolidation concept that will increase compressor throughput and reduce compressor size.

Question 1: Approach to performing the work

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

- The project's approach sounds promising. It is unclear why there is a lack of refueling infrastructure data with approximately 20 stations in operation and the data being acquired by the National Renewable Energy Laboratory (NREL).
- It is unclear if any benchmarking has been done based on the different systems available. It is also unclear if there are noise-related issues with the compressor, which could potentially limit hydrogen fueling station (HFS) installations.
- The presentation did not make clear how this novel compressor design was an advantage over two compressors achieving the same objective. A more practical approach would be to move the proposed "first stage" of the novel compressor to the terminal compression location and use 400–450 bar tube trailers to deliver hydrogen. This concept is currently being developed by some gas merchants, and achieves the same objective as this project without the need to develop this technology for the station itself.

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.

- No benchmark against DOE goals is provided. Overall project progress is good. As mentioned in the presentation, a vehicle simulator could be helpful for station providers and car original equipment manufacturers (OEMs).

Question 3: Collaboration and coordination with other institutions

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

- The project should incorporate additional collaborative partners (industry) and a more diverse stakeholder set and inputs.
- Even though this is a broad approach on increasing performance and decreasing cost of stations, the spectrum of partners is very small. It is unclear if major station providers are 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 **3.3** for its relevance/potential impact.

- If ambitious objectives are reached, the project could have a large impact on station cost reduction. The relevance for major industry players is not clear.

Question 5: Proposed future work

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

- Elaboration on future work is not very detailed. It is unclear what will be tested and when it will be tested.

Project strengths:

- The project offers a large potential impact on station cost.

Project weaknesses:

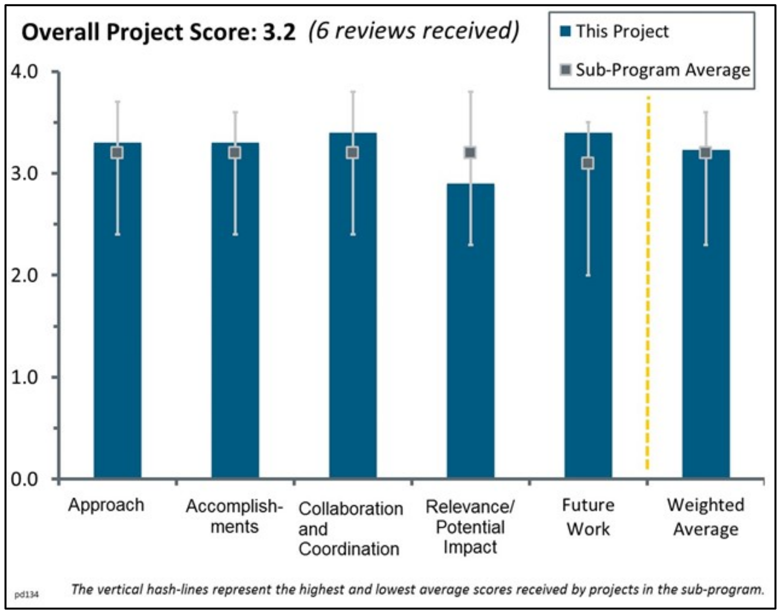
- There is no collaboration with station system technology providers.
- The presentation did not make clear how this novel compressor design was an advantage over two compressors achieving the same objective. A more practical approach would be to move the proposed "first stage" of the novel compressor to the terminal compression location and using 400–450 bar tube trailers to deliver hydrogen. This concept is currently being developed by some gas merchants. This achieves the same objective as this project without the need to develop this technology for the station itself.

Recommendations for additions/deletions to project scope:

- The project leads should convene industry workshops.

Project #PD-134: Cryo-Compressed Pathway Analysis

A.J. Simon; Lawrence Livermore National Laboratory



Brief Summary of Project:

This project is developing well-to-wheels (WTW) cost and emissions estimates for cryo-compressed hydrogen (ccH2) pathways. Investigators are building physics-based and industry-guided estimates of system and cryopump performance and cost into the Hydrogen Delivery Systems Analysis Model (HDSAM). The project furthers the ability to identify the cost-effective options for hydrogen delivery by enabling analysis of infrastructure trade-offs through an investigation of key parameters associated with liquid hydrogen.

Question 1: Approach to performing the work

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

- The approach is appropriate to estimate future costs and greenhouse gas (GHG) emissions for ccH2. This will be helpful to establish technical targets for components, but it is not useful to understand current technology status. The study relies on models that assume high-volume liquid hydrogen (LH2) production and high market penetration of fuel cell electric vehicles. Both sets of data are needed to trace a pathway from today's cost, GHG emissions, and technical performance to their future, desired values. Today's WTW values are presumably very different from what they will be in the future, so it would be helpful to know how different they are. Low station utilization will have a significant impact on the WTW results. It is good that some of the hydrogen losses will be actually measured. If more money becomes available, the next step should be instrumenting the facility to validate station engineering calculations for the losses that have not been physically measured.
- It appears to present a reasonable approach to collecting information about barriers addressed by this project. It appears that the project is estimating "potential" boil-off and not necessarily estimating "actual" boil-off. It is not clear whether the initial description of ccH2 is 350 bar or 700 bar.
- The approach is pretty clear—the project wants to better understand the thermal connection (if any) between the two dewar systems.
- It is a small project with clear focus.
- First, the title of the project appears misleading. The project as described is about LH2 pathways, not cryo-compressed pathways, as is generally understood, and this is correctly stated in the objective on the Summary slide. Secondly, although the objective is stated as pathway analysis, the work described is mostly about estimating boil-off losses. If the project is defined as focused on LH2 boil-off losses, then the approach is adequate. The total liquid hydrogen pathway analysis would also involve estimating costs and emissions of the major contributing step, namely liquefaction.

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 excellent results on the cryo-compressed pathway.
- There is nice work/progress for a small project—it is interesting stuff.
- Progress is good. It seems that the team may be able to finish ahead of schedule.
- It is not clear why "losses from vehicles" after dispensing are included in overall boil-off loses. The project should clarify what assumptions are made for cryopumping and the effect on vehicles, e.g., whether the station dispense 35 MPa or 70 MPa CcH2.
- It would be helpful if the methodology used in estimating the losses were more clearly described. The results as presented are somewhat confusing. For example, on slide 7, Total Delivered and Total Dispensed bars have the same value; it appears that the losses are not subtracted. It is unclear as to how the analysis results presented can be used to understand boil-off losses, reduce losses, or overcome associated barriers. Decreased losses with larger stations, higher demand, and better delivery logistics are obvious and likely well understood in the industry. The value of the work needs to be better explained and represented. It is not clear how variation in demand during the day (with number of cars) would impact boil-off.

Question 3: Collaboration and coordination with other institutions

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

- The right institutions have been approached to provide feedback. Shell is not mentioned as one of the collaborators; it would be useful to get its input on the analysis, given its experience operating a LH2 station. Granted, the hydrogen is re-gasified and delivered in gaseous form, but its expertise with production, delivery, and storage could be useful.
- Relevant partners are onboard.
- Most appropriate partners are selected, although their specific contributions are not obvious.
- The project should consider involving Washington State University Professor Leachman to discuss project accomplishments from an academic perspective on LH2 boil-off losses.

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.

- Given the lack of data in this area, it is critical to understand the potential impact of ccH2 if this technology is to be deployed at large scale in the future. Also, the relevance of the study would be greater if WTW parameters were calculated/measured for current technologies as part of the project's scope.
- It is unclear how this ccH2 will go, but it certainly is an interesting and significant line of research.
- The project contributes to understanding of LH2 boil-off losses, which is good for more accurate hydrogen delivery modeling. However, at currently observed (potential) boil-off loss rates and trailer capacity, it may not make economical and logistical sense to consider alternative distribution methods and/or invest in improving (reducing) boil-off rates.
- Hydrogen boil-off losses are important in the LH2 delivery pathway. While some of the losses may be reduced with better equipment and delivery logistics management as well as higher usage profile, some of the losses may be unavoidable. The impact of these losses on the overall cost and emissions appears minimal.
- It is uncertain how much cryo-compressed systems will play a role in future because of manufacturing issues with the storage system.
- ccH2 does not seem to be a realistic future pathway. Only a single original equipment manufacturer is onboard and collaborating.

Question 5: Proposed future work

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

- The proposed future work is nice—there is a clear plan for project completion.
- The proposed future work is appropriate. Actual measurement of various losses would be useful, and it is important to compare them with the estimates to confirm the methodology.
- Given the budget limitations, the proposed future work is adequate, but it would also be helpful to: (1) estimate/calculate WTW parameters for today's operation conditions, (2) work with an instrument facility to validate engineering calculations for hydrogen losses, (3) ensure that the cost analysis is in alignment with financial models such as the Hydrogen Financial Analysis Scenario Tool (H2FAST).
- The project should stay focused on LH2 delivery losses and LH2 cryopumping. It should not expand too far outside of these areas, because there is plenty to learn in these areas.
- With regards to the project objectives, the proposed final steps make sense.

Project strengths:

- The strengths include the facilities, expertise of Lawrence Livermore National Laboratory staff, and collaboration with partners with expertise in delivery and dispensing of LH2.
- The strengths are the new territory for understanding boil-off rates of LH2 and involved partners.
- The project is interesting and cutting edge—ultimate applicability may be more for medium- to heavy-duty and large marine applications, but this is interesting stuff.
- The understanding of fundamentals of liquid hydrogen boil-off behavior under different conditions is a project strength.
- The project has a very good and detailed analysis of the pathway.

Project weaknesses:

- Venturing into vehicle-side loses is a project weakness. It is unclear what the state of hydrogen is out-of-cryopump and on what infrastructure/type of vehicles this applies, e.g., 350 bar hydrogen, 700 bar hydrogen, 350 bar CcH2, or 700 bar CcH2.
- The weaknesses are the narrow focus within the given pathway and the lack of clarity.
- The relevance of the project is uncertain.

Recommendations for additions/deletions to project scope:

- The project seems good.
- The project should take feasibility of vehicle storage into account if possible.
- The project should be appropriately titled to indicate the focus area. Given the small budget allocated, the scope cannot be increased to cover the entire pathway.
- The project should expand the scope to 1,000 kg/day capacity stations to have broader applicability in the future. The current inclusion of an upper limit of 800 kg/day capacity station can be expected to be too low for future usage (particularly in medium- and heavy-duty vehicle fueling settings). The project should consider assessment of mitigation strategies for boil-off (in addition of what was presented in reviewer-only slides).
- It would be interesting to also calculate the Levelized Cost of Energy of the full pathway and compare it against other hydrogen pathways. The project should ensure that the cost analysis is in alignment with financial models such as H2FAST

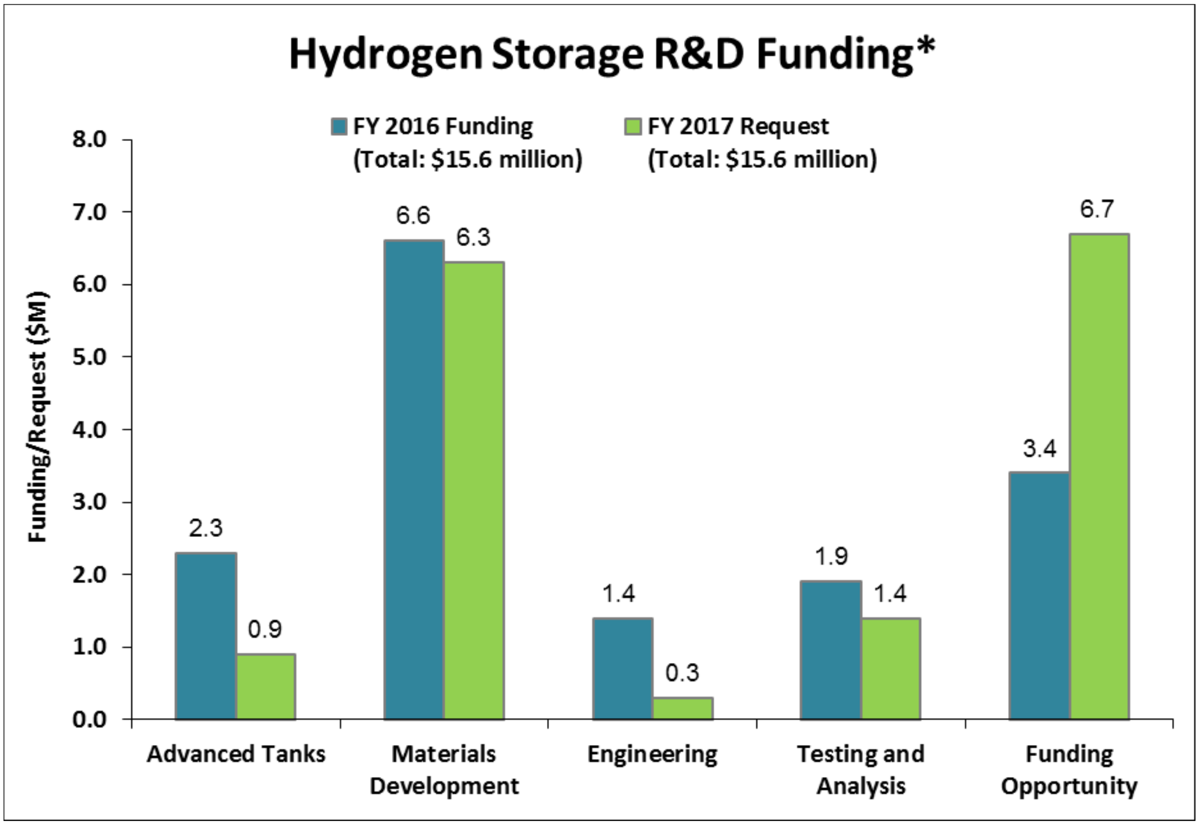
2016 — Hydrogen Storage
Summary of Annual Merit Review of the Hydrogen Storage Program

Summary of Reviewer Comments on the Hydrogen Storage Program:

In fiscal year (FY) 2016, the Hydrogen Storage program portfolio continued to focus on onboard automotive applications through its two-pronged strategy, pursuing strategic near-term and long-term pathways with the potential to meet the cost and performance targets. Reviewers commented that the program is well managed and has been successful in benchmarking progress across its research and development (R&D) portfolio. Reviewers commended the program for ensuring the R&D work remains relevant to the market but also focused on meeting the ultimate targets. The launch of the Hydrogen Materials—Advanced Research Consortium (HyMARC) was cited as one of the program's more notable efforts for the year, and the HyMARC was commended for its emphasis in applying the discoveries from previous materials-based efforts, specifically those from the Hydrogen Storage Engineering Center of Excellence (HSECoE), to address remaining gaps blocking the advancement of materials-based hydrogen storage. With the introduction of HyMARC, reviewers stated the importance of careful coordination across individual projects and related efforts to HyMARC to prevent overlap in activities and maximize results. Reviewers remarked that the program has effectively enabled meaningful collaboration across projects as well as among national laboratories, industry partners, and academia but recommended encouraging partnerships that enable technology commercialization. Overall, the reviewers commented that the program's R&D portfolio is appropriate and comprehensively addresses key technical aspects needed to achieve the ultimate program targets. They noted that greater emphasis should be placed on developing strategies that enable the technology's potential to be commercial in today's market.

Hydrogen Storage Funding:

The chart on the following page illustrates the appropriated funding planned in FY 2016 and the FY 2017 request for each major activity. The program received $15.6 million in funding in FY 2016, and it has a budget request of $15.6 million for FY 2017. In FY 2016, HyMARC, the National Renewable Energy Laboratory-led validation and characterization effort, and various individual projects were launched to advance research on the discovery, development, and validation of novel materials with the potential to store hydrogen and meet the targets. Additional efforts on advanced compressed hydrogen storage systems were initiated in FY 2016.



* 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 Hydrogen Storage portfolio was represented by 24 oral and 5 poster presentations in FY 2016. A total of 23 projects—via oral presentations—were reviewed. In general, the reviewers' scores for the storage projects were good, with scores of 3.4, 2.0, and 3.0 for the highest, lowest, and average scores, respectively.

Advanced Tanks: Five projects on advanced tanks were reviewed, with a high score of 3.4, a low score of 2.5, and an average score of 2.9. Reviewers considered these projects to be relevant in addressing program needs. For the tank cost reduction projects, reviewers commented favorably on the efforts that employ a good mix of modeling and experiments to validate modeled predictions on cost-reduction pathways through fabrication and testing of real systems, including alternative fiber and resin, low-cost balance-of-plant components, and a novel conformable tank. Reviewers also noted the potential increase in hydrogen capacity offered by cold/cryo-compressed technologies, but they emphasized the need for continued temperature/pressure cycling as well as additional emphasis on the vacuum jacket insulation and related issue of hydrogen dormancy. In general, reviewers recommended more detailed and validated techno-economic assessments. Overall, the reviewers thought the efforts could have a significant impact on the industry.

Materials Development: Fourteen materials-based hydrogen storage projects were reviewed, with a high score of 3.3, a low score of 2.0, and an average score of 3.0. In general, reviewers complimented the unique capabilities developed and technical progress made through the wide range of projects in the program's materials development portfolio. Reviewers commented on the potential of the newly established HyMARC and characterization and validation efforts to address the critical scientific gaps in the field and enable the development of storage materials with a realistic chance to meet U.S. Department of Energy (DOE) onboard storage targets that cannot be theoretically met by high-pressure hydrogen storage tanks. Reviewers agreed that the mix of projects in this area is adequate to address the technical challenges specific to both non-automotive and portable applications.

Reviewers recommended a reduction in work on materials that have already been investigated extensively in previous years and that have not shown any potential to meet the most technical and challenging DOE targets. Material projects will continue in FY 2016, subject to appropriations, and new projects will be initiated. Through close collaboration with the HyMARC and the validation and characterization efforts, new projects will have 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.

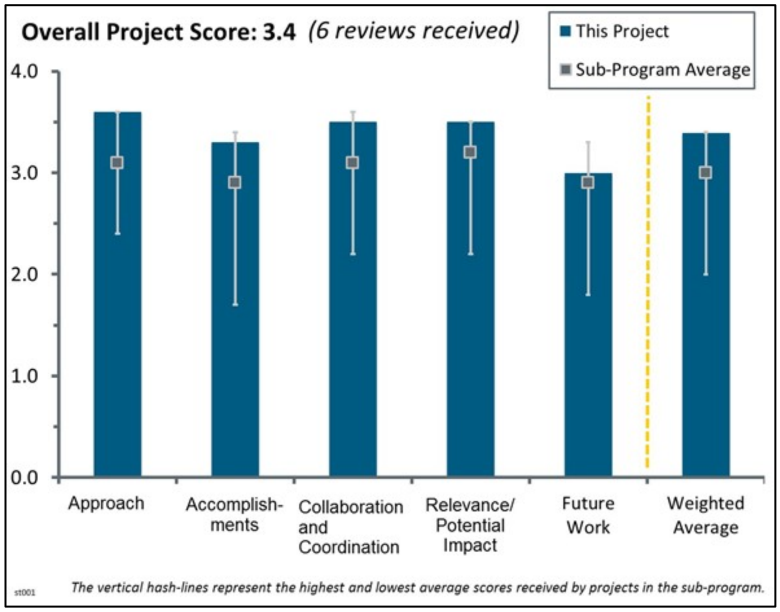
Engineering: Two projects related to hydrogen storage engineering were reviewed, both with a score of 3.3. The reviewers were very satisfied with the approach and accomplishments of the HSECoE and stated that their findings were of utmost relevance to the overall Hydrogen Storage program. They felt that the large group of partners was sufficiently diverse and collaborations were well organized and beneficial for the projects. The reviewers also specified that making the modeling package available to the community was very significant, and that the data obtained on the storage systems will provide a solid foundation for development when a suitable material emerges. The reviewers commended the efforts to enhance the performance and user-interface of the models and stated that it is important to preserve the wealth of information and understanding of engineering concepts and required hydrogen storage material properties developed during the HSECoE.

Testing and Analysis: Two projects related to testing and analysis were reviewed, with a high score of 3.4, a low score of 3.2, and an average score of 3.3. Reviewers stated that these projects are very relevant in assisting DOE's R&D portfolio evaluation. Reviewers commended the performance analysis project for providing unbiased analyses of hydrogen storage options, showing depth in technical evaluation across multiple storage approaches. Reviewers also commended the work on system/material trade-offs, assessing design variations and engineering features for diverse hydrogen storage systems and materials, highlighting areas that either have potential for improvement or are already constrained to current values. However, reviewers cautioned that the assessment of a high-pressure metal hydride storage option needs greater emphasis on overall thermal management issues of the charging performance. For cost analysis, reviewers commended the project's in-depth analysis, including an uncertainty analysis that vets and captures potential cost reduction concepts. Reviewers recommended adding features such as certification costs, tank finishing/rework, and scrap costs to the analyses.

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

Rajesh Ahluwalia; Argonne National Laboratory

Brief Summary of Project:



The main objective of this project is to develop and use models to analyze the onboard and off-board performance of physical and materials-based automotive hydrogen storage systems. Specific goals include conducting independent systems analysis for the U.S. Department of Energy (DOE) to gauge the performance of hydrogen storage systems; providing results to materials developers for assessment against system performance targets and goals and for guidance in focusing on areas requiring improvements; providing inputs for independent analysis of onboard system costs; identifying interface issues and opportunities and data needs for technology development; and performing reverse engineering to define material properties needed to meet the system-level targets.

Question 1: Approach to performing the work

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

- This ongoing project continues to provide both DOE and the storage community with valuable systems-level analyses of various aspects of onboard hydrogen storage. This project also serves to validate some crucial areas in which this project and the Engineering Center of Excellence have overlapping tasks. The “reverse engineering” approach has provided valuable input to materials research and development (R&D) efforts; contributed to understanding where costs and mass can be saved in various aspects of physical storage in tanks and in “hybrid” tanks containing metal hydrides, i.e., the Toyota hybrid tank concept; and provides independent validation/review of various aspects of system models developed by other DOE-funded storage programs. It is clear that this effort is enhanced through its other analysis efforts, e.g., the fuel cell analysis program. Having such an excellent background in all of the cross-cutting technologies in the Fuel Cell Technologies Office makes this a particularly valuable team.
- Regarding the approach to performing the work, the principal investigator (PI) provided an overview of two focus areas: analysis of carbon fiber tanks and preliminary analysis of high-pressure hydrides. The high-pressure hydride work is a great example of using reverse engineering to provide some guidance to the materials researchers.
- A logical and innovative approach for development of physical, thermodynamic, and kinetic models has been adopted to understand properties and processes in hydrogen storage systems. The approach also involves the analysis of system/material trade-offs, information that is crucial to developing comprehensive and effective models. The approach is keenly focused on important barriers and obstacles.
- This project serves a very useful role by independently assessing design variations and engineering features for diverse hydrogen storage systems and materials. While this information does not explicitly contribute to possible improvements in hydrogen storage systems, it does highlight areas that either have potential for improvement or are already constrained to current values. Most major factors have been considered and also reevaluated during the nearly eight years the project has been ongoing. Hence, even though the approach remains sound, it is not clear whether significant novel options can result.

- Use of the Abaqus modeling tool has provided helpful guidance for estimating key hydrogen storage system performance metrics.
- The project provides valuable, unbiased analysis of various hydrogen storage options under consideration by DOE.

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.

- During the past year, the Argonne National Laboratory (ANL) team continued assessing alternative configurations for Type IV 700 bar tanks, where most variations were found to have relatively minimal impact on increasing performance levels against the DOE targets. It was most interesting to see ANL's evaluation of the Toyota Mirai storage vessel when compared to U.S. domestic designs. While Dr. Ahluwalia stated during the question-and-answer portion that the 40% mass reduction claimed by Toyota was for a non-optimized baseline tank, the ANL results indicated a much smaller improvement to current (quasi-optimized) vessels being used in other vehicles. The new task for evaluating high-pressure metal hydride vessels with a 350 bar operating pressure was very comprehensive, but it was difficult to extract the key results with regard to requirements being necessary to match performance of a 700 bar compressed gas tank. Although a number of properties were rapidly covered via multiple figures, summarization of metal hydride was rather sketchy and should be clarified.
- Solid progress has been made in all project areas: physical systems, high-pressure metal hydrides, sorbents, and chemical hydrogen systems. Especially noteworthy results include (1) establishment of new performance metrics for 700 bar hydrogen tanks, (2) development and validation of improved tank design concepts, (3) identification of potential ways to reduce the quantity of carbon fiber and resin in compressed gas tanks, and (4) analysis of thermodynamic requirements for high-pressure, low-enthalpy metal hydrides capable of enhancing the performance of compressed hydrogen tanks. These are all important results that have an impact on DOE decisions about hydrogen storage system development.
- This project continuously provides valuable feedback, validation, and review of a wide-ranging array of technologies surrounding the multiplicity of approaches to onboard hydrogen storage. This requires chemical engineering, mechanical engineering, and cost analysis techniques, among others, and this team carries out the application of these various analyses very well.
- Very good progress has been made related to the hybrid high-pressure tank and 700 MPa Mirai system.
- The project showed depth in technical evaluation across multiple storage approaches.

Question 3: Collaboration and coordination with other institutions

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

- Extensive collaborations with national laboratories and several private companies and original equipment manufacturers are enhancing the impact and progress in this project. This project has been carried out successfully over the last several years, and it is apparent that the close collaborations between the PI and cooperating partners have augmented the total effort.
- This ANL team does interact well with the other organizations via both effective interchanges of technical inputs and communicating the team's outputs.
- Without superior collaboration, this project would not succeed. Because it does succeed exceedingly well, it follows that the collaboration among the various projects within the Hydrogen Storage program and the ANL analysis effort must be outstanding.
- The project contains a comprehensive list of partners and is well positioned to meet the scope of the program goals.
- Current activities did not have external collaborators looking at materials. The project was more focused on directions from DOE on analysis of Type IV tanks and new concepts in high-pressure materials to learn whether there are new opportunities to explore.
- There is visible collaboration with other institutes.

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 unique in the DOE portfolio; i.e., as stated in the presentation, "project continuation and direction is determined annually by DOE." Based upon the project's long-standing success and continuing DOE support, it is readily apparent that the project is critical to the Hydrogen and Fuel Cells Program (the Program), and it is clearly advancing progress toward achieving DOE goals and objectives.
- The project is very useful in obtaining hydrogen storage performance metrics that help evaluate existing storage methods and possible improvements that could be implemented.
- The technical and cost review and analysis offered by this project serve as valuable tools for vetting various hydrogen storage approaches so that investments can be placed where payoff is highest.
- Without the feedback from these analyses and reverse engineering efforts, the materials and physical-based storage projects could not be as effective.
- The potential of this effort for generating novel improvements is likely limited because most variations for hydrogen storage systems have been considered. From current and past analyses by the ANL project and others, there are virtually no known solid storage media candidates that can simultaneously satisfy the 2020 DOE targets, let alone the ultimate values. ANL and others have found over the past decade that the variety of design features is always a compromise of contradictory requirements and behavior for either physical or chemical storage systems. Perspectives for finding a breakthrough system simultaneously meeting all 2020 vehicle targets have low probability.

Question 5: Proposed future work

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

- The proposed future work on physical and materials-based storage follows logically from the current effort. The project team clearly recognizes the importance of terminating activities as needed and documenting/ publishing relevant R&D results. The continuing focus on physical storage is appropriate. New results and understanding of emerging material systems will be derived from the consortium projects led by the Hydrogen Materials—Advanced Research Consortium (HyMARC) and the National Renewable Energy Laboratory, and it is anticipated that as these results become available, they will be used to guide future work in this effort.
- The project is doing excellent work to help others better understand materials needs from reverse engineering of systems. The team should keep up efforts to publish results in a timely fashion.
- The future plan is very good.
- In many ways, the analysis efforts are tied to the Hydrogen Storage program needs, so it is understandable that sometimes this project has future plans that are "to be decided." What is presented as future plans is logical. There may be some need to re-examine whether the future effort in high-pressure metal hydrides is the best use of ANL resources, as the initial analyses appears to indicate that there are no known materials that have the desired properties.
- Future work should include not only technical review but also market opportunity and guidance for potential future supply chain investments. The Institute for Advanced Composites Manufacturing Innovation's commercialization efforts require a techno-economic model to drive industry investments.
- There should be less emphasis on continuing the analyses of Type IV 700 bar storage vessels and more attention paid to an enhanced scope of treatment on solid storage materials.

Project strengths:

- This ongoing project has been making valuable contributions to the Hydrogen Storage program for several years. The PI and his team provide DOE with expert and timely systems analysis of problems directly relevant to both the short- and long-term needs of the program. Independent analysis of this kind is a valuable component of the overall Program.

- This is an excellent team with a long history of contributions and of doing important work for the Program. The team is highly skillful at addressing a variety of types of engineering analyses. At times, the “reverse engineering” approach may appear to be too general, but the project continues to provide valuable insight to those who need it most: the researchers.
- The ANL team has developed and implemented a variety of models for assessing and predicting the attributes and limitations of nearly all types of hydrogen storage systems. The team provides valuable constraints required from various storage media.
- This is an experienced team that works hard to provide insight into many if not all of DOE’s projects.
- The approach and concepts are useful and have been validated on some occasions.
- The project was comprehensive in review of ongoing technical work.

Project weaknesses:

- Given the current status of metal hydride and chemical hydrogen storage materials, continued work on systems that employ those materials is probably not appropriate, and it is diluting the overall impact of the project. The project should be sufficiently “nimble” to straightforwardly accommodate analysis of those systems as improved materials emerge in the future. (Note: this is not really a “weakness”—the project team should consider it as an observation/recommendation.)
- Because the ANL team does not have resources for direct experimental characterizations and verification of hydrogen storage candidates, the team must rely upon literature and other outside sources for input parameters during the system analyses. Often critical property values are either not available or are unreliable, which can have an impact on the predicted results. It should be noted that only a couple of candidate metal hydrides occupy the desired enthalpy–entropy region in the figure on slide 14. The team should actively seek out experienced researchers both to provide sources of other data and to participate in periodic detailed technical discussions and review to critically adjudicate the project’s predictions.
- The project does not make suggestions for further technical work that would help establish business opportunities for further investment.
- The project is modeling-based and uses several assumptions.

Recommendations for additions/deletions to project scope:

- One of the general outcomes of the Engineering Center of Excellence was a recommendation of materials that had a targeted enthalpy in the range of 20–27 kJ/mol H_2. It would be interesting to see a similar analysis of the entropy range that would be necessary to meet DOE targets based on equilibrium pressure needs and kinetics—similar to what was performed for the high-pressure hydrides presented at the Program’s Annual Merit Review. It is understood that the Program needs to find storage solutions across all vehicle platforms. It would be optimal to have a “single” solution; however, perhaps a reverse engineering approach could provide some guidance about differences between an optimized system for small/subcompact vehicles on the one hand and large sport utility vehicles on the other hand. The high-pressure hydrides might work for certain vehicle classes better than others. Chemical hydrogen storage (CHS) provides an approach to some of the highest gravimetric and volumetric density of hydrogen, yet CHS faces significant challenges for onboard storage from an engineering need. Given the high volumetric and gravimetric density, perhaps there are some findings of the reverse engineering performed on CHS for onboard storage that could be transferred—or considered—as an approach for hydrogen delivery.
- In light of the minimal improvements found during the past few years for the nominal 700 bar compressed hydrogen vessels, further systematic analyses are not recommended because little payback can be expected. On the other hand, the suggested evaluations (presumably comprehensive of onboard and off-board behavior) of cryo-compressed vessels for fleet vehicles should be worthwhile. The assessment of the high-pressure metal hydride storage option needs to be completed with greater emphasis on overall thermal management issues for the charging performance and requirements on the manufacturability and hydrogen-charging infrastructure. A more in-depth comparison of optimized Type III versus Type IV vessels for hydride-base tanks should be made to see which has the better adaptability and lowest cost. The team should make clear the benefits, limitations, and trade-offs necessary to achieve optimal efficiency and economic value. As an example, if the cost of producing and processing the metal hydride alloy is greater than the cost benefits from reducing 45+ kg of high-strength carbon fiber for the vessel containment, there

appears to be little incentive to consider hydride tanks over 700 bar compressed gas vessels other than niche applications.

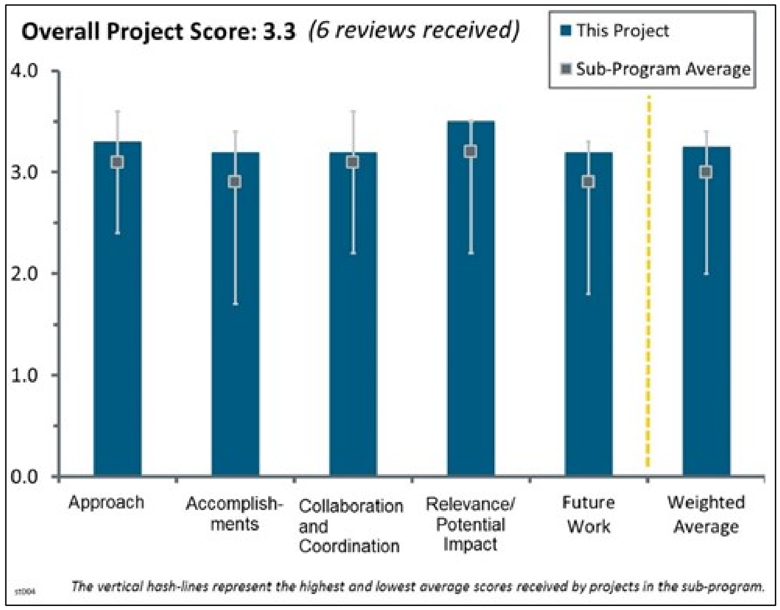
- Having another look at the hybrid tank issue is interesting, and perhaps this comes from the Program management, but it is not clear that there is hope for this approach—or perhaps it was not clear from the presentation what the justification is for this aspect of the future project.
- The project should de-scope work on metal hydrides and chemical hydrogen systems until reversible material candidates that at least approach the DOE storage performance targets emerge.
- The project can make suggestions for further technical work and help establish business opportunities for further investment.
- Including some sensitivity analyses of key parameters is recommended.

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's goal is to design innovative materials-based hydrogen storage system architectures with the potential to meet U.S. Department of Energy (DOE) performance and cost targets. Savannah River National Laboratory (SRNL) will develop and validate system, engineering, and design models that lend insight into overall fuel cycle efficiency. All relevant materials data for candidate storage media will be compiled and required materials properties defined to meet the technical targets. SRNL will also design, build, and evaluate subscale prototype systems to assess the innovative storage devices and subsystem design concepts, validate models, and improve component design and predictive capability.



Question 1: Approach to performing the work

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

- The participants of the Hydrogen Storage Engineering Center of Excellence (HSECoE) demonstrated that the HSECoE approach is a very valuable construct to move very difficult, complex problems forward. The materials-based hydrogen storage area is one of those very difficult problems that have benefited tremendously from the HSECoE approach that brings the best expertise to bear on a set of problems. The presentation outlined one such example: comparing and contrasting the engineering issues and systems issues surrounding the potential utility of adsorbent materials operating at sub-ambient temperatures. The HSECoE provided a very systematic look at two different adsorbent system configurations, which yielded interesting details of the pros and cons of each approach and the impacts on system design and their relative abilities to meet the DOE targets. The failure mode and effects analysis (FMEA) approach the HSECoE adopted from original equipment manufacturers (OEMs) also provided an engineering culture method to demonstrate how project prioritization was helping to more effectively achieve project goals. In the end, the engineering assessments and modeling approach provides critical input to the "next generation" of materials-based storage projects, e.g., the Hydrogen Materials—Advanced Research Consortium (HyMARC).
- The approach, which builds on years of prior work in the HSECoE, combines experimental testing of two heat exchanger systems with detailed heat transfer modeling of these. The integration of these is very good, allowing models to be validated.
- This is a long-term project conducted in a very systematic and professional manner. The project significantly contributed to identifying and helping to solve critical barriers.
- The approach describes aspects of the work that were covered in 2015, but given the nature of the no-cost extension and the fact that this project was initially designed to end last calendar year, this is not surprising. Somewhat more data on the modular adsorbent tank insert (MATI) system was presented that described cycling characteristics, noting that no degradation of capacity was seen.
- This project is concluding in 2016. The approach during this reporting period focused on addressing remaining adsorbent engineering issues—mainly completion of the MATI and Hexcell heat exchanger work. This is important because it provides the basis for down-selecting an optimum heat exchanger for

adsorbent systems for the final prototype storage system. The approach is straightforward and is keenly focused on characterizing and evaluating the two heat exchangers for practical system applications.
- The HSECoE was initially tasked with developing onboard reversible hydrogen storage systems. Although the initial goal was to engineer prototype tanks for all three main classes of hydrogen storage systems, the majority of the research efforts were focused on designing and engineering cryogenic Hexcell and MATI sorbent tanks. The future plans include tests under more realistic conditions of temperature for a practical sorbent system (160 K and above), yet the tests on prototype systems were performed exclusively at temperatures between 80 K and 90 K.

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.

- As this Center of Excellence wraps up, the work presented demonstrated good to excellent progress toward its final goals, particularly in the sorbent system work, and in ensuring that the enduring online access to the models is maintained and that the models are now more "user-friendly." Without providing for an enduring "online presence," the HSECoE would not have the desired impact. Therefore, the progress is good to excellent.
- Solid progress was achieved on evaluating the MATI and Hexcell heat exchanger options or adsorbent materials. An especially noteworthy result is that a system employing MOF-5 adsorbent pucks compacted within a MATI heat exchanger in a Type I tank exceeds the volumetric and gravimetric capacity obtained from compressed hydrogen at 700 bar in a Type IV tank. However, a higher-temperature metal–organic framework (MOF) is needed, and the small performance advantage obtained with the current MOF-5/MATI system is likely outweighed by the complexity of that approach compared to the compressed gas option. The FMEA for an engineering system comprising MOF-5 adsorbent in MATI and Hexcell heat exchangers provides useful information concerning limitations of the two options in a practical, real-world storage system. Likewise, the clear articulation of remaining sorbent engineering issues and obstacles provides a focus for future work. Overall, even though an optimum material system meeting all DOE targets was not available, the HSECoE team has done a first-rate job of developing and evaluating engineering subsystem options that incorporate the principal classes of emerging materials systems.
- Of most value was the assessment that, while short of DOE requirements, the material class of promise was adsorbents. While the development of new materials or materials systems cannot be anticipated, lack of information on costs associated with forecourt delivery requirements at low temperatures is lacking (and outside of the scope of the HSECoE). Some guidance from DOE on the practicality of low temperature from a cost standpoint may be required in order to fully gauge the practicality of such systems. Given the present limitations of other materials, especially complex hydrides, in requiring high temperatures for significant hydrogen release, adsorbents may be the only game in town. The work describes some of the difficulties encountered with the substitute MATI system that suffered from brazing issues and so did not fully reflect engineering data of an optimized heat transfer design. The personnel changes at Oregon State University that were mentioned are unfortunate. Whether this was a student who graduated or technical help that moved on, some contingency would have been of value.
- The project accomplished much-needed analysis and experimental objectives over its seven-year lifetime. However, after considerable monetary expenditure, the MATI and Hexcell systems were shown to (potentially) perform only slightly better than compressed gas. The project was able to demonstrate this potential and thus was a success. However, in the end, a new form of hydrogen storage with significantly improved performance was not identified.
- The main accomplishments seem to be in the area of modeling the physical hydrogen storage behavior in materials. The predictive models of evaluating the impact of various parameters and physical properties of materials on the technical targets are certainly valuable. Unfortunately, most of the models were not validated experimentally. For the prototype systems built based on MOFs, the main efforts were focused on probing the behavior at cryogenic temperatures, which scientifically is of interest, but unfortunately, it is far from the technical targets of a practical storage system. It is regrettable that no tests were performed under more realistic temperatures.

- Milestones were mostly met for the Hexcell system but not for the MATI system (modeling is incomplete at this stage). This is understandable, however, in light of the problem with the brazing on the initially delivered MATI unit. A manufacturing defect was discovered (brazing had wicked into passages in the MATI and blocked nitrogen flow). Off-line non-destructive evaluation (NDE) analysis revealed this problem. Although unanticipated, identification of this problem provides a useful heads up for future manufacturing involving this design. A large array of tests was done for both subscale systems and the full 2 L adsorption unit, providing a substantial body of data for model validation. Cycling tests also help to understand system performance under realistic conditions as well as repeatability and durability. Remarkably good agreement of experimental data with models is found. For Hexcell discharging, the team had to periodically shut off hydrogen flow because the system was heating too much. It is not clear whether this is a design flaw that needs to be fixed. The full-scale Hexcell system would produce 3.2 wt.%, and the MATI system would give 3.1 wt.% (surpasses 700 bar tank). The team completed the FMEA and developed the full-scale Hexcell system design concept, and the model is complete. All reviewer comments from the previous DOE Hydrogen and Fuel Cells Program (the Program) Annual Merit Review appear to have been adequately addressed.

Question 3: Collaboration and coordination with other institutions

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

- Without great collaboration among participants and with project management, the HSECoE would not have succeeded. As the HSECoE did succeed, the collaboration must have been excellent.
- The team tied together numerous institutions in a productive manner. The team was sufficiently diverse to provide adequate representation of skills and points of view.
- Although collaborations were not summarized in this final presentation, extensive and highly beneficial collaborations and cooperation both within and outside the HSECoE have enhanced the progress throughout the duration of this project.
- This is a large team (13 organizations). From the presentation, it was not clear what the role of each partner is, although logos on various slides indicate contributions from organizations outside SRNL. It does not appear that all of the partners are still contributing, but considering that this year was an extension of the project, this is understandable.
- There seems to be good collaboration and coordination within the HSECoE, but not much communication and collaboration with outside institutions and principal investigators.
- This effort was originally set up with a number of collaborations in place. There appears to have been some difficulty with the speed with which implementation and feedback among various institutions could take place. For projects of this type, design and construction of various components may have been better off had they been outsourced.

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 relevance of an engineering center of excellence to DOE goals could not be greater. There is no higher calling within the Program than to provide systems-level analyses and feedback to current and future materials-based efforts. As the HSECoE succeeded in providing such feedback through analyses and through various system-level models that have been or are nearly complete, it has provided an outstanding degree of technical impact.
- The information that was gleaned from the HSECoE activity offered valuable data on systems and designs of potential interest and was especially valuable in highlighting materials requirements needed to achieve the range of metrics as demanded by fuel cells.
- This project was a logical follow-on to the various materials centers of excellence and has generated a great deal of useful practical information. In addition, modeling tools and data are being made available for others to use through the HSECoE website, enabling the impact to extend beyond the HSECoE partners.

Engineering challenges remain, but it is likely that further significant progress will depend on the identification of a material that can meet the DOE targets.
- This is clearly an important project for the Hydrogen Storage program. It provides a solid foundation for development of engineering prototypes and storage systems in the future.
- This HSECoE effort is relevant to achieving the DOE technical targets as it provided important guidance for the materials discovery efforts. The potential impact could have been even higher if the prototype systems were tested under more realistic conditions of temperature and hydrogen pressure for practical sorbent systems.

Question 5: Proposed future work

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

- The work that remains is to complete the modeling work in a way that makes the package generally available to the community, and the plan for completing this is hopefully on track.
- As this is the closeout, this category is not terribly applicable, but to the extent that the HSECoE has provided for enduring access to its online information and models, its contribution is excellent.
- The remaining engineering issues were clearly summarized. Some materials-related issues also exist (e.g., compaction of MOFs), but identifying a specific material to focus on should come before investing a great deal of effort into these.
- There is some value in finishing up the proposed modeling work. However, it is suspected that the models will only be useful to the team if research activities related to MATI/Hexcell continue. If they do not, the models will likely be a waste of time.
- The future proposed efforts seem to be exclusively focused on sorbents. It would be desirable to include modeling of other classes of hydrogen storage materials, such as chemical hydrides and reversible metal hydrides.
- The reviewer did not score this area—the project is concluding in 2016 (except for ST-008 on documentation, testing, and enhancement of storage system models). Consequently, a review of future work is not really relevant. However, the HSECoE team has provided an excellent summary of remaining issues and obstacles that affect successful development of an engineering prototype. These can be viewed, at least in part, as a good basis for a possible follow-on effort in the future.

Project strengths:

- Project strengths include the well-organized, multidisciplinary team involving laboratories, industry, and OEMs. The project has a good combination of experiments and modeling. There is a concerted effort to make results widely available.
- The HSECoE is, overall, well organized. The research efforts have been focused on addressing the most significant engineering challenges associated with developing materials-based hydrogen storage systems for hydrogen vehicles. The main strength of the project is that it accomplished one of the original objectives in designing and evaluating prototype solid-state hydrogen storage systems; the data obtained for sorbent systems seem to be the most useful for future research and engineering efforts.
- The project combines both theory/modeling and experimental/engineering components in a synergistic way. The project team is well qualified to conduct the engineering and development work on this project. The project is well managed, and extensive collaborations and cooperation are evident.
- This is a professionally executed and complex project.
- Project strengths include the collaboration, great team, systematic approach, detailed analyses, and accepted feedback from multiple stakeholders. It took a while, but this Center of Excellence turned out okay.

Project weaknesses:

- In the end, there were none.
- No significant ones are identified.

- As noted both by reviewers and by the HSECoE project team members, the overarching weakness was the lack of a storage material capable of meeting DOE targets. Despite that, the HSECoE team did an excellent job of evaluating existing materials and developing engineering subsystems that employ the best candidate materials that are currently available. That work provides a solid foundation for development of an optimized engineering system if/when a suitable material emerges.
- The HSECoE seemed to lack a comprehensive and well-defined research plan. Certain areas of research, such as thermal energy management, seem to be studied rather extensively; but some other areas, such as materials compatibility, potential reactivity (especially for metal and chemical hydrides), and hydrogen purity, remain poorly understood and require more investments. It is quite difficult to sort-out why certain particular compounds (instead of others) were selected for study. The approach seems semi-random; no justification was given for why certain research activities were prioritized versus others.

Recommendations for additions/deletions to project scope:

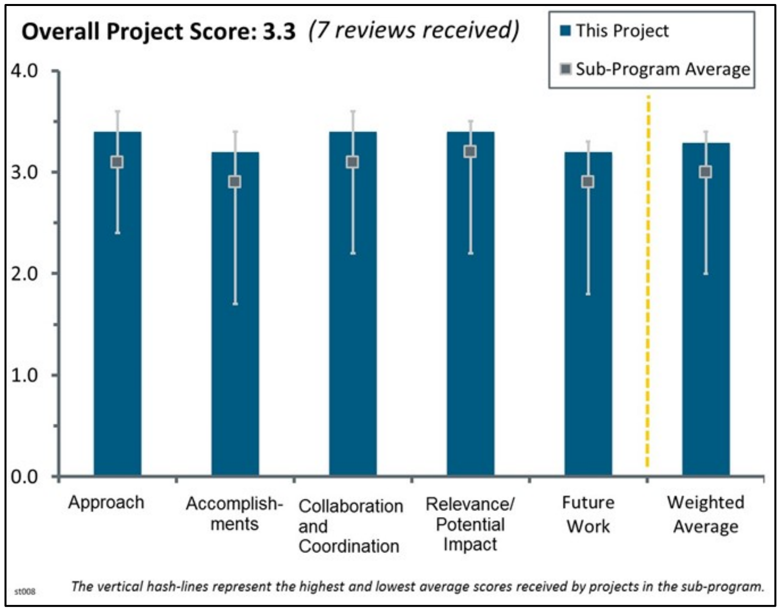
- The sorbent hydrogen isotherm data fitting effort is valuable and should be continued. It would be desirable to expand the models to other classes of hydrogen storage materials, such as reversible metal hydrides.
- The project should consider whether completion of the modeling efforts is needed. Performance data for 100 L/minute and 500 L/minute are reported for the Hexcell system. However, the station impact of these two flow rates should be assessed.
- The project is ending in 2016. Consequently, there are no recommendations for revision of project scope.

Project #ST-008: Hydrogen Storage System Modeling: Public Access, Maintenance, and Enhancements

David Tamburello; Savannah River National Laboratory

Brief Summary of Project:

The ultimate goal of this project is to provide and enhance publically available material-based hydrogen storage system models that will accept direct material property inputs from material developers to accurately predict material-based hydrogen storage system performance. In support of that goal, this project maintains, enhances, and updates the Hydrogen Storage Engineering Center of Excellence (HSECoE) hydrogen storage system modeling framework and model dissemination web page.



Question 1: Approach to performing the work

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

- Managing and documenting the models developed in the HSECoE project is critical to ensure that the project continues to have an ongoing presence and impact in future years. The approach adopted in this project provides an effective means for external investigators to access and employ HSECoE models in a straightforward way. Several useful models and simulation modules and frameworks have been developed. The approach successfully addresses the multiple challenges underlying successful development and deployment of those models. The project will provide investigators with the opportunity to input new materials properties and to assess the associated system characteristics. This will be an important legacy of the HSECoE effort.
- The approach sounds practical and user-friendly. It is good to see there is a focused effort to distribute results of the HSECoE and maintain their availability, which is always a challenge after a project ends. The ultimate goal of allowing users to input their own material property data is excellent.
- This project is an extension of a portion of the HSECoE consortium effort that is continuing and updating the predictive modeling techniques for alternative hydrogen storage media (e.g., metal hydrides, chemical hydrogen, and adsorbents) with some complementary baseline models for compressed gas storage. This task is currently focused on performance enhancements of example media by allowing impact of varying relevant materials properties within specific storage vessels evaluated during the HSECoE project. In essence, these online models should allow outside users in the international hydrogen research and development community, who possess the appropriate software, to make comparisons over a range of parameters and operating scenarios against reference materials. The objective is to assist these researchers to identify viable candidates with the potential to meet the U.S. Department of Energy (DOE) vehicle performance targets. The project will continue provide a level of technical support to the model website to assist outside users.
- Ongoing efforts are focused on making “models” for sorbents and chemical hydrogen storage materials available to the research community to provide a rational approach to compare different materials and better understand how different materials properties will affect vehicle performance.
- The approach helps scientists and engineers to identify opportunities and challenges with materials from tank and system points of view.
- This presentation represents the completion or near-completion of vehicle modeling for various materials categories, accomplished with a somewhat limited data set based on available materials.

- The relatively easy-to-use modeling programs are the best possible means by which to make the conclusions of the HSECoE available to the materials hydrogen storage community. The project lacks an effort to obtain feedback from users and then to make appropriate adjustments to better facilitate use.

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.

- Overall accomplishments are substantial and address the needs of potential users. Good progress is being made on all models, and website support, access to document models, and data are provided, including user manuals. Available codes include a metal hydride acceptability envelope. The project is also providing a framework model with physisorption and metal hydride storage, etc.; a chemical hydride storage model and tank volume/cost model (raw materials only); sorbent models that will be made flexible to consider materials other than metal–organic framework (MOF)-5 (the same for the chemical hydrogen storage model); and a model that allows one to assess the quality of isotherm data and show where additional data are needed. This progress is remarkable considering the relatively small amount of funding for the project.
- Good progress toward all project and DOE goals was made in this reporting period. A large number of varied and useful storage system models either have been posted or are projected for release in the near future. The models are being updated and validated as needed. Good progress has also been achieved on developing and testing a comprehensive hydrogen vehicle simulation framework. The project team is working to make the user interface as “friendly” as possible, ensuring that the models will find wide acceptance in the hydrogen storage and fuel cell electric vehicle communities.
- This project started officially at the beginning of fiscal year 2016 and is making steady progress in updating, refining, and maintaining the HSECoE model dissemination website. Steady international interest and activity with the website was indicated from the tracking statistics given during the DOE Hydrogen and Fuel Cells Program Annual Merit Review presentation. Plausible comparisons between the adsorption system models and the laboratory tests on the two configurations for the storage beds were shown, along with scale-up for modeling full-size beds (i.e., circa 5.6 kg of hydrogen capacity). Nevertheless, both the HexCell powder and Modular Adsorption Tank Insert pellet systems fail to meet the desired 2020 targets. Simulations are helpful tools in understanding behavior but may not hasten discovery or development of the specified targets.
- The software capabilities developed at this stage allow for exploring different materials, apart from adsorbents.
- Excellent progress has been made. The development of the modeling programs appears to be on track.
- There is strong collaboration between laboratories to make models “user-friendly” for researchers developing new materials for onboard storage.
- At present, the model relies heavily on MATLAB. One hopes that enough documentation will eventually be provided to better judge the transparency of the code. As it stands, the data that were presented appear adequate in projecting the performance of the two adsorbent designs that were ultimately built.

Question 3: Collaboration and coordination with other institutions

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

- Team members from different nations, as well as outside members, had very close interaction with a number technical interchanges throughout this project. The modeling programs and operating software are the results of dedicated team effort between the laboratories, reflecting excellent collaboration.
- The three laboratories appear to be collaborating well. It is difficult to gauge the actual extent of the interactions, but judging from the extensive results presented, these interactions are occurring and are effective.
- This project on modeling is one of the best examples of how collaboration can lead to accelerated progress on a complex, challenging problem.
- Close collaborations among several partners in the HSECoE are evident. The project is managed and coordinated well. The collaborations are clearly augmenting the overall progress.

- There is visible collaboration among other stakeholders at the HSECoE.
- There is a strong collaboration among team members. Effort should include "outside" users that are not former HSECoE members.
- This effort relied on input from University of Quebec at Trois-Rivieres and Oregon State University in order to validate the models used. Otherwise, the effort was conducted for the most part within Savannah River National Laboratory with some outside testing by project consultants.

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 website is attracting good attention—it looks like there are ~200+ regular users. Codes are being downloaded, although it is not clear how (or whether) they are being used. Nevertheless, the access numbers indicate that there is serious interest in the information and tools that are available here. Kudos to the team for enabling this user-friendly access. This high level of interest is also testament to the significance of the results achieved by the HSECoE.
- This project is an important adjunct to the overall technical effort in the HSECoE. Proper documentation and distribution of user-friendly models developed in the HSECoE project are essential to ensure that the project has an impact on future development and testing of engineering systems. As such, the project strongly supports the goals and objectives of the DOE Hydrogen and Fuel Cells Program.
- Ultimately, the HSECoE's relevance will be determined by the extent to which results from the center are utilized by future investigators working on the development of hydrogen storage materials and materials-based hydrogen storage tanks. Thus this project is key to the success of the HSECoE.
- Overall, this work will be useful in making available the HSECoE models and results to the general hydrogen storage community, and one hopes the work will provide a legacy by enabling transfer of engineering concepts and materials properties during the development of improved materials-based hydrogen storage systems.
- Identifying materials limitations from system point of view would aid the community in proposing new concepts and solutions to overcome existing challenges up front.
- While there appear to be a number of visitors to the site, the overall impact is difficult to discern, as the original equipment manufacturers presumably have in-house means of assessing the performance that they require.

Question 5: Proposed future work

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

- Continuation of this effort beyond the conclusion of the core HSECoE activity will involve maintaining and enhancing framework models and beta testing and posting of selected models. The plans and milestones are straightforward, and they follow logically from the solid work conducted thus far on the project. An important aspect will be the publication of the work (especially underlying assumptions) in widely read journals (i.e., in addition to project descriptions provided in the final report).
- On slide 24, the team succinctly outlined the planned future activities and objectives. These are all worthwhile and reasonable within the funding allocated to this effort. The reviewer concurs with the sequence of effort and intent of the specific tasks. An important aspect of this task is to document fully the support manuals for these documents along with providing detailed example test cases for the outside user to establish correct procedures during analyses.
- The plan is for this effort to provide an executable from the original MATLAB code and to make this available with an Excel interface for user-provided inputs. This seems to be the best approach for completing this effort.
- Future work will include stand-alone fitting routines, adsorbent model updates, and versions of codes that do not require users to have MATLAB. A remaining challenge that was not addressed in the presentation is

what happens after funding for this project ends and who will maintain the website. Changes to servers and associated software can lead to links breaking and codes not functioning properly.
- The planned future work, which includes maintaining the webpage and supporting the users, is good.
- Future plans are on target, except that there are no plans to obtain input from “outside” users and no plans to make adjustments to make utilization of the modeling programs easier for “outside” users.

Project strengths:

- A highly capable team has been assembled to conduct the technical effort on this project. The team understands the need to develop models that are effective and powerful but are also are user-friendly (beyond MATLAB). It is apparent that careful thought has been devoted to proper documentation of the models and consideration of how the models might be enhanced and adapted for use with emerging new materials.
- A great strength of this project is that the core team members have extensive knowledge and expertise of all of the hydrogen storage media as well as the appropriate software and analytical packages to develop and execute the modeling codes for the website. This is an ideal collection of experienced individuals to continue and extend the HSECoE objectives. The remaining challenges and barriers as summarized on slide 26 demonstrate that the team recognizes several key areas that need to be addressed to produce better materials-based hydrogen storage systems.
- The project is developing very useful tools based on the results of the HSECoE for the materials-based hydrogen storage community within the allotted time frame.
- The project allows the public to make use of the vast amount of knowledge generated through the HSECoE.
- This is a strong, effectively collaborating team with a clear vision and understanding of its mission.

Project weaknesses:

- Probably the most significant limitation is that the current detailed models are written in MATLAB/Simulink or Comsol computer codes that are not universally available for many materials researchers. Hence, these models being developed and made available on the website may not be sufficiently utilized to evaluate new candidate materials or storage systems.
- Outside materials developers would find it useful to have access to the source codes so that they could be modified if needed. This access is currently not available.
- Model validation is always a concern. It is unclear how the core models and model enhancements will be validated in the absence of an ongoing HSECoE engineering activity.
- It is unclear how online information and the website will be maintained in the future.
- No “outside” user feedback is being obtained.

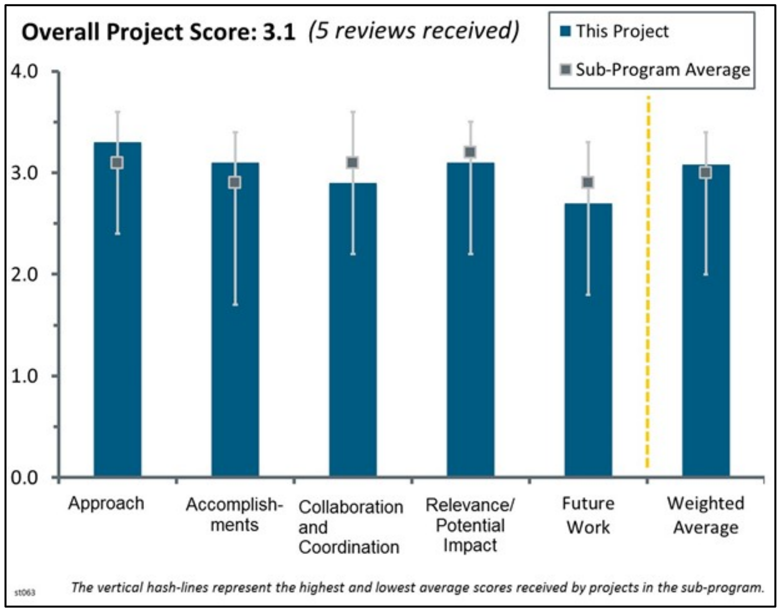
Recommendations for additions/deletions to project scope:

- It is not clear whether the current model for the sorbents works well for materials being designed to operate near ambient temperatures. The model may need to be modified to allow researchers to look at sorbent materials that have sufficient binding energies that they do not require liquid nitrogen. It could be valuable to host a workshop once or twice per year demonstrating how to use the model with input from a couple “outside” users. For example, someone could demonstrate how the model is supposed to work using a material that has been investigated in the recent past, showing how to input the critical material property parameters and what the output looks like for a given material—and maybe even include a sensitivity analysis that would show which parameters are more sensitive to minor modifications.
- It will be important to publish the results of this work in relevant and widely read journals. An essential element of the publication(s) must be a discussion of critical assumptions that have been made in development and implementation of the models. References to the publications should be included in the website.
- The issue of website and data availability beyond 2018 (project end) should be addressed. The project should consider making the codes and manuals available independently from the Internet, such as on a compact disc (CD) or memory stick.

- In general, the outlined scope for this project is appropriate and should be feasible to accomplish. Thorough documentation of the instructions for the downloadable computer models should be provided, and including specific software requirements and limitations is very desirable.
- The project should obtain feedback from “outside” users that are not former HSECoE members and then make adjustments based on their input.

Project #ST-063: Reversible Formation of Alane

Ragaiy Zidan; Savannah River National Laboratory



Brief Summary of Project:

The overall goal of this project is to develop a low-cost rechargeable hydrogen storage material with cyclic stability, favorable thermodynamics and kinetics, and high volumetric and gravimetric hydrogen density. Specific objectives include (1) development of cheaper techniques to synthesize alane, which avoids the chemical reaction route of AlH_3 that leads to the formation of alkali halide salts such as LiCl or NaCl; (2) utilization of efficient electrolytic methods to form AlH_3; and (3) development of crystallization methods to produce alane of the appropriate phase, crystal size, and stability.

Question 1: Approach to performing the work

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

- This project shows how alane (AlH_3) is an ideal hydride for the use (hydrogen liberation) end of the cycle. Except for ST-116 (Ardica Technologies [Ardica]), this is essentially the sole U.S. Department of Energy (DOE) project on this interesting and high-potential hydride. Alane is not reversible by direct rehydrogenation, so this project is correctly focused on electrochemical processes for efficient regeneration of spent alane (Al metal). The processes are complex and multi-stepped, but clearly possible. The many barriers are well addressed and are focused on DOE needs and targets for hydrogen storage for portable power fuel cells.
- This project is an interesting take on producing relevant amounts of alane by electrochemical means. The current process is extremely expensive and impractical for real applications. The team is addressing the inefficiencies by recycling materials, electrolyte regeneration, and reducing dendrite formation. It is a systematic line of work that is producing results.
- The project is aimed at relevant issues that are aligned to addressing barriers in order to meet cost targets. It is unclear from the presentation how well designed the approach is since most of the results are presented as final improvements and there is no sense that a range of parameters or materials has been investigated in an effort to optimize performance.
- The electrochemical method for formation of alane is more efficient than the conventional chemical route currently adopted by Dow Chemical. While the electrochemical method has been demonstrated in the laboratories, many practical issues remain for large-scale production. This project has encountered some of these issues (such as dendrite formation and crystallization of alpha-alane) and is still in the process of addressing them satisfactorily. Regeneration of $LiAlH_4$ is crucial in the overall scheme for using alane as a hydrogen storage material. However, there is no new information to address the low regeneration efficiency that was presented by the project in previous years. The assumption that $LiAlH_4$ can be regenerated in situ is highly questionable in practice. In all likelihood, $LiAH_4$ must be regenerated out of cell in a separate regeneration process.
- The approach is barely adequate and perhaps could have been improved with more integration and communication with the partner, Ardica. There was little experimental design considering that much of what the project is trying to accomplish is to make incremental improvements toward a viable process. This should be a systematic progression, but that was not apparent. There was little discussion of what the rates

of the various processes must be to make alane cost-effectively. The approach seemed to duplicate some of Ardica's approach, but in general, the project executed those areas of duplication at a much lower level, e.g., cost analysis. Perhaps a more thoughtful division of labor would provide DOE with a better set of projects. There were many typos in the slides, which seems to represent a lack of attention to program execution. The photographs of the chemical reaction glassware indicated a very sloppy-looking laboratory. This does not lend a feeling of confidence in the overall research and development efforts.

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.

- Further progress in efficient alane synthesis and crystallization has been made. These are more evolutionary than ground-breaking but are nevertheless valuable.
- There is good progress in the recovery of additives, identification and control of the phases of alane, electrolyte material selection, etc. The project barriers are addressed one at a time.
- Impressive progress toward goals has been made on many fronts during this reporting period: dendrite elimination with a new MgNi electrode, recovery and recycling of reactants, better adducts, improvement of efficiency (suggesting lower alane ultimate production cost), improvement, and stabilization of the resultant AlH_3 crystalline product, to mention the main results. Importantly, the effort has resulted in the synthesis of excellent AlH_3 (98% of theoretical H-capacity). This long-standing project switched from onboard light vehicle to portable power targets in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP), apparently because of high projected cost ($100/kg target). However, the Ardica companion project (ST-116) is currently projecting the possibility of significantly lower ultimate cost.
- An MgNiHx-based cathode was developed and implemented to reduce dendrite formation. It is unclear how pure LiH can be recovered from the electrode; this has yet to be proven. There are simple mechanical methods to solve the dendrite growth problem, such as using scrapers to scrape off dendrites and collect them in a basket underneath the cathode. This method has been successfully adopted in large-scale electrorefiners for reprocessing of spent nuclear fuel.
- This project's progress was not at the level expected. There was not much to report relative to last year. Perhaps it was just the presentation style that did not adequately reflect the true level of accomplishment. The most noteworthy accomplishment was the use of MgNi as a cathode material that seems to reduce dendrite formation. There was apparently little discussion of this development with the project partner, who is most concerned with developing an economically viable process, so it is not clear whether the Savannah River National Laboratory (SRNL) accomplishment can lead to improvements in the overall cost. Many of the highlights discussed by SRNL came from Ardica (and were properly attributed), but this seemed to indicate that not much new was happening on the SRNL end of the project.

Question 3: Collaboration and coordination with other institutions

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

- This project has good collaboration with partners Ardica and SRI International (SRI) but does not have any collaboration with other institutions outside of the partnerships with Ardica and SRI.
- There are excellent connections between the teams at SRNL, SRI, and Ardica. The project has an industrial component that is working in overcoming the barriers and making the process scalable.
- There are collaborations with cooperative research and development agreement (CRADA) partners Ardica and SRI, but the roles of these partners are not defined completely, especially that of SRI.
- Despite the frequent contact between the primary collaborators (Ardica) on this project, the evidence for the impact of this is not great. For example, the partners are independently using different adducts to replace tetrahydrofuran (THF) for crystallization without apparent reference to each other. The division of labor in some tasks is also not clear; both mention electrolyte recovery and developing efficient crystallization, for example, with no explanation of what role each is taking.

- The project's communication with Ardica appeared to be at less than nominal level. It appeared that results were not shared in a timely fashion (e.g., the MgNi cathode results were not discussed or mentioned by Ardica in the Ardica presentation). The SRNL "cost analysis" was too rudimentary to provide any useful technical guidance relative to Ardica's very comprehensive process/cost analysis.

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 very relevant to the portable power goals and objectives of the MYRDDP. Third parties are already interested in specialized applications of alane made by the synthesis process developed at SRNL.
- The project has potential to meet DOE goals in some portable power applications. Alane has sufficient hydrogen capacity, and the project is aimed at improving efficiency and reducing cost.
- The relevance is good, aligning with DOE objectives. The impact of this project is reduced, however, by the reduced level of accomplishment relative to prior years, and the SRNL work contains some duplication of effort with Ardica.
- The potential impact is limited because the material is unlikely to be low-cost. It may find niche applications in defense, long-term emergency energy storage, etc. However, it may get people to think of alternate and more "chemically sound alternatives" to store hydrogen in systems (unlike ball milling, for example).
- The cost to produce alane under the electrochemical method remains out of reach for light-duty automotive applications. Suitable applications could include portable low-power systems that use alane as a hydrogen storage material.

Question 5: Proposed future work

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

- The team has a clear and systematic line of work that so far has produced a real physical material.
- The future work proposed extends the project in the current direction and is somewhat general. It is not clear what is proposed for efficient recrystallization methods. Two of the bullet points relate to improving electrolyte conductivity (the first appears to have been completed this year). It would be good to establish a target for conductivity, perhaps using Ardica's cost and process models.
- The proposed future work is sound, aiming to improve cell conductivity by using THF and establishing a method to recover alane from the alane–THF adduct. There is no mention of work related to regeneration of $LiAlH_4$, which needs to be proven beyond small laboratory-scale and with high efficiency.
- Much of the future work in optimization could be better accomplished at Ardica, where the process details may come closer to realization. It was unclear whether the SRNL future work was in concert with what the project partner requires. The SRNL future work slides appeared unfocused and possibly with too many potential areas relative to the level of effort that can be applied.
- The remaining challenges and future work (slides 19 and 20) seem reasonable but rather open-ended. This project has no go/no-go decision point and is not very clear as to when there will be an end point. It can be argued that the project has made enough excellent basic progress that it can soon be ended and turned over to industry (Ardica) for practical cost optimization and commercial production.

Project strengths:

- The project is aimed at improving cost and efficiency of a relatively high-gravimetric-capacity material, and therefore is aligned with DOE goals.
- The principal investigator and his team have many years of experience working on this project, and their expertise is unique in the United States.
- A potential strength lies in the national laboratory–small company collaboration toward developing a viable commercial storage "product."

- This project is done in a systematic way and shows progress in an area that is, at the moment, stalled (hydrogen storage). Electrochemistry can be turned into an industrial process.
- The project is a very detailed attempt to make practical the low-cost production and use of a most interesting hydride, AlH_3. The approaches are innovative and practical.

Project weaknesses:

- The project is nearly complete and will soon get to the point of diminishing returns. The low-cost production/regeneration of alane is a significant challenge relative to the needs for light vehicle onboard hydrogen storage.
- The project does not show great structure and planning in overcoming barriers. The principal investigator's response to a similar comment from last year's review contains statements that are in general admirable but do not really address the comment. The project comes across in the presentation as a series of ad hoc experiments. Perhaps this is not the best way to make progress in this project. Communication with partners is good, but the outcome of this is not strong, and there appears to be duplication, and opportunities have been missed for this project to have goals more clearly set by Ardica's needs.
- Improving cell current and yield by using more a conductive agent such as THF brings along a different set of problems, as the alane–THF adduct is very stable. The transamination process with triethylamine to separate alane from the adduct is energy intensive and inevitably reduces the overall alane recovery efficiency.
- The material, alane, is unlikely to meet the demands for transportation, so its impact is reduced.
- The project as presented appeared unfocused. The project has a duplicative effort with partner Ardica and an apparent lack of significant progress relative to prior years.

Recommendations for additions/deletions to project scope:

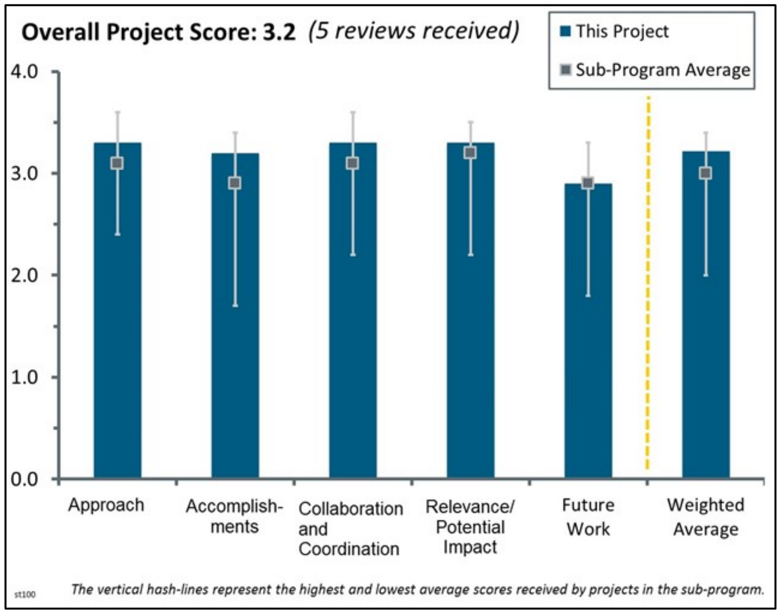
- This project is on track.
- The team should have a comprehensive meeting with partner Ardica to discuss where Ardica feels there are gaps, and then where SRNL can play a role in filling these gaps to move alane along the commercial pathway, if there is one. If there are gaps, then there should be a very detailed experimental design to optimize, make incremental improvements, etc., where the commercial partner believes there is the greatest need(s).
- This project would benefit from a reasonable go/no-go decision point and a plan to finish the remaining process details and turn the alane effort over to industry.
- The project needs a more coordinated approach with partners, with each concentrating on facets they can deliver to help one another.
- The project should include regeneration in future work and aim to achieve high regeneration efficiency.

Project #ST-100: Hydrogen Storage Cost Analysis

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:

The goals of this project are to (1) conduct independent Design for Manufacture and Assembly (DFMA) cost analysis for multiple onboard hydrogen storage systems, (2) assess/evaluate cost-reduction strategies, and (3) identify pathways to reduce the cost of onboard hydrogen storage systems by 15% compared to the U.S. Department of Energy's (DOE's) 2013 record and meet the DOE 2017 target of $12/kWh for onboard hydrogen storage for light-duty fuel cell electric vehicles (FCEVs).



Question 1: Approach to performing the work

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

- With the specific objective of assessing factors that affect 700 bar Type IV hydrogen storage, the Strategic Analysis, Inc., (SA) team has provided a comprehensive evaluation that was expanded and updated to address issues raised during the 2015 review. They examined variations in carbon fiber (CF) materials and processing as well as balance-of-plant (BOP) components.
- The approach, including the uncertainty analysis for tank manufacturing, captures the improvements across the Hydrogen Storage program, and the DFMA analysis for the BOP components is very thorough and seems to be a very accurate reflection of what the costs could be after including the selling, general, and administrative expenses. It would be good to see this applied to some of the more quickly developing areas of the Hydrogen Storage program, such as the more promising materials. In the program manager's overview presentation, he highlighted alane several times, but there has not been a thorough cost analysis of alane by SA in the past several years. This would be very beneficial to the community, especially when attempting to drive the cost of this material to below $10/kg of alane.
- The project has a good approach.
- The approach is generally good, but in looking at manufacturing considerations, particularly winding time, there are a number of variables not considered to date. For example, winding with larger tows potentially appears to reduce winding time, but winding equipment does not generally handle very large tows well. Additionally, when considering lower-cost CF—even those that are currently commercially available—the nature of the material itself must be considered. While the material itself may be substantially lower in cost, significant tow breakage and "fuzzing" is observed in winding, resulting in a need to stop the process frequently to clean the winding apparatus and thereby increasing winding time. It is not clear whether there is actually a cost benefit to using lower-cost fibers.
- The cost focus is narrower than ST-001 but goes into more depth on compressed gas storage composite tank cost drivers. It would also be beneficial to see analysis on the original equipment manufacturer selection process to examine cost versus other design considerations, such as weight and volume.

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 updates to the modeling, including the uncertainty analysis to capture the research and development (R&D) advancements into the model, were great improvements and accomplishments for this group. Also, the focus on the alternative manufacturing techniques is very useful to the R&D and manufacturing community, considering the batch-style process that occurs for tank manufacturing currently.
- The project offers in-depth analysis comparing to the current 2013 benchmark—a good means for vetting potential cost-reducing concepts.
- The reported cost analysis for several relevant processes and tank components would help in reducing the high-pressure tank cost.
- The project provides guidance as to what potential methods of cost savings should be targeted for further R&D. However, that said, there are many nuances to the manufacturing operations—especially filament winding—that have not been addressed. The "fuzzing" issues with low-cost fibers are among these. Also, while some cost savings were realized with the vinyl ester resin systems for 700 bar Type IV systems, it should be understood that these systems may not translate as well into future work (e.g., cryo-compressed).
- After completion of its assessments during the current year, the SA team reported that less than a 15% net cost reduction could be achieved between the 2013 and 2015 status. While these newer values may be more robust than previously reported, they still are >40% higher than the 2020 cost targets. There seems to be little more to be gained from continuing these analyses at this time. There is also a major gap in extrapolating manufacturing and processing costs for less than 10,000 storage units per year to the case example for 500,000 units per year.

Question 3: Collaboration and coordination with other institutions

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

- The interactions and data exchanges between SA and its partners and other organizations seem strong and fruitful. There seem to be no issues in this area.
- The project offers a comprehensive team with strong ties to related projects.
- It seemed that a more concerted effort to collect data from other DOE projects was undertaken during this performance period.
- Collaboration is visible with other institutes and industrial partners.
- The interplay between Argonne National Laboratory and SA has seemed to work well. It is not clear how much the National Renewable Energy Laboratory is involved and what its exact role is in this 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 **3.3** for its relevance/potential impact.

- The techno-economic analysis is a very important aspect that should guide DOE's direction in determining portfolio selection. This is also why it is important to have a broader outlook than just tank manufacturing, especially when the materials and systems are in such an early stage, but this is a key component to driving a great portfolio that definitely should continue.
- The merit of this project has been to provide an independent assessment of all the cost factors for large-scale manufacturing of hydrogen storage systems. Nearly all effort during the past year has been on the Type IV 700 bar compressed gas system, which is the only current contender for hydrogen FCEVs.
- The relevance toward reducing cost in 700 bar Type IV hydrogen storage systems is excellent. A cautionary note is, however, that analyses conducted here may not be entirely applicable to other up-and-coming storage concepts (e.g., cryo-compressed employing composite overwrapped pressure vessels). In these cases, other factors will need to be addressed.

- The project offers strong guidance to sort through various cost-reduction approaches.
- The cost analysis would help capture possible cost-reduction opportunities.

Question 5: Proposed future work

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

- The planned work is good; however, the project should include BOP and tank estimates for other ongoing projects. For example, the project should evaluate the effect of changing the Ni content in stainless steel on the cost (from ST-113) and the effect of material cost on the high-pressure metal hydride hybrid tank.
- The future work should take a broader view of the portfolio and definitely include a focus on the materials that are currently viewed as functional for the various applications (portable power, materials handling equipment, and vehicles), as these are all key sectors that can drive gross domestic product (GDP) and greenhouse gas reduction.
- The project should consider basalt fiber as midway between glass fiber and CF in performance and cost. The project should also consider economic drivers in cost versus performance, i.e., what drives the “buy” decision. It is not clear what the key material characteristics are that drive tank design; the project should consider thermal plastics or thermoplastic polyurethane (TPU) for tougher resins that may provide enhanced safety and damage tolerance.
- Since the updated comprehensive assessments of the Type IV tanks show little potential for any significant progress toward meeting DOE targets within the foreseeable future, SA appears to have taken its evaluations to a stopping point. There is little value in performing further analyses on this system, such as those listed on slide 24, beyond the end of fiscal year (FY) 2016.

Project strengths:

- The results produced by the SA team seem robust and well vetted as practical, considering the still very limited manufacturing production levels for FCEVs. Good collaborations with component developers and manufacturers should lead to more reliable results.
- The project has a very good approach, and the project has been progressing well.
- The project takes a comprehensive look at all aspects of 700 bar Type IV hydrogen storage vessels.
- The model has a strong basis but can add other features such as certification costs and tank finishing/rework, as well as scrap costs.

Project weaknesses:

- In order to reduce costs for BOP components of Type IV storage systems, the SA team has proposed highly integrated multi-function devices. However, the ability to manufacture such systems and validate their robustness and reliability remains to be demonstrated. Also, there seems to be little room to reduce cost for high-strength CF to the level necessary to reduce system mass and volume.
- The manufacturing assumptions remain a bit simplistic—more data from using low cost CF and towpreg should be collected to refine assumptions.
- There are challenges associated with predicting the effect of changing processes on the tank cost.
- The project should consider economic drivers in cost versus performance, i.e., what drives the “buy” decision. It is not clear what key material characteristics drive tank design; the project should consider thermal plastics or TPU for tougher resins that may provide enhanced safety and damage tolerance.

Recommendations for additions/deletions to project scope:

- It is recommended that the project include BOP and tank estimates for other ongoing projects. For example, the project should evaluate the effect of changing the Ni content in the stainless steel on the cost (from ST-113) and the effect of material cost on the high-pressure metal hydride hybrid tank (from ST-001).
- The project should consider economic drivers in cost versus performance, i.e., what drives the “buy” decision. It is not clear what key material characteristics drive tank design; the project should consider thermal plastics or TPU for tougher resins that may provide enhanced safety and damage tolerance.

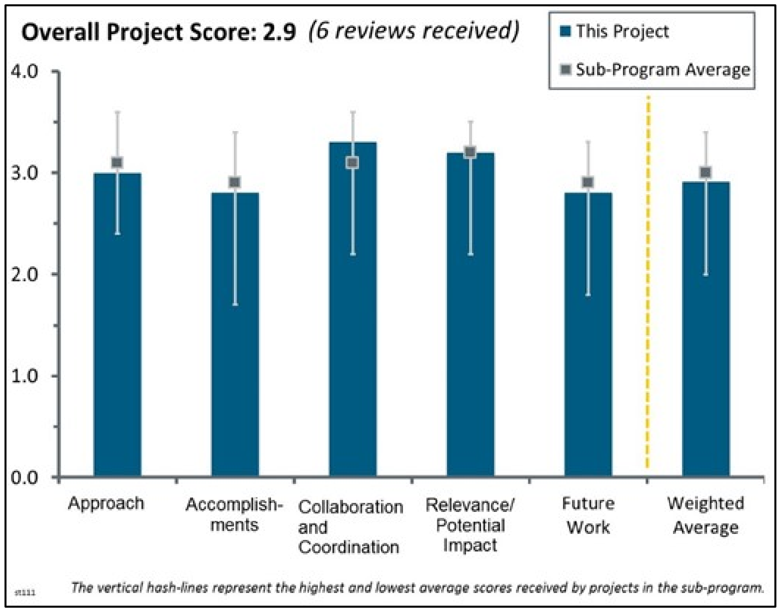
- In light of the current status of the competent analyses performed by SA and the lack of eminent improvement in carbon materials or BOP components, this project should finish at the end of FY 2016. The DOE resources could then be made available to investigate and develop higher-performing materials and improved components.

Project #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

Brief Summary of Project:

The objective of this project is to demonstrate a scalable 700 bar cryo-compressed hydrogen storage system that storages 5 kg of hydrogen and is capable of achieving gravimetric and volumetric capacities of 9+ wt.% and 50 g/L, respectively. This system offers the potential to exceed the U.S. Department of Energy's (DOE's) weight and volume targets at a modest cost. In addition, a liquid hydrogen pump that can rapidly and consistently refuel cryogenic onboard hydrogen storage to 700 bar will be assessed.



Question 1: Approach to performing the work

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

- This continuation of several prior Lawrence Livermore National Laboratory (LLNL) projects on the development and demonstration of cryogenic compressed hydrogen addresses experimentally most of the critical issues concerning this high-capacity storage system. For widespread usage of onboard vehicles, robustness, safety, and durability of the Type III tank must be demonstrated during operation, and extended pressure–temperature cycling for this method must be considered.
- The facility is excellent and provides a national capability for testing hydrogen storage tanks. The one flaw is that it has very little instrumentation to detect onset of tank damage such as acoustic emission sensing or in situ nondestructive evaluation (NDE).
- Both the approach to develop the testing facility and the approach toward the demonstration of cryogenic cycling are excellent. However, the broad scope of the project seems almost to interfere with making progress on the tank aspects of the work. The project should have been considered as two projects.
- The goal of this project is to develop and demonstrate a cost-effective 5 kg cryogenic hydrogen system at 700 bar with 9+ wt.% and 50 g/L. The stated approach is to test the durability of four 65 L prototype vessels before building a 5 kg 700 bar cryo-compressed hydrogen system demonstrating 50 g hydrogen per liter. Phase 1 objectives (instrumentation to measure cryo-pump power and boil-off, a safety plan for a cryogenic hydrogen cycling facility, and a 1600 bar cryogenic liquid nitrogen [LN2] strength test of an initial prototype design) have been accomplished. Phase 2 objectives (a containment system for 1300 bar 160 K hydrogen burst and 700 bar cycling to 300 K) have been partially accomplished. Phase 2 objectives yet to be accomplished are 1500 cycles and cryogenic hydrogen end-of-life strength testing of two vessels. Phase 3 objectives that will be done subsequently include aggressive cycling and then strength testing of two higher-performance vessels, installing the final vessel design in a lightweight compact vacuum jacket, and performance demonstration (volume, peak hydrogen density, dormancy, and vacuum stability). However, slide 19 indicates that many of these Phase 3 objectives may need to be renegotiated. This greatly lowers the confidence that this project will be successfully completed. The approach is focused on developing and demonstrating a viable tank that can withstand 700 bar. Much care has been paid to safety aspects of the containment system and the liquid hydrogen (LH2) pumping apparatus. However, not enough attention has been paid to topics such as hydrogen boil-off and overall economics of the

technology. Slide 22 very briefly touches upon driving range inconsistency due to cryogenic refueling, but the corresponding inevitable boil-off when periods of frequent use are intermingled with periods of infrequent use has not been addressed. The thermal insulation aspects have also not been adequately addressed.

- The approach of this project is too aggressive, as it attempts to develop cryo-compression for both high-pressure and cryogenic temperatures. The advantage of decreasing the temperature is to reduce the pressure. The project should have considered the optimization of the pressure vessel at a lower pressure rather than 700 bar. In addition, the project should have avoided welding for liner construction and considered current seamless tank liner technology. The project should also have included an evaluation of the thermal insulation.
- The approach for this project was too ambitious and reflected poor planning from both the principal investigator (PI) and DOE. It is not certain what value will be gained from the sum total of the project because of the approach. A better approach would have been to focus first on a facility that could perform the testing, then develop individual components (e.g., identify a liner material, cycle it, do vacuum insulation, cycle it), then develop a system from these and demonstrate that through performance cycle testing.

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.

- The completion of construction and successful commission of the LH2 pump that permits filling cryogenic vessels with cryogenic hydrogen at 700+ bar is a significant accomplishment. The difficulties of manufacturing the Type III pressure vessel that satisfies safety criteria have certainly had an impact on the proposed schedules for subsequent full-scale fueling and cycling tests. However, the team needs to address the production and qualification steps to fabricate vessels that can pass the inspections. On the other hand, confirming that both the LH2 pump and cryogenic pressure vessel satisfy operation and life cycle criteria is necessary for this project to reach its stated objectives.
- The project has made excellent progress building and testing the facility. There is more to be done for in situ monitoring.
- While Phase 1 and some Phase 2 objectives have been accomplished, significant and challenging Phase 3 tasks are yet to be performed. The one major accomplishment has been to build the containment system for LH2 cycling tests. On the tank side, the project demonstrated an LN2 tank to 1560 bar burst pressure. However, all subsequent tests with water cycling up to 700 bar fell short of the project's stated metrics. The project was unable to achieve the desired results with ambient temperature water cycling, which was attributed to manufacturing deficiencies (lack of roundness and poor weld quality). The team's solution to inadequate ambient water cycling performance is to proceed immediately to LH2 cycling with the belief that cryogenic temperatures will improve elastic range, ultimate stress, and fiber modulus and, hence, cycle life. This feels a bit like throwing a "Hail Mary" pass. A more prudent approach would be to address and eliminate the manufacturing defects (lack of roundness, poor welds), retest with water, and only then proceed to LH2. Thermal stability and the inevitable boil-off have not been addressed. Overall, the project seems to be in some jeopardy. The accomplishments to date do not create a feeling of confidence that the remaining tasks will be successfully completed.
- Good progress has been made on the hydrogen test facility, considering all of the hurdles that needed to be overcome. In retrospect, siting the facility in a location without the seismic code requirements might have allowed this aspect to proceed faster. There seem to be many issues with the thin liner—many failures during autofrettage and at relatively low cycle numbers. It is not clear what specifically is being done to address the issues with the liner. At this point, the high-pressure target seems completely unrealistic. There may be value in determining exactly how high you *can* go; this information could provide guidance toward system improvement. Also, additional NDE sensors during testing would be useful. It is not certain that they really *need* to be hydrogen-rated. The project could consider stopping a test as soon as a leak is detected and flushing the system with nitrogen to prevent hydrogen contact with air.
- The accomplishments thus far have been mainly related to station certification, which is a tremendous accomplishment but only a small part of achieving the project goal. The failures in developing the tanks

and liner materials so far are learning opportunities, and it seems that the investigators are progressing, just not at the expected schedule required to complete this project. This reflects poor planning.

- The project accomplishments were minimal since the tank designs were unable to achieve the desired cycle life, even at room temperature and during autofrettage. The project has accomplished the construction of the test facility, although this facility was not required for identifying the failures with the tanks since the issues occurred at room temperature.

Question 3: Collaboration and coordination with other institutions

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

- Partners include Linde, BMW, and Spencer Composites. The responsibilities of the team members are well defined, and the team seems to be working well together.
- There appears to be very strong long-term and close collaborative interactions among the LLNL, Linde, and BMW partners providing complementary expertise to this project. The pressure vessel fabricator (Spencer Composites) seems to have appropriate experience, although meeting the properties for cryogenic and 700+ bar pressures may be a greater challenge than initially assumed. It does not appear that this team has contacted other organizations (e.g., NASA laboratories or contractors) that also have extensive high-pressure and cryogenic hydrogen expertise.
- The project's collaboration with industry leaders with cryogenic hydrogen is excellent, although the tank supplier may not be the appropriate partner for the tank design. The tank development for this project should not have experienced premature issues in autofrettage and room-temperature cycles.
- Collaboration between other institutions and LLNL seems to be adequate. It is not clear whether there is any direct communication between BMW and Spencer or if it is all done via LLNL as the middle man. There could be significant benefit in direct contact.
- The collaboration between LLNL, BMW, Spencer Composites, and Linde seems to be adequate to complete the project. Additional consulting on the liner failure issues might be required.
- Collaboration needs to include expertise for structural health monitoring and NDE. The project will have much more value if progressive damage can be monitored to help understand failure mechanisms.

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 a unique project within the DOE Fuel Cell Technologies Office (FCTO) portfolio on the development of appropriately sized and efficient cryogenic hydrogen storage. This project will provide the only empirical data necessary to assess whether this concept actually provides for increased operating range over a broad spectrum of vehicle classes.
- The relevance of this project is highly aligned with the DOE research, development, and demonstration goals, as the project attempts to achieve a system with 9+ wt.% and a volumetric density of 50 g/L. Previously, cryo-compressed hydrogen was projected to have the potential of meeting the 2020 DOE system targets. It is important to have this project in the portfolio to evaluate and develop this technology.
- The project has great potential for Hydrogen Storage program impact if sensors to monitor tank failure onset are included.
- Hydrogen storage is a very challenging problem from a technical and economic point of view. To commercialize fuel cell vehicles, developing a viable solution to this problem is essential. Hence, this project is well aligned with the Hydrogen and Fuel Cells Program. However, the main concern is this: even if this type of pressure vessel were successfully demonstrated within the scope of this project, major questions pertaining to safety (inevitable boil-off) and economics (whether the LH2 approach is really viable from a well-to-wheels perspective) remain unanswered.
- The relevance of the project is good in that the cycling of thin-lined vessels for cryo-compressed storage needs to be understood. It is assumed that liner embrittlement upon long-term exposure to hydrogen has

been considered in designs, but this is not clear at this point in the project. Since no real cryogenic cycling with hydrogen has been conducted to date, it is difficult to assess the value of the data at this time.
- Cryo-compressed vessels could have a significant impact on a vehicle's storage capacity and volumetric efficiency, but there could be significant trade-offs because of hydrogen boil-off related to driving patterns that might force these vessels into niche applications that are not applicable to light-duty vehicles.

Question 5: Proposed future work

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

- Future proposed work is excellent but relies heavily on success with thin-lined vessels in cryogenic hydrogen cycling to high pressures. At this point, there is no convincing evidence that this goal will be met, placing the longer-term objectives in jeopardy.
- The team needs to focus primarily on the fabrication and qualification procedures for producing cryogenic Type III storage vessels with the "conservative design" 1.5 mm metal inner wall. Safety and durability are much more important to establish than slightly larger storage capacities at this stage. Optimization can occur during later development. However, increasing dormancy and providing robust thermal isolation are important issues that need to be addressed.
- Phase 2 objectives yet to be accomplished are 1500 refueling and cryogenic hydrogen end-of-life strength testing of two vessels. Phase 3 objectives listed include aggressive cycling and then strength testing two higher-performance vessels; installing the final vessel design in a lightweight compact vacuum jacket; and performance demonstration (volume, peak hydrogen density, dormancy, and vacuum stability). Slide 19 indicates that many of these Phase 3 objectives may need to be renegotiated. This greatly lowers the confidence that this project will be successfully completed. Overall, the project seems to be in some jeopardy. The accomplishments to date do not create a feeling of confidence that the remaining tasks will be successfully completed.
- The proposed future work does not include specific steps to resolve the root causes of the premature failures or a change in direction to ensure the project can demonstrate cryogenic hydrogen cycling. The effort needs to be redirected toward lower pressures and/or seamless liner constructions.
- The proposed future status of this project is sketchy at best, considering that the team has not achieved a successful tank design. The investigators admit that it will require renegotiation of current milestones and go/no-go to achieve success, which reflects poor planning on the part of both the PI and DOE.
- Tanks should not be tested until proper NDE and structural health monitoring (SHM) are in place.

Project strengths:

- A unique cryogenic hydrogen filling station has been assembled at LLNL, and a comprehensive safety evaluation has been completed. This is certainly the best location in the United States to evaluate directly the behavior of charging and discharging cryo-compressed hydrogen storage vessels.
- The development of the containment system is a project strength. The hope is that the containment system developed at considerable expense will be available for use by other parties. Whether this project will reach a successful conclusion is questionable.
- The project's strength is the development and optimization of cryo-compressed technology, which could have near-term potential to be competitive with 700 bar compressed storage systems.
- The project addresses key issues with hydrogen refueling for the case of cryo-compressed storage.
- The project has excellent test facility capability.

Project weaknesses:

- There have been many liner failures during autofrettage and leaks occurring at very low cycling levels. It is not evident that there is a clear and concise plan to address these issues, which appear to be at the welds. It is not clear what exactly is being done to address these failures, whether different joining methods or relocation of the welds from the dome to the cylindrical section, etc. At the present time, the capabilities of these cylinders are unknown. They are supposed to be for 700 bar storage, but it is not clear what the maximum operating pressure *really* is at this point. It could be 350 bar, 500 bar, etc.

- Although considerable funding is allocated to this work compared to most FCTO projects, the costs for fabrication, enhanced inspection and qualifications, testing, and cycling are quite high. From leak and burst behavior of the first six pressure vessels, more effort probably will be necessary that affects not only schedules but also budgets.
- The weakness of the project is the aggressive approach for high pressure and cryogenic temperatures while attempting to optimize the tank construction. The project would have benefitted from a systematic incremental approach rather than reaching and failing to achieve the maximum bookend.
- The design and development of Type III cryo tanks at 700 bar have faced some technical hurdles to date. It is not clear whether these hurdles can be successfully overcome.
- Tanks should not be tested until proper NDE and SHM are in place.

Recommendations for additions/deletions to project scope:

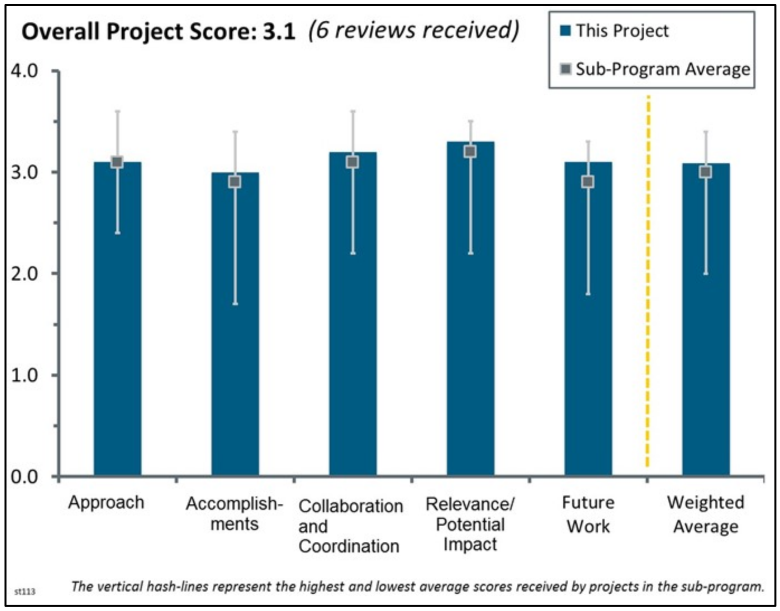
- The PI has indicated a second go/no-go: a successful cryogenic 1300 bar (SF = 1.85) strength test of at least one prototype vessel after 1,000 thermomechanical cycles. This is a reasonable approach for the continuation of the project. However, it would also be good to see the project demonstrate a successful water cycling test after addressing the manufacturing deficiencies (roundness, welding quality). During the question-and-answer session, it was suggested that NDE methods be employed to detect cracks and other defects in tanks. This will save time and costs compared to cutting open the tank and examining it visually. It was also suggested that welds should be eliminated completely rather than trying to get good welds.
- First, more prototype Type III pressure vessels should be fabricated and validated via several testing methods with both hydraulic and hydrogen gas pressurizations prior to initiating the extended cycling work. Also, more qualification measurements are needed on assembled components using NDE and cryogenic proof testing, along with the future testing proposed on slide 23. It is strongly recommended that LLNL contact appropriate NASA hydrogen expert personnel for some advice and possible support.
- The recommendations for this project scope are to focus on lower pressures, seamless liners, an alternative tank supplier, and an incremental understanding of the cryo-compressed tank design. In addition, the project's cycle criterion is too low at only 1500 cycles. The lower cycles are fine for initial screening, but there should be a fatigue projection for the industry standard in the Global Technical Regulation for 5,500 cycles. A cost projection of the technology would also be useful for comparison to the 700 bar technology.
- Inner cylinder performance goals should be met before proceeding with any work on the insulation/vacuum jacket.
- Tanks should not be tested until proper NDE and SHM are in place.

Project #ST-113: Innovative Development, Selection, and Testing to Reduce Cost and Weight of Materials for Balance-of-Plant Components

Jon Zimmerman; Sandia National Laboratories

Brief Summary of Project:

The overall objective of this project is to identify an alternative to high-cost metals for high-pressure balance-of-plant (BOP) components. The project goals are to (1) reduce weight by 50%, (2) reduce cost by 35%, and (3) expand the scope of construction materials for BOP.



Question 1: Approach to performing the work

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

- This project aims at U.S. Department of Energy (DOE) cost and weight barriers by looking to reduce BOP components of austenitic stainless steels through composition changes (i.e., metallurgical modifications, particularly lower Ni contents than Type 316L conservatively used austenitic stainless steel). In particular, the tensile and fatigue strengths of alloys are being measured in hydrogen. This approach has not been systematically explored of late and is needed to address DOE cost and safety barriers. The project is well designed. The attempts to calculate and measure stacking-fault-energy with fatigue and strength properties in hydrogen should result in important new fundamental metallurgical understandings of hydrogen embrittlement. However, there should not be overreliance on stacking fault energy (SFE) alone. Limiting the main composition variable mostly to Ni is probably too restrictive, i.e., Cr is another important composition variable. The project targets (-50% weight and -35% cost go/no-go) are very quantitative, ambitious, and directed at important DOE barriers.
- Combining density functional theory (DFT) with experimental efforts to understand the material properties of steels and other BOP types of materials and predict and measure advanced materials is an adequate approach. However, it seems this project has gotten off to a slow start with some of the fiscal year 2015 milestones still not 100% completed. The theory associating SFE with material stability is a reasonable approach and should be able to be adequately described in DFT space.
- The researchers are using a combination of theory and experiment, which can be a good approach. The targets, except the go/no-go, seem reasonable if the cost and mass reductions are for the BOP components only while maintaining material performance. The project is looking at some low-temperature testing (-50°C); however, cryogenic testing needs to be included.
- Reduction of the BOP costs through the use of alternative steel alloys is expected to substantially lower the tank's system cost.
- The approach of the project attempts to combine both experimental and computational methods to evaluate the current 316L material and recommend alternative materials with lower Ni content. The fatigue performance assessment appears to be useful and a better method than the historical tensile data. It is unclear whether the computational efforts with the SFE will provide a useful outcome in recommending other materials for hydrogen applications.
- This project combines experiments and computation. In principle, this combination should accelerate the development of optimal materials. One concern is the feasibility of the DFT calculations, which have proven to be more complex than expected.

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.

- Many new and highly useful data have been generated in the last year. The project is moving along at a very useful rate toward accomplishing its objectives and DOE goals. It seems likely the project will pass the go/no-go decision point.
- The progress is good where failure testing and modeling of SFE have been conducted.
- The project seems to be making good progress with experimental measurements. The computational work has not made a significant contribution because of complications associated with calculating the SFE of complex magnetic alloys.
- The project has progressed, although further results were expected based on the extensive experience of Sandia National Laboratories (SNL) in this area of hydrogen embrittlement. The characterization of new materials is useful, but the project should also ensure the fatigue approach is developed into an acceptable screening method. SNL is also involved in a broad effort to progress the advancement of hydrogen embrittlement standards. This work should be indicated when discussing the progress of this project.
- The investigators have identified and started to demonstrate a reasonable testing method for assessing the fatigue life of materials in hydrogen. It would be imperative to extend this to both temperature and pressure ranges. It is recommended that the go/no-go be reassessed to enable discovery of a broader range of new materials to meet the performance and cost improvements.
- For the budget, more accomplishments would be expected. It is unclear how the experiments are validating the theory. The researchers say they are using scanning electron microscopy for this, but the connections need to be clarified. It is unclear how the theory is helping the progress, and it seems they are testing the same materials as last year.

Question 3: Collaboration and coordination with other institutions

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

- The collaborations are excellent, not only in the fatigue testing area (High-Performance Materials Testing) but also with a BOP manufacturer (Swagelok) and stainless steel manufacturer (Carpenter).
- Strong collaborations appear to exist with Swagelok, Carpenter, and Hy-Performance.
- The current collaborations with Hy-Performance, Swagelok, and Carpenter seem adequate if the project is going to move toward more of a development-of-materials stage. The investigators should reach out to other expertise within the national laboratory community, as there is a wealth of expertise on hydrogen exposure on steels and other materials in the NNSA laboratories, and the investigators are part of that community.
- The collaboration of the project is clear between the SNL experimentalist and theorist, although the contribution of the other partners is not well defined. It would be helpful to have further input from Swagelok and other manufacturers regarding the cost and machining of these materials. In addition, SNL should acknowledge other involvement with standards organizations and international organizations in the assessment of hydrogen embrittlement.
- The project shows that there is collaboration with other institutes and an industrial partner who is a BOP developer.
- The project has engaged materials companies and component suppliers, and this is a good team, but the interactions with the collaborators 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 project clearly supports and advances progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan. The project adds much-needed BOP weight and cost support.
- The project provides an important area of study regarding evaluating the fundamentals of hydrogen embrittlement. The focus of the project is aligned with the DOE research, development, and demonstration goal to reduce the cost and weight for compressed hydrogen storage systems.
- The project's high-pressure tank's system cost can be reduced using alternative BOP components.
- This project is relevant because we need lower-cost, lighter-weight BOP components.
- The project shows how reducing the cost of BOP components is an important objective.
- The project could have a reasonable impact on hydrogen storage systems and tank components, but the reviewer is not totally convinced that the project will identify new materials.

Question 5: Proposed future work

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

- The future work provides good detail on the next steps to address the challenges of characterizing materials for hydrogen embrittlement. The future work should include a path to implement the outcome of the project in screening methods for other materials.
- The proposed future work for the remainder of the project is reasonable. It is important that the practical acceptances of Swagelok and Carpenter for the new alloy(s) be in place by the end of the project.
- The plan is good overall, but it would be useful to correlate the alloy composition to cost and performance so that trends are obtained.
- The go/no-go in this project seems very weak. From the simple analysis, it is clear there are several candidates to meet the cost and mass reduction. This should be changed to meet cost, mass, and performance requirements. As written in the presentation, the go/no-go does not have any performance requirements. Performance requirements need to be added to make the go/no-go meaningful; they need to validate the theoretical work. At this point, it is unclear what value the computational work is providing in the search for new materials.

Project strengths:

- This project is extremely well funded for the work scope.
- The project has appropriate collaboration and approach.
- This project has a sound metallurgical approach for lowering BOP weight and cost.
- The strength of the project is the fundamental understanding of hydrogen embrittlement and the pursuit of materials with low cost based on lower Ni content.

Project weaknesses:

- The project seems too focused on select types of steel alloys.
- The project needs some cryogenic testing, and how the experiments are used to validate the theoretical is not clear.
- The project does not have enough time to experimentally confirm all the effects and new alloys that are calculated to have potential.
- Given the configurational and magnetic complexity of these alloys, it is unclear whether the DFT calculations will make a meaningful contribution during the timeframe of this project.
- The project overestimates the BOP improvement for both cost and weight by replacing the 316L stainless steel material. Many components in the BOP utilize aluminum rather than 316L stainless steel, so the

improvement will be limited, and the new materials should be compared against aluminum. Also, the project needs to ensure that the new information is highlighted to distinguish it from previous SNL efforts in the area of hydrogen embrittlement.

Recommendations for additions/deletions to project scope:

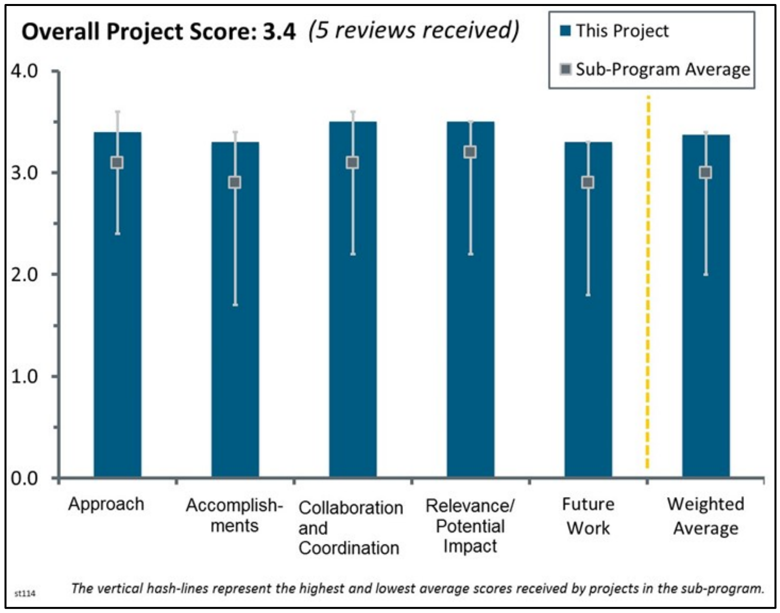
- In addition to exploring the magnetic contributions to the SFE, it is not obvious that configurational degrees of freedom have been considered. For example, if these alloys are solid solutions, then short-range order can lower the energy of the system compared to a truly random atomic arrangement. The project should also consider comparing reliability of the indirect estimates provided by the expression for SFE (slide 13) and direct calculations.
- The project should extend the studies to other cheaper steel alloys to make property/performance correlations. In addition, for best-performing alloys, it would be useful to take into account interfacial effects, i.e., oxides, etc., as these can affect the alloy performance.
- The team should consider Cr as a variable, in addition to Ni. Such an approach should be extended to nonferrous alloys (especially Al) in a follow-on project.
- The BOP improvement opportunities should be reconsidered based on actual BOP component designs. The project scope should also include a direct connection and acknowledgment of the opportunity to influence codes and standards related to hydrogen embrittlement based on the outcome of the project.
- The milestones of the project need to be revisited, especially the go/no-go.

Project #ST-114: Next-Generation Hydrogen Storage Vessels Enabled by Carbon Fiber Infusion with a Low-Viscosity, High-Toughness Resin System

Brian Edgecombe; Materia

Brief Summary of Project:

The objective of this project is to develop and demonstrate a 700 bar Type IV composite overwrapped pressure vessel (COPV) with (1) a reduction in carbon fiber (CF) composite volume of 35%; (2) a cost of composite materials of $6.5/kWh, which is an important element of the U.S. Department of Energy (DOE) 2017 system cost target of $12/kWh; and (3) performance maintained (burst strength of 1,575 bar and 90,000 cycle life).



Question 1: Approach to performing the work

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

- The approach of proving concepts using plate samples and then proving them out on small-scale prototypes is excellent. It has allowed a better understanding of void content effects on shear strength, which roughly correlates to burst strength. The project should continue to follow the same approach to resolve any issues with vacuum infiltration and then apply those learnings to larger prototype vessels.
- The idea of using a high-toughness, infusible resin offers many benefits to both tank fabrication and overall performance. The use of vacuum-based processing will greatly reduce the void content of the resulting composite.
- The project provides strong potential to reduce tank cost through better matrix, better quality, and faster winding.
- The overall goal of this project is to demonstrate a 700 bar Type III COPV with 35% reduction in CF composite volume to reduce the cost of composite materials to $6.5/kWh, while maintaining performance of burst strength of 1575 bar and 90,000 cycle life. The technical premise of this project is that the use of very low-viscosity resins (< 20 cP) during vacuum infusion of dry-wound forms will greatly reduce the incidence of voids (<1%), which can cause significant knockdowns of shear strength, which is important in the shoulder region of the tank. Additionally, the high fracture toughness of the Proxima resin is expected to contribute to better COPV performance. The overall proposed methodology for meeting these objectives, such as fabricating test plates with deliberately introduced voids via controlled air leaks and preparing and bursting small Type III COPV tanks, appears to be sound. The use of inspection methods such as void characterization, finite element modeling of the vacuum infusion process, and use of laser-based non-contact measurements of diameter changes associated with fiber crimping or buckling is reassuring.
- The approach of the project is to reduce voids with vacuum infusion, increase fatigue performance, and improve crack resistance. It is helpful to explore alternative manufacturing methods to the traditional wet winding, although the project could be improved in respect to confirming the relationship between voids and CF reduction opportunities. In addition, the cycle criterion for the project is too high. The certification standards are 11,250 cycles with a maximum number at 45,000 cycles rather than the 90,000 cycle target in the project.

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 following tasks have been completed: (1) demonstrate infusion process feasibility (thin and thick plates), (2) design a tank using models and materials data (static and fatigue), (3) conduct dynamic testing on composite plates, (4) prepare small tanks (Type III) as "proof of principle" for the process, and (5) design a COPV winding pattern for full-size Type IV tanks. The researchers have successfully demonstrated small Type III tanks to a burst pressure of >26,000 psi using their dry-wound vacuum infusion process. They have used laser scanning to characterize compaction of fibers during vacuum infusion and identified fixes to processes that cause localized compaction and buckling. They have conducted tension–tension cycling on glass fiber composite plates at 0.7% strain (corresponding to hoop strain at the maximum operating conditions), while deliberately introducing voids of a desired percentage via controlled air leaks. These laminates showed excellent retention of tensile strength despite moderate void content. Finally, they have completed an Abaqus flow model of the vacuum infusion process to aid experiments. All indications are that they are making strong progress toward meeting their goals.
- Significant accomplishments have been made and demonstrated with subscale tank production and testing. The reviewer has some concern about additional variables that will be introduced during the last few months of the project: (1) the switch from Type III to Type IV cylinders (should be mitigated by Spencer Composites participation), (2) the switch to Toray fibers from Mitsubishi (many factors can contribute to performance: handling during winding, fiber sizing effects on adhesion, and subsequent load transfer/fiber property translation), and (3) infusion at larger scale.
- The accomplishments during the past year to resolve void properties and winding/buckling issues by combining results from the modeling efforts have been impressive. The project seems to be progressing toward demonstrating the total process on larger-scale prototypes that will meet the DOE goals.
- Tanks were successfully fabricated and tested; quality was good (low voids).
- The accomplishment of manufacturing a tank with COPV infusion is good, although the results have not shown correlation to reducing the CF content. In fact, the cost analysis has shown an increase in cost from the baseline wet winding tank design.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration between Materia, Spencer Composites, Hypercomp Engineering for filament winding optimization, Montana State University (MSU) for finite element analysis (FEA) modeling and plate sample analysis, and Powertech Labs for full tank testing seems to be organized and coordinated effectively to achieve the project goals of cost savings.
- Materia seems to be taking advantage of expertise at partner institutions to quickly optimize processes involved in achieving the project goals.
- The collaboration seems to have clear responsibilities and roles. It was excellent to have the cost analysis performed by Strategic Analysis, Inc., in this year's effort. The project would benefit from having a series-production tank manufacturer either as a partner or in a consulting role to better guide the development toward commercialization.
- Partners include MSU for FEA modeling and mechanical testing; Spencer Composites for filament winding, fiber winding modeling, and burst testing; and Hypercomp Engineering and Powertech Labs for testing and modeling. The roles of MSU and Spencer Composites are clear; the roles of the other two partners are less clear.
- The project has a strong technical team. It is not clear whether Spencer Composites will commercialize the technology. It would be beneficial to see an original equipment manufacturer or compressed gas storage (CGS) converter on the team.

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 clearly supports the goal of DOE's Hydrogen and Fuel Cell Program (the Program) toward making cost-effective and safe hydrogen storage systems.
- This project can really have an impact on the manner in which tanks are manufactured. The process could be more amenable to an assembly line production over traditional wet winding of tanks, and it could possibly reduce some of the uncertainty with voids created during the wet winding process that might reduce the cycling durability of the tanks. Ultimately, if the project can reach the goal of reducing the cost of tanks by 35% from the 2012 baseline cost, it will allow greater adoption of fuel cell technology.
- The project is well aligned with the Program goals of cost reduction and/or performance enhancement.
- The relevance is high for this project because it is attempting to reduce the cost of the main factor associated with the compressed hydrogen tank system, which is the CF. The objective to reduce the CF by 35% is a stretch for this technology, although it would be significant if achieved.
- The project has strong potential impact but needs to focus on winding speed considerably. The project may need to interact with a fiber supplier and sizing company to maximize winding speed. The project could have a possible follow-on through the Institute for Advanced Composites Manufacturing Innovation.

Question 5: Proposed future work

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

- The proposed future work was well in line with meeting the requirements to test small-scale prototypes for dry winding vacuum-infused vessels.
- Proposed future work is appropriate, but the project should add enhanced fiber sizing to maximize winding speed and perhaps enhance infusion time.
- The future work is fine, although additional detail could be provided to demonstrate the CF reduction. This project seems to be focused on developing the infusion manufacturing process rather than focusing on the composite volume reduction from the baseline tank.
- The major remaining technical challenge is the vacuum infusion of large (full-scale) prototype tanks. The principal investigator (PI) indicates that wet winding will be considered if vacuum infusion cannot be achieved. Unfortunately, reverting to wet winding will negate the primary technical premise of this proposal, which is that the use of vacuum infusion will reduce the formation of voids. There is considerable expertise in the composites community for vacuum assisted resin transfer molding (VARTM) processes for complicated geometries. It is a bit surprising that the PI is so willing to give up this major advantage of the team's proposed method without fully investigating avenues to pursue vacuum infusion to the fullest extent possible.

Project strengths:

- The project explores an innovative pathway to reducing cost and/or improving performance of COPV for high-pressure hydrogen storage. The project team is excellently structured to achieve maximum progress toward Program goals.
- A well-conceived project has been executed so far for reducing the cost of Type IV COPVs by using a vacuum infusion process to reduce the formation of voids and thereby reduce the amount of CF that has to be employed. The project has employed a good mix of modeling and experiments with panels and small-scale tanks to demonstrate feasibility. The team has also employed void characterization and laser scanning measurements to identify and mitigate fiber buckling and undesirable fiber compaction during the winding process.
- The novel approach to reduced CGS tank cost has potential to succeed and opens the door for novel resins and fibers.
- The project strength is the focus on the key cost driver for compressed hydrogen tanks.

Project weaknesses:

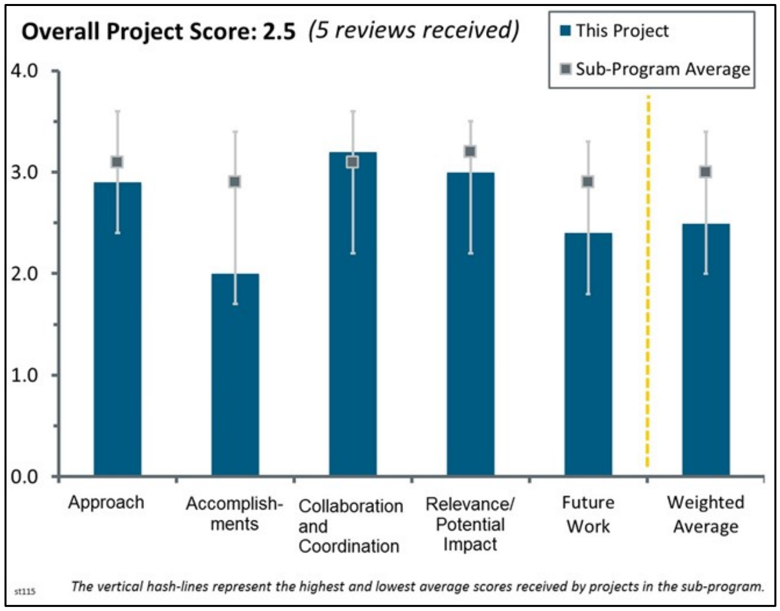
- If wet winding of the Proxima material is to be considered, it probably would have been advantageous to look at it earlier in the project. Resin viscosity and pot life requirements vary substantially between the two processes, and formulation adjustments and/or process changes to conventional wet winding may be necessary (and time-consuming). Difficulties may be encountered in changing from Mitsubishi to Toray fibers. No two fibers handle alike, and achieving the necessary degree of fiber property translation could be difficult. There is not much room to accommodate this within the budget/timeframe remaining.
- The team's readiness to revert to wet winding in the event that vacuum infusion proves intractable for the full-scale prototype tank is disconcerting. It is not clear why the team does not leverage the vast experience in the composites community with regard to VARTM processing and thereby fully accomplish its objective.
- The project needs to optimize fiber tow sizing to increase winding speed and needs to reduce waste from the resin infusion process by using state-of-the-art reusable materials.
- The project needs to emphasize the CF reduction potential of the technology and correlate the voids to improved performance. The models developed in the project should be further utilized to evaluate the potential of the technology.

Recommendations for additions/deletions to project scope:

- The team should leverage the vast experience in the composites community with regard to VARTM processing and thereby fully accomplish its objective of vacuum infusing the full-scale prototype tank.
- The project needs to optimize fiber tow sizing to increase winding speed and also needs to reduce waste from the resin infusion process by using state-of-the-art reusable materials.
- The project scope should emphasize the demonstration of CF fiber reduction at appropriate cycle life rather than just proving the feasibility of the vacuum infusion process. The cycle life target should be reduced from 90,000 to the industry standard. Adding feedback with a series production tank supplier would help in formulating a commercial path and ensure other important parameters are not overlooked in the manufacturing development.

Project #ST-115: Achieving Hydrogen Storage Goals through High-Strength Fiberglass

Hong Li; PPG Industries, Inc.



Brief Summary of Project:

The objective of this project is to develop a Type IV composite overwrapped pressure vessel (COPV) reinforced exclusively with glass fiber having composite strength that matches T700 carbon fiber composite at less than half its cost. The project will also demonstrate a novel glass fiber manufacturing process and study the stress rupture behavior of composites made from the new fiber.

Question 1: Approach to performing the work

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

- The approach, by creating a high-strength glass fiber that could potentially meet or exceed the strength of Toray T700 carbon fiber, would substantially reduce the cost of hydrogen storage tanks for fuel cell electric vehicle applications. An analysis of the associated weight penalty and its effect on driving distance per fill-up would be interesting.
- The approach of preparing glass fiber with higher strength than T700 would help reduce the compressed tank cost.
- The approach, using low-cost, high-strength glass fiber, is of value.
- The goal of this project is to reduce composite contribution to the cost of Type IV COPVs by 50% by replacing T700 carbon fiber with glass fiber. The central premise of this project is the desire to produce glass fiber with strength exceeding carbon fiber with half the cost. The approach in Budget Period 1 is to produce this high-strength fiberglass using a new manufacturing process, build and test tanks, and model performance and cost improvements using tank data. In Budget Period 2, the team wishes to improve the fiberglass, build large tanks and test at higher pressure, and investigate safety factors. The approach is reasonable, but it hinges entirely on the team's ability to produce the required high-strength glass fiber. This ability has not been demonstrated so far. A one-year no-cost extension has been sought to address these shortcomings.
- The approach of evaluating alternative glass fibers that could potentially have better mechanical properties than carbon fibers while being lower-cost is reasonable; however, the project has shown no evidence of proprietary glass fibers that can even achieve properties that are comparable to T700. The project has been in a no-cost extension during Budget Period 1 and still cannot achieve the properties required, so it is not likely that this approach will be successful. The U.S. Department of Energy also recently funded a similar effort through the Small Business Innovation Research program comparing basalt fibers to a T700 COPV, and it added so much weight to the overall system that the automotive companies were not interested in the concept, so it is not clear why this concept is continuing to move forward.

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

This project was rated **2.0** for its accomplishments and progress.

- Substantial difficulties have been encountered with the glass fiber manufacturing process, and a one-year no-cost extension was granted in order for this issue to be addressed. Assuming that these manufacturing issues can be successfully addressed, substantial progress during the next performance period could be expected. The reviewer was not pleased with the comparison to E-glass tanks. While substantial improvement, especially in cycling, was noted with respect to E-glass tanks, it is not clear that this will correlate in any way with performance in comparison with T700 tanks. Testing during the next reporting period must be compared with T700 tanks in order to prove feasibility.
- The approach is valid; however, the strength of the glass is not sufficient to offset the additional weight and additional manufacturing cost required for the added fiber. Sizing technology may help, but most likely not enough to compensate.
- There have been issues with the fiber quality attributed to fiber processing that are not resolved. Despite the fiber quality, prototype tanks were prepared for high-pressure testing. This does not seem to have value.
- Of the various tasks listed, only two tasks have been accomplished to date: (1) novel fiber development and (2) tank modeling and validation. All of the remaining tasks have been delayed. Two types of glass (A and B) with types of binder (I and II) have been created by PPG Industries, Inc. (PPG). Thirty-six vessels have been made at Hexagon Lincoln; however, the size of these tanks was not specified. Also, it is vexing that nowhere is the design operating pressure of the tanks presented. It was found that while glass fiber A and B had a pristine single fiber strength of 5357 MPa and 5583 MPa, respectively (exceeding that of T700 carbon fiber of 4900 MPa), unfortunately, there was a 40% loss of strength in translation. This loss of fiber strength is a huge setback. If it cannot be addressed, then clearly the entire project is in jeopardy, and the stated goals will not be met. The principal investigator (PI) compared the high-strength glass fiber tank performance against reference E-glass. It is not clear why the team wants to compare against E-glass. The team's stated objective is to exceed T700 performance, so all comparisons must be made against T700 tanks and not E-glass.
- The PI acknowledged that PPG has had many problems manufacturing the glass fibers but could not provide a reasonable path toward success. Therefore, this project does not seem to be producing reasonable accomplishments and has no clear pathway to achieve success.

Question 3: Collaboration and coordination with other institutions

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

- The partnership between PPG, Hexagon Lincoln, and Pacific Northwest National Laboratory (PNNL) seems to be working well.
- The team collaborates with a tank manufacturer and a national laboratory, which is very useful.
- The project seems to be clearly utilizing the expertise of PNNL to guide some of the evaluation of designs and utilizing the expertise of Hexagon Lincoln to develop tanks. However, without actual material, there is not a clear pathway for continued collaboration.
- The partnership with Hexagon Lincoln for tank production is functioning well, as is the experimental stress rupture work being done with PNNL. The role of PNNL beyond this experimental work was not described in the slides/oral presentation.
- The team includes a high-level tank fabricator and DOE laboratory technical support.

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 desire to produce safe, reliable, and cost-effective hydrogen storage tanks is well aligned with DOE objectives.
- The ability to produce high-strength glass fiber that could replace the carbon fiber is expected to reduce tank costs.
- If the project is successful, a dramatic cost reduction for 700 bar hydrogen storage would be possible. However, the impact of the associated weight penalty on driving distance (anti-lightweighting) should also be considered.
- The project seems to align somewhat with the DOE efforts to lower costs, but it is not completely apparent that these high-density glass fibers, combined with the safety factor of 3.0, will ever be viable candidates to supplant T700, so the potential impact is probably lessened by this fact.
- The approach and relevance are appropriate, but the results are not as encouraging as hoped.

Question 5: Proposed future work

This project was rated **2.4** for its proposed future work.

- Future work appears reasonable. It very much relies on solving the manufacturing issues encountered to date.
- The path forward for PPG to overcome the manufacturing hurdles of producing adequate glass fibers was not clear. It seemed that the company was not completely supportive with moving forward on this project and re-evaluating, so the prospects for future work are unclear.
- Everything hinges on the researchers' ability to produce high-strength glass fibers with low translation loss. If they cannot solve this technical challenge, then the whole project is in jeopardy.
- Glass-fiber-based tanks may be more appropriate for hydrogen refueling storage or delivery to replace steel. The added tank weight does not seem appropriate for transportation systems. The project needs a business model.
- There exists no concrete plan to tackle the quality issue of the fibers themselves. Continuing tank testing despite the fiber quality issue is not productive.

Project strengths:

- PPG has expertise in glass fiber manufacturing, which can be leveraged to produce hydrogen storage tanks with low composite cost compared to carbon fiber.
- The project has significant potential for system cost reduction.
- Having both the know-how and a production facility for glass fiber are project strengths.
- The project is good from the standpoint of fiber cost reduction.

Project weaknesses:

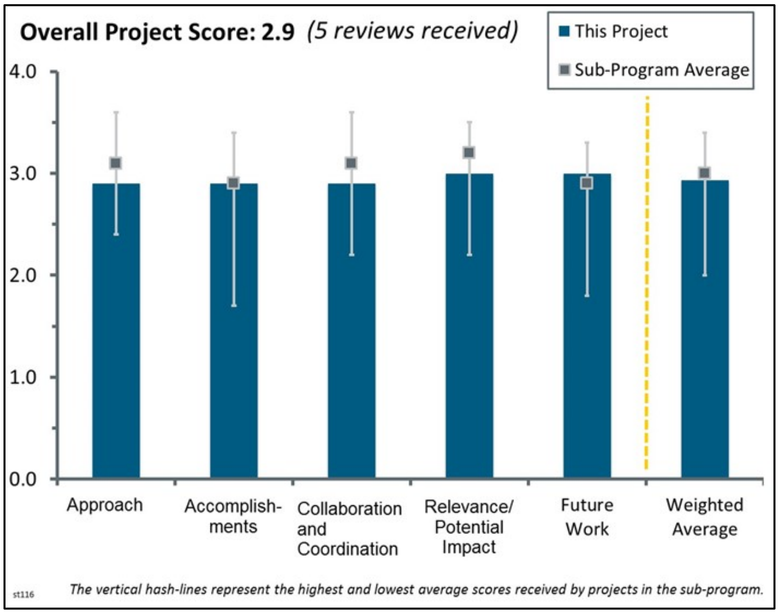
- Manufacturing difficulties have been substantial. While it seems that there is a path to success, a good deal of uncertainty remains. Lack of comparison to T700-based tanks is still an issue. It is not clear why the team is building and testing tanks with sub-performance fiber.
- The project seems to have limited progress and coordinating issues.
- High translation loss in glass fiber strength has proven to be a major impediment to progress.
- The project relies too much on materials development rather than on tank design and testing.

Recommendations for additions/deletions to project scope:

- It is hoped that the team can successfully address glass fiber manufacturing issues to produce fibers with low translation loss.
- The project needs to examine the business case and best application for the glass fiber.
- The project should consider the impact of the weight penalty associated with changing to glass fiber from carbon.
- It is recommended that focus be directed on improving the quality of the fiber for a proof of concept. In addition, the impact of the glass fiber on the tank’s weight should be addressed.

Project #ST-116: Low-Cost α-Alane for Hydrogen Storage

Richard Martin; Ardica



Brief Summary of Project:

Overall objectives of this project are to reduce production cost of α-alane (aluminum hydride, or AlH_3) to meet the U.S. Department of Energy (DOE) 2015 and 2020 hydrogen storage system cost targets for portable low- and medium-power applications. Results will enable broader applications in consumer electronics (e.g., smart phones, tablets, and laptops), back-up power, unmanned aerial vehicles, forklifts, and vehicles.

Question 1: Approach to performing the work

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

- A well-explained, justified, and focused approach lends credibility to the overall effort. For example, the approach for using a moving particle bed electrochemical reactor was justified, as it enables improvements in process kinetics (e.g., throughput, reactor size, etc.), important considerations for commercialization. The approach to estimating costs, including commercial-scale estimates provided by Albemarle, was well characterized and well presented, lending confidence in the outputs.
- The approach to estimating costs seems to follow standard procedures and incorporate the relevant processing steps. The overall approach addresses the objective of lowering alane cost through the electrochemical synthesis method at a relevant scale. This effort is directed at addressing DOE barriers and targets in portable power.
- The presentation this year was improved with greater detail provided in both the Annual Merit Review (AMR) slides and reviewer-only slides.
- Little was known about the electrochemical process for alane production developed at Savannah River National Laboratory (SRNL) at the time Ardica performed their original, limited economic analysis of the industrial-scale production of alane by this method. As more information about the chemical, physical, and mechanical complications of this system has come to light, the promise of this approach to low-cost production of alane has become increasingly dim.
- The project has developed a detailed cost model for alane production via the electrochemical route. The projected cost ($56/kg alane) is about 50% lower than that produced by the chemical route. The key model assumptions and parameters, however, should be independently verified by a third party such as Strategic Analysis, Inc. At the highest production level of 3200 MT/yr, the cost model predicts a cost of $30/kg alane, which is three times higher than the target in project milestone M1.03 preceding the go/no-go decision. If there are no major changes to the manufacturing process for achieving the cost target, a no-go decision should be considered.

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 project made good progress in three areas: (1) much-improved details (compared to 2015 report) in the cost model, (2) fabrication and operation of the particle bed, and (3) partial recovery of alane-tetrahydrofuran (THF) adduct from electrochemical cells. A main challenge that needs to be overcome and demonstrated is the regeneration of $LiAlH_4$ from the products deposited on the cathode.

- Working with project partner SRI and a University of California (UC), Berkeley, researcher, Ardica designed, built, and operated a moving bed electrochemical reactor and characterized its performance with respect to key parameters (e.g., particle size, electrolyte, electrolyte flow rate, etc.), thereby providing not only proof of principle but also early experimental guidance on the cost/process analysis of a scaled-up process. The cost analysis described was derived from a credible process flowsheet developed with the project's commercial collaborator, Albemarle, and yielded information valuable to determining research and development needs to reduce costs, improve throughputs, etc.
- The team has analyzed a significant number of variables providing some guidance to direct where research should focus to get the most advantage. Some discussion at the end regarding the need for work on reducing cell resistance without the need to worry about cell potential could have been handled much better. Ohm's law would suggest that resistance (R) and voltage (V) would be directly proportional.
- Progress in mapping a potential (electro) chemical process with all relevant materials and methods appears good, although it is not clear that some of the individual steps have been designed in a final scalable manner. Progress in demonstrating new efficient process steps in appropriate scale is sporadic and largely constitutes a series of one-off results. The project aims to "maximize" $LiAlH_4$ recovery; results point to characterization of cathodic products. The project also aims to "optimize" the fluidized reactor; results indicate a design has been chosen that will facilitate evaluation of various parameters, so perhaps optimization will occur. With the project more than 50% complete (even at slide submission), there is a need to make further progress in a prototype reactor that allows realistic evaluation of alane production rates and yields and that actually produces alane (or at least the adduct) in sufficient quantities (perhaps kilograms) to give confidence in the economic assessment. Recrystallization of the adduct is also needed at scale for the same reasons.
- The economic analysis reported this year was a vast improvement over the analysis that was presented a year ago. However, as came out during the question-and-answer period, several experts feel that the costs of the recrystallization, hydride recovery, alane separation, electrolyte recovery, and/or alanate regeneration are still significantly underestimated. Even more concerning is that the effort to develop an electrochemical cell with high enough performance to meet system targets is still in the notional design phase. As a result, a heavy component of the "capital costs" portion of the cost estimate is based on a guess of the cost to produce "a scaled-up version of a prototype of a yet to be fully designed particle bed electrochemical cell." This makes the whole cost estimate unreliable.

Question 3: Collaboration and coordination with other institutions

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

- The end user, Ardica, clearly has an excellent collaboration with SRI International (SRI), who has a very good collaboration with Professor Evans at UC Berkeley and with Albemarle, who is looking at the cost and process modeling. The collaboration with SRNL was less well defined, and it is not clear whether there is any discussion between SRI and SRNL, or whether any is needed.
- There is good collaboration with SRNL and other partners.
- There seems to generally be good communication between SRI, Ardica, and SNRL. However, it was disconcerting that the SRI presenter said that the problem of dendrite formation persisted right after the SRNL presenter told us this problem had been solved. Professor Evans is a very nice addition to this team.
- There was some discussion about conference calls between Ardica and SRNL but not much discussion that included SRI.
- Communication with partners appears good, but the evidence for effective collaboration is not so great. The use of THF is an example of good coordination, but SRNL appears to be working on a different adduct for recrystallization. There is no mention of the MgNi cathode development presented by SRNL in the current project—this may need to be incorporated. SRNL also claims $LiBH_4$ (as well as $LiAlH_4$) is critical for recrystallization; this was not apparent in the detailed chemical process plan. The partners also seem to be duplicating some work. Perhaps there is an opportunity for some separation of effort here, with one partner focusing on a critical step that the other requires. Collaboration with UC Berkeley appears more straightforward, with this partner providing advice on reactor design.

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 project goals support DOE goals in portable power though cost-effective regeneration of a high-capacity storage material.
- Although alane has an extremely high energy density, its use as an energy carrier is impractical because of the high cost of its production through standard chemical methods. The novel electrochemical process for the production of alane discovered at SRNL might change this situation. Thus, an attempt to develop a practical, low-cost process version of the SRNL electrochemical process is of high relevance to the DOE goal of developing advanced hydrogen storage systems for fuel-powered portable electronic devices.
- There is good potential for this project to provide a viable material that meets the targets for medium- and low-power applications for the U.S. Department of Defense and others, making this a very relevant project for DOE. As the approach is well defined and the accomplishments are timely and appear to be leading to progress toward viable processes, this project may also end up having an impact in the storage media field.
- This project is relevant to portable low-power systems that use alane as a hydrogen storage material. The cost of alane is prohibitively high for light-duty automotive applications.
- The project appears to align with the needs for small power, and the main target for research seems to be the U.S. Army; it is not clear why this is being funded by DOE.

Question 5: Proposed future work

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

- The future work is well posed, concise, and logical. The team appears focused on critical issues (e.g., kinetics, yield, recyclability of solvents and reagents, etc.) that the team's process spreadsheet indicates result in reducing costs.
- The future work proposed is generally appropriate and directed toward overcoming barriers. It is somewhat generic, and there is little evidence of detailed planning, decision points, or risk mitigation strategies.
- The proposed future work is on target, but it will be difficult to complete it within the time and budget remaining for this project.
- In the remaining time, the focus on the reactor is important.
- The proposed pulsed method reduces process efficiency considerably. The project should look into mechanical methods for removing and collecting dendrites.

Project strengths:

- The project is focused on key parameters that will enhance throughput and reduce process costs. The team and collaborations are good.
- The team has considerable experience in alane synthesis and is very focused on solving the technical issues.
- SRI has vast experience in the development of alane syntheses.
- There is detailed consideration of processes for economic assessment, although some of the steps are still being worked out. There is focus on materials and processes with potential to have an impact on DOE goals.

Project weaknesses:

- While much of the work has been to demonstrate proof of principle for various aspects of the process, there was little shown or discussed about what the path to scale-up might comprise. It is hoped that the next review will include more experimental evidence that scale-up is occurring and achievable.
- The presenter seemed to be missing the point of several questions. In the future, he needs to be more direct with answers. Asking someone if he/she is a reviewer and then telling him/her to look at the reviewer-only

slides comes across as less than genuine and leaves the audience wondering if there is an answer or just a weak knowledge.

- Progress in the scalable reactor appears to be somewhat empirical, with little evidence of a carefully planned approach. Collaboration could be more effective with partners contributing complementary results.
- Regeneration of $LiAlH_4$ could be a major road block in terms of cost and efficiency.
- The limited initial understanding of the SRNL electrochemical process has proven to be a weak foundation for this project.

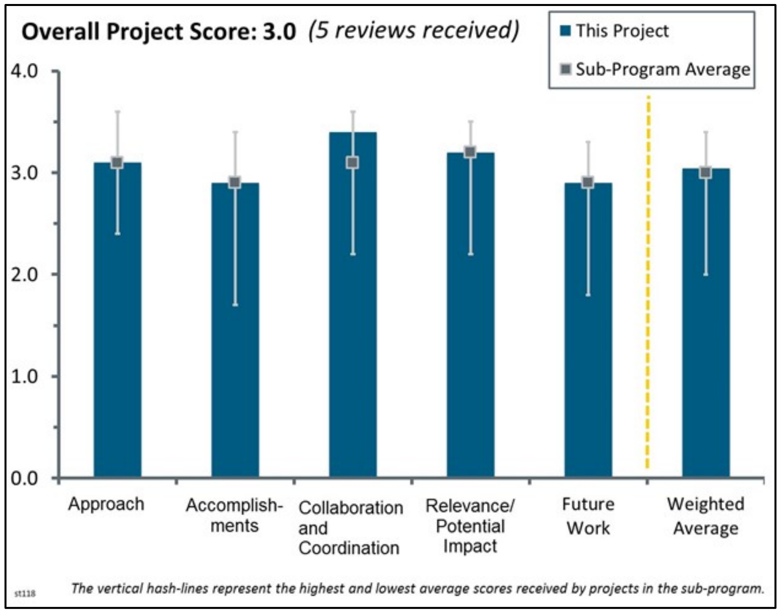
Recommendations for additions/deletions to project scope:

- The project needs clearer division of labor with partners, better division of effort and better incorporation of results from one partner to another, and a more focused effort to build a reactor with high throughput and appropriate product qualities (this is planned but warrants reinforcement).
- The project should incorporate mechanical methods such as scrapers to prevent the dendrite growth problem.
- Some emphasis should be placed on looking into the implications for doing a semi-commercial scale process using high-flammability solvents such as THF; where this figures into the eventual cost is unclear. If THF adduct formation is required and no other solvent substitute can be employed, working with THF could be problematic at scale.

Project #ST-118: Improving the Kinetics and Thermodynamics of $Mg(BH_4)_2$ for Hydrogen Storage

Brandon Wood; Lawrence Livermore National Laboratory

Brief Summary of Project:



The objectives of this project are (1) to combine theory, synthesis, and characterization techniques at multiple length/time scales to understand kinetic limitations and possible improvement strategies in $Mg(BH_4)_2$ with relevance to other light-metal hydrides, and (2) to deliver a flexible, validated, multiscale theoretical model of (de)hydrogenation kinetics in “real” Mg-B-H materials and use predictions to develop a practical material that satisfies 2020 onboard hydrogen storage targets. Current project year objectives are to synthesize MgB_2 nanoparticles with <10 nm diameter, measure x-ray absorption and emission spectra for bulk MgB_2/$Mg(BH_4)_2$ during stages of (de)hydrogenation, and compare measured and simulated spectra on informed models to determine local chemical pathways.

Question 1: Approach to performing the work

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

- The combination of theory, synthesis, and characterization across multiple time and length scales provides a tremendous opportunity to gain a deeper understanding of kinetic processes in $Mg(BH_4)_2$. The research and development (R&D) team understands the barriers and challenges that must be overcome in this system, and the project has developed an innovative research approach and work plan that is fully consistent with achieving success. However, it is somewhat disappointing that work on cycling and reversibility is deemed by the project team to be beyond the scope of this project. Kinetic limitations are being addressed using nanoengineering and catalytic additives. Although this approach is sound, care must be taken to ensure that agglomeration of “free” nanoparticles does not limit kinetics (this was pointed out in the presentation—it is simply being reinforced here).
- The main objective of this work is very ambitious. The multiscale problem, both theory and experiment, presents very difficult tasks, and so far nothing has been completely successful in this area of science. The project’s efforts are appreciated. Trying to separate the different components of the process, particularly from the modeling side, is very hard. The main problem in statistical sampling in theoretical models is the presence of rare events such as diffusion, dissociation, etc. The models are always going to be “toy models” of reality. On the other hand, the experimental part is more solid because it is just observation of reality.
- This work ties in well with the ongoing efforts within both the Hydrogen Materials–Advanced Research Consortium (HyMARC) and the validation team. It is not clear how this work was delineated from the HyMARC effort when the principal investigator (PI) was constantly referring back to HyMARC.
- This effort employs computational approaches to evaluate the energetics and spectral properties accompanying hydrogenation (apparently of at least the initial hydrogen bound to either the surface or edge on slide 11).
- To date, this project has taken a highly fundamental approach, nicely integrating theory, characterization, and synthesis. It has been successful in that it has provided important new insights into the processes controlling the kinetics of the hydrogenation of MgB_2. However, the goal of this project is to make a

significant improvement in the performance of the $MgB_2/Mg(BH_4)_2$ system as a hydrogen storage material. Increasing the kinetics of hydrogenation of only 10% of the material without changing the dehydrogenation kinetics falls very short of this goal. A much more practical approach needs to be adopted in the next year of the project.

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.

- Initial results and progress are encouraging. Calculations of B-H and B-B bond scission and formation in $Mg(BH_4)_2/MgB_2$ provides important baseline information. Likewise, identification and interpretation of intermediate species is critical to quantifying limitations to kinetics. The demonstration that hydrogenation of MgB_2 under high-pressure conditions can lead to direct conversion to $Mg(BH_4)_2$ is a signature accomplishment that provides an "existence proof" that unwanted reaction intermediates can be suppressed. Initial theoretical studies of phase nucleation and nucleation barriers and interpretation of reaction intermediates through comparison with experiments are providing useful guidance for the experimental work. This is the first project that provides a theory/modeling capability to address reaction kinetics issues in a complex hydride in a comprehensive way. The effort to prepare size-selective nanoparticles seems to be progressing at a slower pace. The procedure for nanoparticle synthesis (surfactant-assisted ball milling and Stavila stripping method) may be too aggressive (brute force) to permit the formation of nanoparticles with a well-controlled size distribution. It will be important to rapidly evaluate the efficacy of this approach for achieving project goals for nanoparticle formation.
- The project should further explore the heterogeneous surface reactions that can occur and how that would constantly be varying the dissociation versus diffusion shown on slide 10 (9). The information on slide 12 (11) began to address this issue. The PI pointed out several times that the issue tends to be that the changes initiated are going to change as hydrogenation occurs. However, this may create a "diffusion" barrier as the hydrogen tries to get into the bulk—but this also means that the bulk is being constantly redefined as hydrogenation occurs—i.e., you no longer have the homogeneous material with which you started. The point at which bulk versus edges/basal planes are defined as hydrogenation is unclear. It is unclear whether new "introduced" materials stresses can be manipulated to generate more basal plane sites. The only reason a 4.0 was not given was slides 16 and 17, on which the equation shows a BH_5^{2-} species. What the PI was trying to picture was clear, and he explained it well in the question-and-answer (Q&A) period. However, it is recommended this be done differently in the future.
- Evidence has been found that indicates that the hydrogenation of MgB_2 occurs on the surface through direct hydrogenation to BH_4^- rather than through a borane intermediate process as occurs in the dehydrogenation of BH_4^-. This is a very significant finding that could be of major importance to the development of this material. On the other hand, the results of the computational studies seem to be predicting processes that do not make chemical sense. Although it was explained that the nonsensical species BH_5^{2-} appeared in the presentation as a typo, it is still not clear that sensible species are being predicted by the computational studies. This underscores that results of the computational studies need to be subjected to critical evaluation and reconciled with fundamental chemistry.
- Segments of MgB_2 are depicted on slide 11. The size of the MgB_2 used to determine the energies is unclear. It is unclear whether there is a binding energy dependence on cell size. It is unclear whether the binding energy is determined for the initial hydrogen atom only. There is no indication as to what would happen if more hydrogen atoms are successively added to the cell. There appears to be a difference in the experimental x-ray absorption spectroscopy data shown on slide 10, which shows a sharp, well-separated pi* transition that is less obvious in the computational spectrum. No explanation of this difference was provided. Slide 13 mentions hydrogenation at interfaces. Given the schematic depiction, this appears to indicate that interfaces means edges in this case. If this is the case, then the interpretation could be that an effective strategy for direct hydrogenation is to maximize a particle's geometry where edges dominate. This would presumably run counter to the notion that a particle morphology that maximizes the normal to the c direction minimizes surface free energy. On slide 16, the issue was raised regarding the molecular fragment stoichiometries and charge depicted in the reaction. It is uncertain these fragments are known to exist in nature.

- Accomplishments include the molecular dynamics (MD) shown in slide 17. If a reaction happens spontaneously in a computer simulation using MD, then either the model is very wrong or the material reacts spontaneously. This is dealing with rare events. The model should be revisited. Phase nucleation is probably going to be a big problem: how to determine the energetics and kinetics of the interfaces between phases, hydrogen transport, etc. The sampling of possible structure combinations is enormous and unlikely to be possible in a rigorous way (slides 20 and 21).

Question 3: Collaboration and coordination with other institutions

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

- The collaboration within the project team is excellent. There is good communication with other groups doing related DOE work.
- Extensive collaborations exist with other HyMARC partners and external institutions. As new projects are folded into HyMARC, this project will undoubtedly serve as a centerpiece for enhanced collaboration and cooperation among all participants. Understanding sorption thermodynamics and kinetic processes in the $Mg(BH_4)_2$ system is particularly challenging, and it requires the full complement of capabilities available in the HyMARC project.
- The experimental partners are solid. The connection between the Nuclear Magnetic Resonance confirmation and some vibrational spectroscopy is not properly explained. It is just stated and used for fingerprinting. In order to reproduce the experimental data, addition of a modeling demonstration is necessary.

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 effort is an integral part of a vital project, HyMARC, and for the overall DOE Hydrogen Storage program. The project is providing the consortium with a powerful means to obtain a deeper understanding of the fundamental processes operative during sorption reactions in complex metal hydrides. Likewise, this project offers a pathway to guide future work on emerging systems. It has significant potential to advance progress toward achieving DOE goals. The work on $Mg(BH_4)_2$ is especially important because this system is among only a few complex hydrides that has potential to meet DOE storage targets.
- The $MgB_2/Mg(BH_4)_2$ system currently is the most promising candidate among the complex hydrides with potential to meet DOE targets.
- The choice of material seems to be driven by the old-fashioned "weight percent" criteria, i.e., it has a good deal of hydrogen, so an important step is to see how to get the hydrogen out, nanosizing, etc. This is unlikely to be a useful material itself. It seems to be more of an academic interest (which is necessary and very important but at odds with "impact in real life").

Question 5: Proposed future work

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

- The proposed effort to develop and improve catalysis in the next phase of the project is a good step toward its becoming more practically oriented. However, further studies should be focused on overall hydrogen cycling performance and not just the hydrogenation or dehydrogenation half-cycle.
- The proposed future work is provided mainly in sentences given at the bottom of a few of the "accomplishment" slides. That makes it difficult to offer a thoughtful and complete review of the proposed work. That being said, it is fairly straightforward to "read between the lines" and understand at least to a reasonable level what is planned for future work on the project: alternative microstructures with interfacial transport paths, deeper understanding of phase nucleation processes and barriers by incorporating stress and interfacial penalties; exploration of why there are differences in results from experiment and theory for

stability of intermediates; etc. A more concise and self-contained summary of future work would be helpful.

- Most of the problems have been identified. It is still uncertain whether it can be done. Furthermore, if we understand the formation of the hydrides, it is unknown if it can be made to work.

Project strengths:

- A very strong team with extensive expertise and capabilities in theory/modeling, materials synthesis, and characterization/testing is conducting research on this project. The investigators are well-versed in the challenges and issues that must be overcome for a complex system such as $Mg(BH_4)_2$ to ultimately be successful as a practical hydrogen storage material. The integrated experiment–theory approach is powerful, facilitating progress toward gaining a deeper understanding of the critical kinetic processes and rate-limiting steps operative during hydrogen sorption reactions in $Mg(BH_4)_2$.
- It is a combination of state-of-the-art techniques from the modeling point of view. It aims to study materials and processes in great detail. Results could be transferable to other systems.
- The PI and the team are excellent. The additional efforts at HyMARC can only serve to move this forward.
- This project has an excellent core of integrated fundamental theory, characterization, and synthesis efforts.

Project weaknesses:

- Although the project team is especially strong in theory/modeling, metal hydride synthesis, and surface characterization, there seems to be limited expertise in the chemistry of metal hydride systems. Bringing in an individual (or individuals) with a deeper understanding of complex metal hydride chemistry would be beneficial. It was stated during the presentation (Q&A) that studies of reversibility and cycling are beyond the scope of the current project. However, these issues should not be ignored or marginalized (cycling efficiency and reversibility are DOE mandates). For DOE's hydrogen storage application, these issues are every bit as important as understanding thermodynamics and kinetics issues. In fact, cycling/reversibility studies provide an indirect way to explore the presence and evolution of rate-limiting intermediates. It is important for the project team to consider expanding the scope to include at least limited studies of cycling and reversibility.
- The project is oriented toward incremental advances in the fundamental understanding of the system, rather than producing a significantly improved hydrogen storage material.
- The observation of hydrogen using x-rays is indirect. This is probably not the best choice, in particular when it comes to comparing theoretical models. Statistical sampling of rare events and energetics of a large number of possible structures is very difficult; a calculation of few structures needs to explain $>10^{23}$ atoms in real life and be predictive. It is a hard problem, and the approach is over-selling the capabilities of theory.
- Stronger interaction with Jensen/Autrey in the future is suggested.

Recommendations for additions/deletions to project scope:

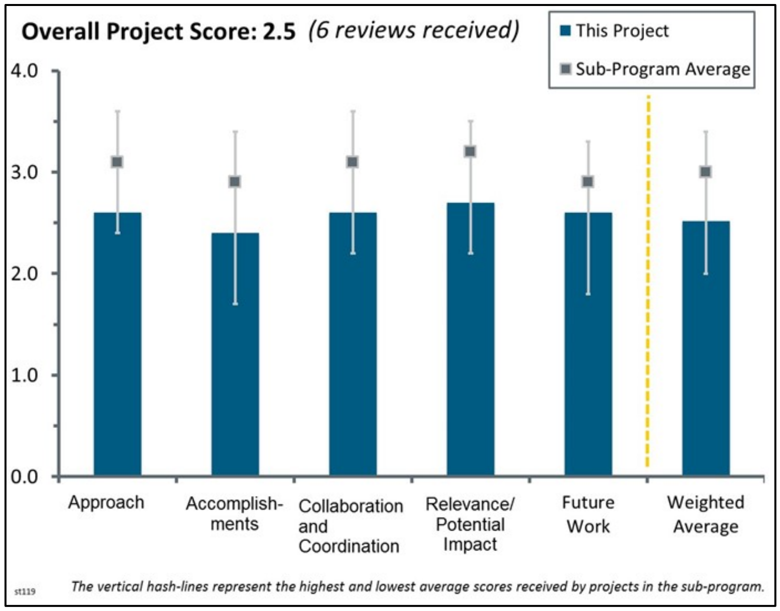
- The project can use additional complex hydride chemistry expertise (either via collaboration, a new funding opportunity announcement project, or addition of a staff member). The project can expand the scope to include at least preliminary studies of cycling and reversibility.
- The project should address the statistical sampling of structures in the models at the ab initio methods level. That way, the project can try to validate the models in a more convincing way.

Project #ST-119: High-Capacity Hydrogen Storage Systems via Mechanochemistry

Vitalij Pecharsky; Ames Laboratory

Brief Summary of Project:

This project is developing novel high-hydrogen-capacity silicon-based borohydrides and composites with the aim of achieving low-cost, high-performance hydrogen storage materials. Silicon-based borohydride materials are predicted to have borderline thermodynamic stability. Researchers will use stabilization strategies based on hypersalt formation using alkali and alkaline-earth cation additions to bring the enthalpy of desorption into the targeted range. The project will also investigate borohydride/graphene nanocomposites that utilize graphene's advantageous properties.



Question 1: Approach to performing the work

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

- The project aims at making high-capacity Si borohydrides via mechanical chemistry (ball milling) and substitution of alkali and alkaline earth elements to help stabilize them. There are calculation, synthesis, and programmed hydrogen desorption components of candidate materials. This is a new idea, never tried before, and certainly has the theoretical potential to meet DOE targets at reasonable desorption temperatures. It is a high-risk, high-reward effort and is certainly worth trying. The chemistry is complex and sometimes not predictable and calculable. The project seems hard to judge as to feasibility and not very integrated with other DOE efforts.
- The investigators have approached the project exactly as proposed: first using computational methods to determine potential candidate materials, followed by related mechanochemistry experiments.
- This project is based on the shaky foundation that the targeted borohydride compounds will undergo *reversible* dehydrogenation and that they can be synthesized by ball milling techniques. Unfortunately, this is not consistent with the findings of the worldwide effort over the last 10 years to develop mixed metal borohydrides. Hundreds of mixed-metal borohydrides have now been synthesized, and none of them has been found to undergo reversible dehydrogenation. Recently, this was found to be due to the concurrent elimination of diborane upon thermal dehydrogenation. Furthermore, successful syntheses of lightly stabilized aluminum borohydrides has been achieved only through low-temperature "wet" chemistry approaches, *not* ball milling. Thus, mixed-metal borohydrides that undergo reversible dehydrogenation and the preparation of compounds with *borderline stabilities* through ball milling seems unlikely. Likewise, the isolation of compounds with silicon in the +2 oxidation state seems unlikely. Finally, this project does not employ the techniques that have become standard in the structural characterization of borohydrides: IR, multinuclear nuclear magnetic resonance (NMR), and structure determinations derived from Rietveld analysis of powder x-ray diffraction (PXD) data. Instead, the approach is to obtain only the raw powder diffraction pattern (that reveals only that LiCl is formed) and inconclusive NMR data and then speculate on the nature of the compounds that are obtained.
- The idea of using computer modeling to identify the stability of phases is not new. This proposal tries to lead the discovery by calculating possible solid compounds and then trying to synthesize them. There is a

prediction of materials that are not known to exist, and the proposed work is to use ball milling to create them. The driving force is the "weight percent" metric.

- The U.S. Department of Energy (DOE), DOE reviewers, and the project investigators should review the literature and work funded by DOE's Office of Basic Energy Sciences (BES) and accomplished by Savannah River National Laboratory (SRNL) and Virginia Commonwealth University. They have made both Si and Al hypersalts and exploited the benefits of borohydride/graphene composites when using C_{60} for the carbon sources. This work seems merely to attempt to reproduce those efforts.
- The project is aimed at addressing gravimetric capacity barriers and has proposed high-risk materials that could offer progress. The presentation suggests the project aims to improve understanding of hydrogen chemisorption, but there is no evidence of this. The objectives include composites with graphene for thermal conductivity, but without sufficient knowledge of the proposed borohydride salts, the reason for this is unclear.

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

This project was rated **2.4** for its accomplishments and progress.

- During the first project year, many interesting results have been generated using fundamental calculations and mechanochemistry synthesis trials. New stabilized Si borohydride compounds have been made, some of which have promising H-desorption properties. The science being developed so far in this project is very interesting and of potential future use in DOE projects.
- This is an unknown area of exploration and, therefore, there is no guarantee that promising Si-borohydride hypersalts or borohydride/graphene composites will be found. Having said this, it is worthwhile to take this approach to search for such new materials. Despite investigating various systems, there does seem to be only fleeting success as of yet, which is not necessarily the investigators' fault.
- The researchers have investigated a relatively large number of systems and carried out a good number of computational and physical experiments. However, the computation has made some counterintuitive predictions that are not explained, and the experimental products are not well-characterized, especially in a quantitative sense. The concept of Si^{2+} is not one that is soundly based in chemistry (Si forms covalent rather than ionic products), and it would be good to see some literature precedent for Si^{2+} compounds. Of course, it is sometimes necessary to explore new territory, and the project may be an example of this. The characterization of products does not give much information, especially quantitative. The formation of LiCl does indeed indicate that a reaction has taken place, but to what is not clear. The NMR shows BH_4 remains in some samples and has undergone decomposition during preparation of others. The use of "onset temperature" for hydrogen release is not especially illuminating and could be misleading if this represents a small fraction of the sample. Pure phases of standard hydrogen storage materials (including borohydrides) often show a lower "onset temperature" after high-energy ball milling, but this is attributable to a highly defective surface or amorphous regions that do not survive cycling. The weight changes presented do not represent a progression toward DOE goals. As one of the reviewers noted, the conclusion of no B_2H_6 formation is not justifiable given the experimental protocol and the findings of other groups regarding B_2H_6 detection by mass spectrometry.
- The computer screening shows some metastability of certain compounds. Experimentally, the trials show that most of the systems show far less "weight percent" than the calculations, and some diffraction data seem to support the formation of certain compounds. There is not unequivocal determination of the formation of the SiH_4 species. So far, nothing apart from some calculation-based predictions has been demonstrated. There is no unique set of theoretical models that support the stability of these compounds, so the relative stability can have different orders or not exist at all using other functionals, etc. The evidence that this is true is very thin.
- These efforts have generated only poorly characterized mixtures of materials, the most promising of which *irreversibly* eliminates 3.2 wt.% upon thermal decomposition. In view of the approach that is being taken by this project, the lack of progress is not surprising.
- The accomplishments seem mainly from the theoretical side, and results are not promising for producing the theoretical materials. The team should refer to literature and reproduce SRNL's results from hypersalts as a starting point, where the real concern is stabilizing these materials.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.6** for its collaboration and coordination.

- The collaboration with the University of Missouri–St. Louis (UMSL)—a project partner—appears strong. No evidence exists of collaboration with other groups.
- They seem to be collaborating fine.
- It is not clear how to judge this since the project does not stress collaborations with other institutions. Nonetheless, it seems like the project could benefit from other experimental techniques from other DOE-funded entities such as the Hydrogen Materials—Advanced Research Consortium (HyMARC).
- The collaboration with UMSL is adequate and producing most of the results presented so far. However, the recommendation is to pull in institutions that have modeled and produced these materials to ensure this is not just reproducing their efforts.
- Interaction within the project team is good, but otherwise, this effort is quite insular. The inclusion of this project within HyMARC should be a vast improvement in this regard.
- There is only one collaboration partner (Majzoub at UMSL for a priori calculations). The project could use more chemistry collaborations and added outside ideas.

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 **2.7** for its relevance/potential impact.

- This is clearly a worthwhile endeavor since the idea has not been explored to any significant extent yet. It is somewhat high-risk but also potentially high-reward.
- The project is certainly relevant to the Hydrogen and Fuel Cells Program and DOE research, development, and demonstration objectives. However, it is a very new approach and too early to predict the potential impact. It is an interesting and worthwhile addition to the DOE storage projects.
- There is currently a scarcity of promising new candidate complex hydrides. Therefore, the relevance of this project is simply the clear need to explore previously uncharted synthetic territory. However, the investigators appear to be unaware of the lessons learned by the study of similar materials. Thus, it is likely they will, at best, learn lessons already learned and, at worst, make preventable mistakes that will cause confusion and be a harmful distraction.
- The materials could have the potential for impact, but it is not certain this project will achieve that level. It is not really clear how DOE could fund this project with the understanding that BES has already funded these efforts, and it does not seem that this project is advancing the state of knowledge beyond that which is known.
- This is a very unlikely set of compounds from a chemist's point of view. It is not proven that they can even exist from a simple energetics point of view, not to mention that it is even more unlikely that they will have the right kinetics. If any of these compounds can be formed, it could be of some interest from the point of view of an example of the methodology. However, proving that the methodology is correct requires far more than few examples.
- The project is a high-risk effort to discover new materials that can meet DOE targets. However, results to date do not give confidence that this will pay off.

Question 5: Proposed future work

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

- Proposed future work seems reasonable with respect to the funding provided.
- The proposed future work plans are very reasonable. However, the planned borohydride/graphene composite work may detract from the main Si borohydride mechanochemistry challenge.

- The plans to confirm the identity of products and to apply some quantitative analysis are what this project needs. Reversibility is of course a good aim; however, this could come later once it is clear what has actually been synthesized.
- In view of what has generally been previously found for mixed-metal borohydrides, the proposed efforts to isolate the products from the ball-milled reaction mixtures and to grow single crystals are unlikely to be successful.
- This is more of the same that has been done in the first part of the project.

Project strengths:

- Project strengths are the following: a potentially new class of promising compounds is being explored; the combined theory/mechanochemical approach seems highly appropriate to the task; and the investigators are known to be competent researchers in the field.
- The aim is a completely new class of materials with potentially very high gravimetric H-contents.
- The project is making an attempt to make novel compounds to try to meet DOE goals.
- The project uses computer modeling to drive the discovery of materials.

Project weaknesses:

- The project is high-risk. It will likely take much longer than the project timeframe to see whether these materials have practical potential to meet DOE goals for vehicles.
- Characterization of products is poor (particularly quantitatively) and not aided by presentation of results (all the blue-stripe x-ray diffraction patterns are extremely difficult for a reviewer to interpret, for example). The concentration on initial decomposition temperature instead of temperature to release a stoichiometric quantity of hydrogen is also a weakness.
- Project weaknesses stem from the limited success so far. It seems clear that more characterization techniques and more experiments are still needed to find out what is being formed. The investigators would benefit from using outside help from other potential DOE partners, such as HyMARC.
- It is a very simple approach: trying to characterize the energetics and extrapolate to the stability of phases. There is no exhaustive search of other models, etc. It is not a unique, accepted, and well-defined theoretical tool.
- The project team does not appear to have a grasp on the vast, recent body of papers that has appeared on mixed-metal borohydrides in the last eight years.

Recommendations for additions/deletions to project scope:

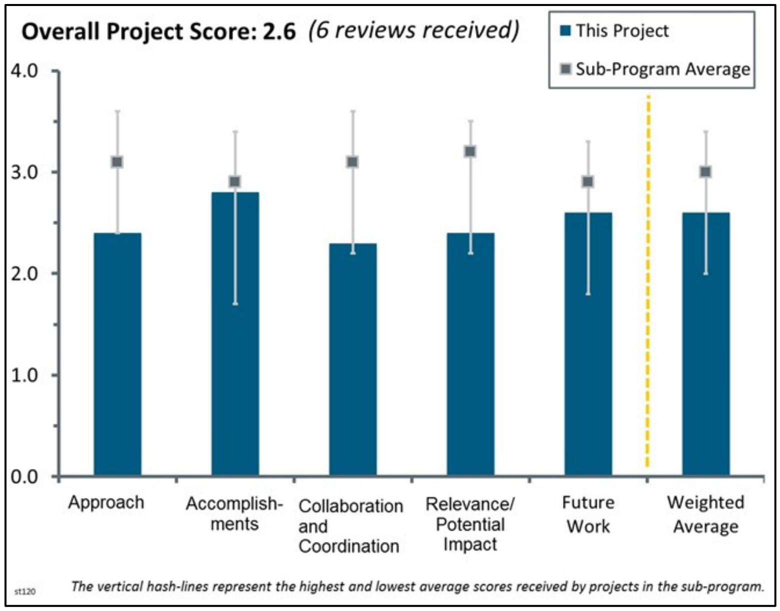
- The project needs to take a more quantitative approach, starting with a mass balance of reactants versus products. Product mixtures need to be much better characterized, as is planned (and perhaps more so). The recommendation is to concentrate on this aspect first and not investigate composites with graphene.
- Perhaps there should be a little more outside collaboration. The borohydride/graphene composite work could be deleted to allow more time on the main Si borohydride mechanochemistry effort.
- The project should drop the borohydride/graphene composite tasks if they get in the way of any Si-borohydride advances. The investigators mentioned that there are no precursors with Si^{2+} available. If they have not already, the team could look into $MSiH_3$ salt compounds.
- The project should be terminated.

Project #ST-120: Design and Synthesis of Materials with High Capacities for Hydrogen Physisorption

Brent Fultz; California Institute of Technology

Brief Summary of Project:

This project aims to address challenges related to the volume of onboard hydrogen storage systems and the low temperature and low enthalpy of adsorption. Researchers are designing and synthesizing materials with high capacities for hydrogen physisorption. The focus is on graphene rather than activated carbon as single-layer graphene is a platform with an excellent surface-to-volume ratio. The project will use graphite oxide chemical routes and plasma approaches to synthesize and functionalize the materials.



Question 1: Approach to performing the work

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

- This is a new project with an approach to "functionalize" single-sheet graphene. The focus appears to involve deposition of single metal atoms on a graphene surface. The principal investigator (PI) shared progress in setting up a laboratory to measure sorption of hydrogen onto metal-doped graphene, as well as a project plan providing some insight into what has been accomplished and what is planned over the next months. This was a good start, but more details would be extremely helpful.
- The systematic approach proposed by the authors is refreshing.
- The guiding hypothesis for this project is not at all clear from the presentation. It is agreed that graphene has a higher surface-to-volume ratio, which should favor uptake of hydrogen, but no support is provided for the somewhat broad statement ("functionalize it") on slide 5. It is not clear *why* it should be functionalized and with what rational design principle. Treatment using graphite oxide chemical routes and the plasma physical approaches will no doubt change the material, but no clear reasoning is presented to indicate why this should improve hydrogen adsorption. The deposition of gold nanoparticles on the surface is also puzzling. It is well known that gold atoms and clusters diffuse across graphite and graphene surfaces with very low activation energies (e.g., see Jensen et al., *Surface Science* 564 [2004]: 173–178). Consequently, it is likely that clusters will form at low-energy locations, such as edges (Zhang et al., *Phys. Rev.* B 81, 125425 [2010]) or steps (P. Jensen et al., *Phys. Rev.* B 70, 165402 [2004]). Perhaps that is the intent, but it was not made clear. Recent experiments suggest that three-dimensional (3D) island growth will result because of the very low adsorption energy on graphene and high coalescence energy of gold (Liu, *Phys. Rev.* B 86, 081414(R) [2012]).
- The project seeks to use single-layer graphene to support metals for enhanced hydrogen storage. The project uses several techniques to methodically step through each treatment process and catalogue everything that can be catalogued. It is not clear what utility this will bring in the long term, and some hydrogen uptake results and/or characteristics would be more meaningful and are urgently needed for the project.
- It is unclear whether the functionalization will be successful. It is unclear whether the functionalization is aiming for addition of atomic metal sites or nanoparticles. Furthermore, there exists the possibility that agglomeration of the metal additives will occur, resulting in low-surface-area features and formation of metal hydrides. It is not clear how this will be prevented. It is unclear whether the metals to be used for

functionalization—which, based on slide 8, appear to be heavy (Au, Cu, Co, Ni, Zn)—will achieve high gravimetric densities. It is not certain that this approach can achieve high volumetric densities.

- There are two major problems: the lack of defect sites in graphene, and the assumption that the gold is generating atomic Au at the surface. These are the cornerstone assumptions of the project. It was not proven in accomplishments that these assumptions are valid. Overall, the project was not well defined.

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.

- The project appears to be on track, based on the milestones. Synthetic approaches projected to be tested at this stage of the project have been completed. New Sieverts apparatus was installed, and a method of rapid screening of materials based on single-point measurement was demonstrated.
- Progress has been good. However, the project should look into some questions about the distribution of gold atoms on the surface. Density functional theory calculations and experiments from Manchester suggest that it is difficult to avoid clustering of gold atoms unless the graphene is a few layers thick (Zan et al., *Nanoletters* [2011] dx.doi.org/10.1021/nl103980h).
- This was perhaps the most organized part of the presentation. It would have been better if there were more insight into specifics of how the laboratory setup was going to be used to demonstrate future progress toward DOE goals.
- Given the way this project is progressing, it is difficult to assess whether it is strongly moving toward DOE goals. The project has been active for only about eight months. Clearly, hydrogen adsorption characterization has not been strongly emphasized as yet and needs to be more aggressively pursued. The rather simple single-point isotherm analysis is not a new concept and has been discussed in the hydrogen storage centers before. That it was a milestone was surprising. The PI detailed some interesting results for the graphene preparation (i.e., of high quality) and some hard-to-understand results of Au on these sheets. There is very little evidence that Au atoms exist individually, as they usually coalesce into particles (one known exception being a particular surface reconstruction of an iron oxide surface) unless very cold. It is not clear that this approach will work as intended, and more than transmission electron microscopy images are needed to characterize this.
- Reaction schemes are needed, not pictures of the reactions in the laboratory. On slide 11, it seems the plasma may generate a series of defects in the material. Also, with a 300 keV beam, the particles will move around—and the beam can introduce more damage to the substrate. On slide 12, in the bright field there are typical particles agglomerated on the surface, ranging in size from 1–5+ nm. It is surprising that the dark field image on the right of what is apparently the same region does not show these bigger particles. The interpretation that the small lighter areas are atomic gold is a stretch—it appears to be more just how the graphene is wrapped around the grid, as well as the depth of field. It is highly unlikely that the gold would not agglomerate because of surface energy effects. On slide 14, a large distribution of 1–2 nm particles is visible on the surface, indicating not-unexpected movement. The beam is heating the metal and graphene indirectly, causing an Ostwald-ripening-like effect. On slide 15, the same activation process has been performed as one would use for activated carbon.
- Work on functionalization has not progressed very far. Some careful benchmarking studies have been completed.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.3** for its collaboration and coordination.

- The project does not currently rely on significant external coordination. Ted Baumann and Robert Bowman were listed as collaborators, but there was little insight into the roles. The PI also works with Channing Ahn, who has expertise in both sorbents and metal hydrides and would be valuable to consult to prioritize specific metals to focus efforts.
- This seems to be a mostly self-contained project, so extensive extramural collaboration is not expected/not applicable.

- The PI may want to consider relying more on the expertise of Ted Baumann or Channing Ahn to characterize the materials.
- The project involves a collaboration with Lawrence Livermore National Laboratory (LLNL), but no evidence was provided that this provided any benefit to the project.
- There seems to be little interaction with anyone outside the PI's own laboratory. No collaborative results were 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 **2.4** for its relevance/potential impact.

- Graphene is a relatively new material, and its potential for hydrogen storage is not fully characterized. Consequently, this project seems to align well with Fuel Cell Technologies Office goals, given the relatively low level of information available for this material. Given the uncertainties associated with the approach indicated above, however, the potential impact is still rather unclear.
- There is a very basic effort here that has not really been at the pace needed for DOE to realize its goals. The basic nature of the work is needed—it would be excellent to see whether this approach has promise—but the actual hydrogen results are absent so far.
- This is a basic science project with an interesting methodology. However, it is not obvious that a clear pathway to a new material is emerging at this stage.
- There was some discussion as to why graphene chosen as the support. The weakest part of the presentation was why specific metals are under consideration.
- Until the PI has proven his claims, this work will not be taken seriously. Too much past history on spillover graphene oxide, and the relevant fuel cell work on platinum-group metal/substrate interactions were not considered.
- The presumption is that functionalization of graphene/graphene oxide could increase hydrogen storage density. However, the potential gains over conventional carbons were not clearly identified in the presentation.

Question 5: Proposed future work

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

- Only milestones were given, and these are reasonable.
- Emphasis should be placed on materials that could yield high gravimetric capacities: functionalization using lightweight elements or those that can bind multiple hydrogen molecules. Estimates of volumetric performance should be provided, too.
- Proposed future work is not described in any detail. The only information provided is the project plan on slide 8. This, however, is not cast in the form of a timeline, so it is unclear what work will be the focus during the coming year. Most of the project elements seem to make sense, but the atomic layer deposition TiO_2 work seems out of place; no mention of it is provided anywhere else in the presentation.
- The PI should carefully consider a more specific hypothesis and more specific research goals. These may be known but were not well explained in the presentation. The concept that is being tested was unclear. It could have been room temperature sorption, multiple metal sites, particle size, optimized dispersion of metal particles, etc.

Project strengths:

- The project focuses on a novel material (graphene) with potential for high surface area and functionalizable groups. Results obtained during the previous year indicate that the experimental strategies are working; surface areas increased considerably after activation, and gold nanoparticles were successfully deposited on material surfaces.

- The project aims to develop new hydrogen adsorbents. If successful, this could lead to lower-cost hydrogen storage systems. Using graphene/graphene oxide as the skeleton material could enhance thermal transport during refueling.
- The project has a systematic approach, enhancing gas–surface interactions.
- Strengths are simple assumptions that can be easily tested; good characterization.

Project weaknesses:

- Weaknesses include the PI's apparent inexperience with the importance of the Annual Merit Review (AMR) and the opportunity to show reviewers sufficient information in a 15-minute presentation. The PI needs to ask for, and listen to, advice of collaborators with years of experience. It was obvious from the AMR presentation that there has been very little interaction with collaborators. The discussion with reviewers at end of the presentation lacked detail. The PI did not clarify the rationale for the planned future work when given the opportunity. The PI showed a beautiful high-resolution image of the carbon matrix, but the resolution shown for the Au atoms appeared to be lower, leaving the audience and reviewers in some doubt as to the PI's claims of distribution. There was no scale bar on the graphene image, and the reviewer cannot recall whether the image was from the PI's laboratory or an image taken elsewhere to illustrate the nature of the graphene surface. It was also disappointing that the PI was not better able to answer questions about keeping metal clusters dispersed on the graphene matrix. If this data were presented to a journal for peer review, there would need to be more connection to previous published work. The PI did not appear to know much about the published work in this area, or if he did, he did a poor job of putting his work into perspective.
- The project's rationale and guiding hypotheses are not well defined. The approach seems to ignore existing literature. Collaborations appear to exist only on paper. The project lacks a theory element that could guide synthesis.
- The AMR presentation was missing some aspects that might be better considered next time around and have a more hydrogen-centric presentation with more results. The approach was relatively simple, but a series of X's in a table with no real indication of meaning or results is not helpful.
- Stability of the functionalization (via agglomeration into bulk metal) is a concern. A second concern is that the projected performance gains are unclear.
- There were too many claims without experimental evidence.
- The path to a final material is a bit cloudy.

Recommendations for additions/deletions to project scope:

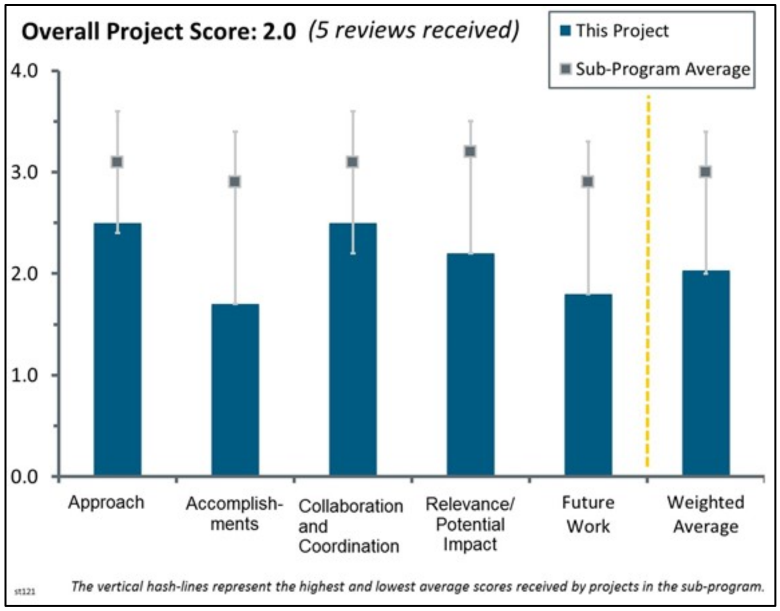
- Clear hypotheses needs to be defined (or at least articulated). The investigators should consider the implications of existing literature concerning the mobility of gold and other metals on graphite and graphene surfaces and use this information to guide experiments. A theory component would be beneficial. Advantage should be taken of the collaboration with LLNL, presumably to provide high-surface-area carbons. The contribution of Bob Bowman needs to be defined.
- There are likely some very good ideas behind the work plan. However, the PI must understand that the AMR is a review. It is important to lay out work illustrating accomplishments, approach, collaborations, and future work. The reviewers are there to help make the project better, but it is hard to help if one has to guess what is happening. DOE could ask the PI to review projects in the next AMR. This would provide a good opportunity to appreciate how to present all the information needed for a thoughtful AMR presentation.
- The project should clarify the steps to achieving a 3D functional material.

Project #ST-121: High-Capacity and Low-Cost Hydrogen-Storage Sorbents for Automotive Applications

Hong-Cai (Joe) Zhou; Texas A&M University

Brief Summary of Project:

The project is designing robust hydrogen storage materials—high-valent metal–organic frameworks (MOFs)—that offer the potential to meet the U.S. Department of Energy's (DOE's) gravimetric and volumetric targets. Researchers aim to determine strategies that allow materials to exceed the traditional limits of carbon-based materials (1 wt.% hydrogen per 500 m^2/g specific surface area). Strategies include (1) increasing hydrogen affinity relative to surface area and (2) using x-ray techniques to study oxidation state and solvation changes to better understand the activation process at the metal center. Reducing synthetic steps for precursors will help keep costs low.



Question 1: Approach to performing the work

This project was rated **2.5** for its approach.

- The goal of surpassing Chahine's rule for hydrogen uptake is a valid one and would be helpful to the Hydrogen and Fuel Cells Program if it were to be accomplished.
- The principal investigator (PI) has an excellent background in the development of new framework materials for gas sorption. However, the effort, not the approach, seems to be lacking in this project. The slide on barriers was not done correctly; these are not the DOE-defined barriers but the PI's interpretation of barriers.
- The approach to improving on the Chahine rule is reasonable, and MOFs are a good candidate for achieving the DOE goals. PCN-250 might be a reasonable choice for cryogenic adsorption, given its stability. The use of x-ray absorption near edge structures (XANES) might not be the most useful way to determine the degassing temperature, but the project has some results that should correlate with laboratory techniques (though the project team has not attempted to show this comparison). Using both Mossbauer and extended x-ray absorption fine structures (EXAFS)/XANES to characterize the Fe changes with activation is reasonably attempted. The reasoning behind the changes in oxidation state appears to be secondary to actually proving that there is an increased adsorption enthalpy upon oxidation change. There are several high oxidation state MOFs in the literature, and none has shown a large increase in adsorption enthalpy; the MOF-74-$Fe^{2+/3+}$ change, in particular, showed only a small difference (Long, Dalton Transactions).
- Targets and challenges are well defined, but the scientific hypotheses for addressing them are not clear. Increasing the number of strong binding sites, optimizing the pore dimensions, and improving the effectiveness of activation are worthy objectives but not scientific strategies. All three of these are well-known objectives for MOF-based sorbents. The project appears to be focused entirely on improving the gas uptake of PCN-250. The primary objective is to get the uptake to be ≥6.75 wt.%—clarify whether this is excess or total—at 40 bar. The best so far is 4.8 wt.% excess. The goals are very ambitious, but strategies beyond PCN-250 are not clearly defined.
- The approach to addressing the problem of high uptake has been articulated in principle, but there is no adequate description of why this particular open metal site structure is suitable as a hydrogen storage

medium. The project should articulate what the potential energy contours show, how they were calculated, and why they are relevant. There appears to be no significant charge redistribution on the basis of the image shown on slide 8. If this is the case, then a high initial Henry's law enthalpy will be seen in the vicinity of the open metal site, but once that site is occupied, the enthalpy will decrease to values similar to any normal carbon. Fe has an atomic mass of 56, and hydrogen has an atomic mass of 2. The project should define the strategy that will ultimately provide insight that suggests that this particular structure will form the basis of a suitable sorbent.

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

This project was rated **1.7** for its accomplishments and progress.

- Detailed XANES investigation of the activation process was performed. Results suggest that the structure of MOF begins to degrade as temperature is increased. It appears this happens almost immediately as the temperature is raised above room temperature. Removal of the OH group coordinated to iron in the framework requires reducing the oxidation state. This might be possible by adjusting activation conditions, based on Mossbauer data. A systematic investigation of changing the oxidation state of the framework Fe^{3+} to Fe^{2+} is underway.
- So far, there are no strong results related to DOE goals beyond what has been conceptualized and published in the literature.
- The claim of a gravimetric uptake greater than expected on the basis of surface area should have been the basis for this presentation. It appears that no effort was made to understand how this was possible. The first thing that should have been done was to perform a D_2-based neutron diffraction experiment to understand the phenomenon. In the past, understanding higher-than-expected uptake would prompt a serious effort to identify and understand the observation. This does not seem to have happened with this material. The discrepancy between the initial uptake determined by the PI and the difference measured by the National Renewable Energy Laboratory (NREL) was not adequately addressed. The team needs to describe where the initial measurement was done, why those data were not shown, and why there was no explanation given as to the difference in measurements.
    - Figure 10 shows XANES data—a simple density functional theory calculation based on the expected densities of states should have also been performed. The drop in intensity that is stated in the slide is not obvious. Even if it is significant, the XANES peak appears to continue to drop with temperature, without attribution as to why. Slide 11 shows the radial distribution functions (RDFs) of the Fe peak—the rationale for this analysis should be explained. While it is understood that EXAFS provides coordination data, no analysis was done, and again, all we see is a continuing decrease in peak intensity. Slide 13 shows Fe Mossbauer data from a dobdc structure. This is revisiting a system that has been synthesized before; the project should discuss to what end this work was performed. It is already clear that while MOF-74-type structures are interesting, as they show H_2 distances closer than expected, the rationale for this effort was not articulated.
    - Figure 15 shows the results of compression. Since a compression load is indicated, pressure data should be included. The force data give absolutely no idea as to the pressure that the sample can tolerate, which will depend on die size. The surface area of the material used here is lower than the 1600 m^2/g that was presumably measured earlier. Such an inconsistency in specific surface area (SSA) needs to be reconciled. The data as presented on this slide are of no value or meaning at all.
- It was very disappointing to see the incompleteness of the effort; however, even worse was the extrapolation of apparent preliminary work to such broad-based conclusions and implications. How the density was calculated, as explained by the PI, appeared to be wrong. While it appears that NREL did show a sorption that was greater than Chahine's rule, the reasons for this are many. Not all solvent was removed for BET analysis. Long showed how he determined all solvent was removed with infrared spectra; it is suggested that Zhou's group follow suit. Gennett showed how an 8 m^2/g material absorbed ten times Chahine's rule, but seemed to suggest there was a pore structure/volume that could not be accessed from N_2 BET measurements. The PI has the capability to do CO_2 BET measurements with all of his previous CO_2 work. It would be interesting to see the pore volume of this material for the CO_2 measurement. The go/no-go milestone was not achieved. The compaction calculation also appears to be misleading. Also—if one looks at slide 15, figure B—it is interesting how, after degas, the density decreases by 35%. It leads one to

believe there is a considerable amount of solvent in the materials. Slide 15 appears to be a misrepresentation of results; it assumes a direct correlation between the packing efficiency and the capacities. The work of Veenstra in the engineering center on MOF-5 shows this. It is strongly suggested that these pellets be tested for hydrogen uptake *before* claims of packing efficiency can be evaluated.
- The first go/no-go milestone was not met. The goal was to achieve 6.75 wt.% excess, whereas only 4.8 wt.% was measured by NREL.

Question 3: Collaboration and coordination with other institutions

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

- There seems to be reasonable collaboration between Argonne National Laboratory (ANL) and Texas A&M University; samples have also been sent to NREL for verification.
- Collaboration with Carnegie Mellon for Mossbauer is underway, as are x-ray studies at ANL. The project needs to state explicitly whether they are at the synchrotron light source—the presentation does not say. The purpose of the atomic layer deposition studies at ANL should be defined. Slide 18 (Remaining Challenges) says that Texas A&M University investigators will "cooperate with other research groups and institutes to confirm the repeatability and recyclability of PCN-250 in hydrogen storage," but no specifics are provided.
- The interaction with ANL and NREL should be tied.
- The existing collaboration with ANL does not appear to have produced anything. The XANES work was presumably performed there, but no scientific insight was presented, other than to probe for desorption of dangling components or solvents. Neutron scattering would help this project tremendously and should be a top priority.
- The project team seems to be working with ANL and has sent some samples to NREL, but collaboration seems to be weak.

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 **2.2** for its relevance/potential impact.

- If this works, it might be useful. The real issue is the focus at 77 K in that it cannot meet DOE goals.
- It was disappointing—considering the past efforts of the PI—that such claims were made without any data to back them up. The potential impact is outstanding; however, the information as presented is not.
- No clear goal has been articulated. While it is understood that materials development is a difficult task, there are certain physical principles that have not been laid out that describe the strategy that will improve volumetric or gravimetric density. The analytical methods sidestep any real attempt to understand the behavior of the materials that are being developed, and it is not clear that any progress from this effort is actually aimed at addressing programmatic issues.
- The objective of increasing the excess uptake of PCN-250 to a value nearly double that predicted by Chahine's rule by using metal ion reduction to remove coordinated anions could have a significant impact on the field, if accomplished. However, the presentation does not indicate what the theoretical maximum excess capacity is expected to be, so it is not possible based on the information provided to determine what the potential impact will be. However, the best experimental number at this point is 4.8 wt.%, and the project missed its target milestone of 6.75 wt.%, so it is unclear by what means the ultimate goal of >9 wt.% excess uptake will be achieved for even one MOF, much less the 5+ specified in the Quarter 7 milestone.

Question 5: Proposed future work

This project was rated **1.8** for its proposed future work.

- Improving adsorption enthalpies and making new materials for Milestones 2 and 3 are excellent goals, but a priority needs to be on sample reproduction ability and experimental capabilities.
- Slide 18 lists vague directions for this effort, which is tied to a single material, PCN-250. Unfortunately, almost nothing has been suggested here that offers a motivation or vision of the particulars of this material. The PI did not present volume density data on this material and, in general, seems totally unengaged with the needs of this project in addressing the physical attributes of a suitable sorbent. It is not clear that the PI has contacted groups in a position to clarify the phenomena he has observed, and the experimental routes he has suggested do nothing to address project goals.
- The proposed work (slide 19) is cast in an aspirational way, rather than stating what work specifically will be done. Beyond PCN-250, no other MOFs are mentioned, so it is simply unclear how this will be accomplished.
- The project should clarify why it is to continue if a go/no-go decision was not reached.
- Having missed the first go/no-go milestone, it is unclear how much extra effort should be devoted to PCN-250.

Project strengths:

- The project PI is one of the top people in the MOF field and has all of the resources necessary to perform the work required by the project milestones. The collaborations appear strong as well and are providing useful information.
- Discovery of new, high-performing hydrogen adsorbents is an important objective. A successful outcome would clearly aid the development of low-cost hydrogen storage systems.
- The PI and the materials proposed are project strengths.

Project weaknesses:

- Specific details concerning the strategies for producing MOF sorbents with excess capacities well above those predicted by Chahine’s rule are not spelled out.
- The PI does not appear to have any interest in establishing the phenomena responsible for the observed uptake in a cogent way so that physical principles can be developed that can be used to better design an adsorbent. The PI was evasive in his answers to reviewers and never fully explained the discrepancies or rationale for the analytical approach used on this material.
- Greater accuracy is requested in future presentations. The PI should check some of the assumed data. Slide 5 has images of supposed packing densities. Solid para hydrogen in the hexagonal close-packed form has a nearest-neighbor distance of 3.793 Å (the lattice constant at 4 K, by Keesom in 1930), but there is variation if there is an ortho-para mix. This solid distance is larger than the calculated Lennard-Jones potential minimum, and larger than that observed in the liquid (neutron scattering and simulation ~3.7 Å). Generating pellets of MOFs is not a useful step at this point given how variable the samples/experimental techniques are for this group. These issues need to be fixed before spending time on projects that will need to be repeated. The PI indicated that the pellet density was determined by immersion in water—clearly this is not a useful way to do that. The PI should review the data and processes in the laboratories to be aware of what is happening. More attention should be placed on understanding the RDF of the activated product, as a rather large second coordination shell at 2.3 Å seems to be growing after activation and cooling. The MOF-74-Fe Mossbauer data was published by the Long group—it is not clear why the data are included here. Relevant material would be better.
- The lack of effort and attention to detail are project weaknesses.
- Missing the first go/no-go milestone is a major concern.

Recommendations for additions/deletions to project scope:

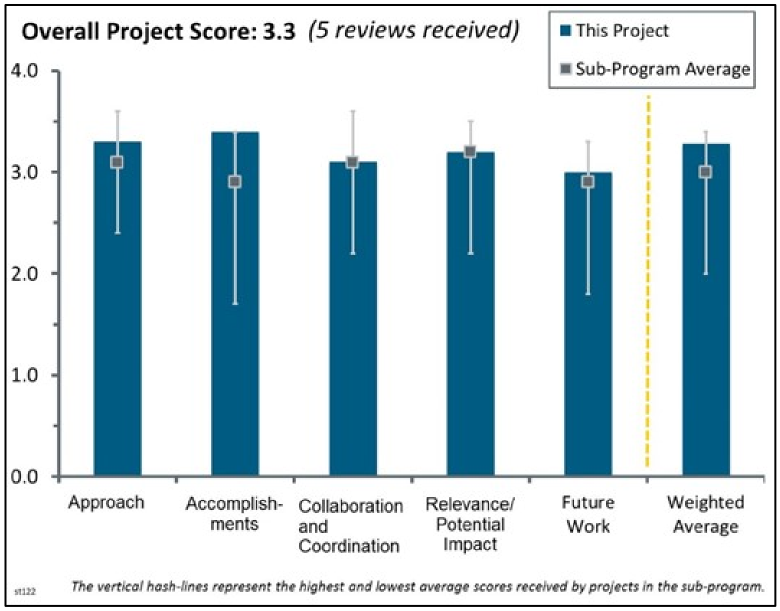
- For future evaluations, the project needs to explain the strategies that are being evaluated for designing new MOFs with the requisite properties. Predictions of the maximum excess uptake achievable for the proposed structures are needed to determine whether the end-of-project goals are achievable.
- The project should work with DOE and NREL to ensure adsorption capabilities are on a par with other laboratories. The project should ensure sample variability is at a minimum. Hydrogen adsorption isotherms should be measured as soon as possible.
- The work on PCN-250 should be scaled back. A clear plan for identifying adsorbents that exceed the Chahine rule by 50% is needed.
- This project appears to have missed the initial milestone of reproducing the results that were claimed in the proposal. This brings the credibility of other results from this project into question.

Project #ST-122: Hydrogen Adsorbents with High Volumetric Density: New Materials and System Projections

Don Siegel; University of Michigan

Brief Summary of Project:

A high-capacity, low-cost method for storing hydrogen remains one of the primary barriers to the widespread commercialization of fuel cell vehicles. Storage via adsorption is a promising approach, but high gravimetric densities typically come at the expense of volumetric density. This project's goal is to demonstrate best-in-class metal–organic frameworks (MOFs) that achieve high volumetric and gravimetric hydrogen densities simultaneously, while maintaining reversibility and fast kinetics. The approach entails high-throughput screening coupled with experimental synthesis, activation, and characterization.



Question 1: Approach to performing the work

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

- There is nice integration of synthesis, theory, and characterization. The project is using supercritical CO_2 to maximize surface area (trying to get to 90% of the projected area). System-level projections are also underway, in collaboration with the Hydrogen Storage Engineering Center of Excellence (HSECoE).
- As an extension of what the team was doing in the HSECoE, this is logical and well executed.
- The project has a systematic approach using numerically efficient tools. The approach to calculate uptake could be improved; results are specific to one temperature. Results obtained using a classical parameterization of the hydrogen–hydrogen interaction are not really relevant.
- It is an interesting project: evaluating the possible outliers in the framework database for possible improvements to achieve theoretical limits. The principal investigator (PI) does need to gain access to the Materials Genome Initiative database.
- The basis for this effort is shown on slide 8, but the data here bring into question the approach adopted to justify some of the materials that are being studied. In general, a linear dependence on uptake with surface area has not been observed, leading to problem data points such as SNU-21, which has a volume uptake comparable to liquid hydrogen. This was an unphysical data point that required some reanalysis and justification for this part of the presentation. Determining a relationship that optimizes how uptake is influenced by the surface-to-volume ratio would be illuminating for this effort and would suggest physical properties that should be pursued or optimized for uptake, allowing the project to rely less on a data-mining effort.

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.

- Codes for doing computational screening were updated. Grand canonical Monte Carlo (GCMC) is being used to predict adsorption isotherms. The project identified an improved force field that gives greater fidelity to experiments. A large number of MOFs (>2000) were screened computationally to identify structures with the desired capacities. This identified a large number of MOFs that could exceed MOF-5 uptake, in some cases by large amounts. Seven MOFs were identified that could potentially surpass MOF-5 by 15%. A much larger computational screening effort is underway, which would consider >50,000 new materials from the Cambridge Crystallographic Data Centre. The initial go/no-go was met; IRMOF-20 was identified and exceeds the MOF-5 benchmark. A number of other MOFs that were predicted to be promising could not be synthesized with high surface areas according to literature procedures (this is not uncommon). Supercritical CO_2 activation experiments show that this method delivers superior properties (higher surface areas) than the standard vacuum activation; in some cases, the differences are huge (e.g., UMCM-9), so this is an important result. A new database was developed (MOF Dashboard) to track progress on promising materials.

- The project has done excellent work to put the idea of some materials to rest, while others deserve more investigation. The accomplishments were limited only because the PI did not access the Materials Genome Initiative database. It appeared that, by the end of the presentation questions, he was made aware and will access the database in future efforts.
- Identifying optimal mass and volumetric parameters through computational screening, then targeting those materials with a feedback loop to avoid overlooking materials that may be more difficult to activate than others, is efficient. The project has at least one success story in unearthing an overlooked MOF that shows promise. It would be better if these were for room-temperature storage.
- The force field approach seems to do a reasonable job of modeling isotherm behavior. Slide 17 indicates that “intuition” was used to identify compounds of interest. It is not clear what this means. The data on slide 20 indicate that some improvement to gravimetric capacity was calculated, but the volumetric capacities are similar. Because of the larger size of the unit cell, the question is whether this ultimately indicates that a larger “tank” would be required to take advantage of the higher gravimetric uptake.
- Proof of concept is achieved. Other selection criteria could be included.

Question 3: Collaboration and coordination with other institutions

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

- Collaborations are good. Synthesis and modeling are done within the University of Michigan by separate investigators. Ford is a subcontractor doing pressure–composition isotherm measurements, scale-up, and characterization. These appear to be well integrated within the project. An unfunded collaboration with the HSECoE is also underway to assist in development of system models.
- Given the amount of computational screening and characterization that goes on with the Materials Genome Initiative, the project should be working across DOE-funded laboratories to ensure minimal redundancy. There are some known efforts run by Basic Energy Sciences (BES) in this area that have external facing prescience for databases. The collaborators do work well among themselves. The system-level model is understandably a bit absent, given the youthful nature of 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 **3.2** for its relevance/potential impact.

- The project is striving to achieve both high volumetric and gravimetric capacities, which is rare in MOFs (or sorbents in general). The goal is to find a MOF that exceeds the performance of MOF-5 (used as a benchmark). Emphasis is still on cryogenic storage, however, so even if the project is successful, the resulting material will not meet DOE targets regarding storage temperature.
- The combination of theory and experiment is working well and can address some DOE goals. It would be useful, however, if the efforts can be used to scale up the temperature of operation.
- The project is identifying materials that show improvements over MOF-5.

Question 5: Proposed future work

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

- Proposed future work is in line with the goals of the project. However, the project would have a larger impact if the screening could take into account more than surface area. It is clear that open coordination sites on MOFs are needed to get heats of adsorption to the point at which room temperature (or at least some temperature above 77 K) is feasible.
- Also of interest is the nature of adsorption enthalpies in materials of this type. While a modified form of a universal force field is used within a Lennard–Jones type 6-12 model, it is not clear whether the computational effort will provide insights on the isosteric enthalpies of the systems being pursued, although given the closeness of fit, some evaluation of enthalpies would be of value.
- Future work was clearly presented, but it would be good to see efforts to move to higher temperatures.
- The future efforts should include the extended database, so milestones may need to be altered slightly.

Project strengths:

- The team is excellent, with deep experience in all areas needed to perform the project. There are good connections with original equipment manufacturers, and the PI is very cognizant of the practical issues associated with solid-state storage.
- The project offers a possible pathway to a high-throughput evaluation of candidate materials.
- This is a comprehensive, logical, and well-executed project. This is nice work overall.
- The PI team and the completeness of the effort are strengths.

Project weaknesses:

- There are no serious weaknesses in terms of the way the project was originally structured, with the focus on cryogenic storage.
- The project may suffer from duplication of effort compared to other DOE-funded projects in BES. This is easily solvable and might generate some efficiencies by sharing code/platforms, if possible.
- Integration of chemical stability, specific heat, and thermal conductivity requirements are weaknesses.
- Lack of information on other databases is a weakness.

Recommendations for additions/deletions to project scope:

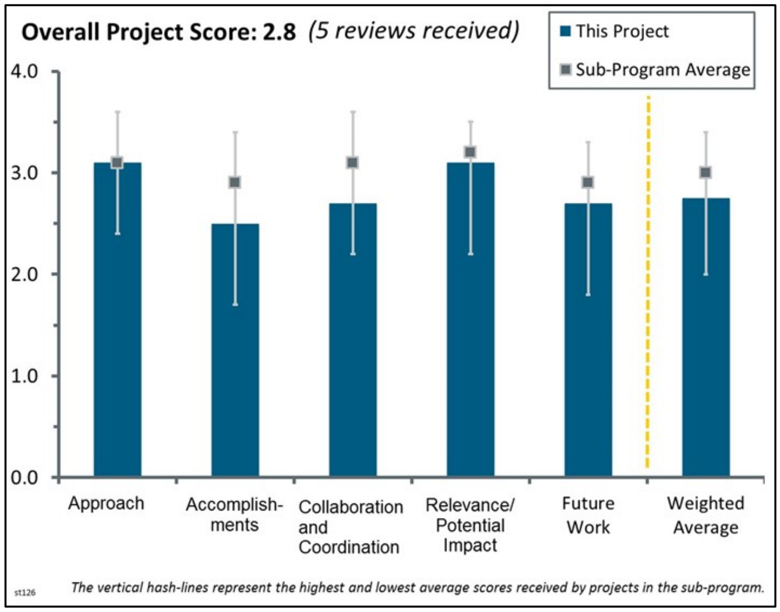
- Taking the possible higher adsorption enthalpies of open metal sites into account in the screening would be valuable. Even though these are difficult to model from first principles, perhaps some empirical relationship could be implemented until more accurate potential energy curves can be incorporated into the force fields.
- The project should target higher-temperature solutions.

Project #ST-126: Conformable Hydrogen Storage Coil Reservoir

Erik Bigelow; Center for Transportation and the Environment

Brief Summary of Project:

The project goal is to develop storage for compressed hydrogen gas that will provide a cost-effective and conformable storage solution for hydrogen vehicles, thereby reducing the cost, weight, and difficult fit of conventional hydrogen tanks. The target is conformable, lightweight 700 bar gaseous hydrogen storage with around 10% gravimetric capacity. Researchers are aiming for continuous production processes for a storage system that can be extended, once proven at smaller sizes.



Question 1: Approach to performing the work

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

- This is an excellent approach to maximize the weight percentage of hydrogen in high-pressure compressed tanks and allow for conformable efficient storage.
- This novel approach has great potential for a preferred method of hydrogen storage for transportation.
- The overall approach for the project is focused on the main barriers for this technology related to the hydrogen permeation, fill temperature limits, and process compatibility. It should be noted that the hydrogen permeation target is incorrect because it is based on the loss of useable hydrogen rather than the permeation value in the industry standards, which is lower. In addition, the project should emphasize evaluating failure modes, such as the evaluation of the burst pressure in the conformable configuration.
- There is a good deal of current emphasis on demonstrating current compressed natural gas (CNG) vessels, presumably with the identified low-permeability liner material. The value of extensive testing at pressures substantially lower than the 700 bar target is not clear. In addition, it does not seem that the possible qualification of these pressure vessels for automotive applications is being considered in any way. Given that the vessels are expected to leak rather than undergo a catastrophic failure, the safety ramifications of these leaks should be considered.
- The approach of developing coiled reservoirs for hydrogen storage is a novel idea, but the presentation does not give a coherent approach to achieving this—only unfounded claims of 10 wt.% with a system mass of 50 kg. The materials presented to withstand such high pressure (700 bar), such as a Kevlar® overwrap and resin-based liner, do not equate. There is no basis for such claims. No data were presented on the composite strength or to show that the resin could possibly support load-sharing.

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 manufacturing approach is maturing and shows low cost potential. The tank design with liner and overwrap also shows promise.
- The accomplishments are good, although more progress was to be expected for a design that has been previously developed for CNG and other applications. Also, the Accomplishments slides did not mention

any progress with quantifying the main objectives for the project, which are the ~10% gravimetric capacity and cost reductions.
- The progress toward a proof of concept has been slow. At this point, only the resin selection and tooling have been completed, despite the fact that the project is due to end by March 2017.
- Considering the fact that this project started in August 2015, the spend rate seems very low. The project will not progress quickly enough to end on time. More specifically, it is not clear what the value is of developing a test vessel for 5,000 psi when the target pressure for the vessel is 10,000 psi. Presumably, this is related to the fact that the vessels are supposed to leak rather than fail catastrophically. However, this is not necessarily the case. It seems all high-pressure testing is planned to occur at the National Renewable Energy Laboratory (NREL). There seems to be no coherent plan beyond testing based on the current CNG vessel configurations. If there is a plan, it was not communicated well.
- No data were presented except for some initial modeling data on the thermodynamics of refilling. There were many pictures of the corrugated molding device and Kevlar spooling, but there was no material performance or cost data to show that this is a viable option. Presumably no real accomplishments have been made on this project toward meeting the goal of demonstrating a 700 bar tank that meets the hydrogen storage technical targets nor the stated 10 wt.% storage target listed in the presentation.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.7** for its collaboration and coordination.

- The effort to expand the collaboration of the project with NREL, CSA Group, and Kuraray Group is encouraging. The project should further explain the respective roles of the Center for Transportation and the Environment (CTE) and the University of Texas. The manufacturing process partnerships should also be provided because it is uncertain whether High Energy Coil Reservoirs is the producer or the developer of the technology.
- CTE's role appears to be to coordinate and report on this project; outside of that, CTE's role is limited. Unfortunately, during the presentation of progress to date, it came across that there was not a complete understanding of the technology being developed by the other partner organizations. Perhaps co-presenting with partner organizations would help to clarify things.
- Future work should include an original equipment manufacturer end user such as Ford to really drive the design and requirements.
- Collaborators were listed, but their contributions were not clear.
- Until this point, there seems to have been no collaboration with other institutes.

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 ability to have conformable 700 bar tanks using Kevlar overbraid with extruded liner can ultimately improve the weight percentage of hydrogen, lower the cost of hydrogen tanks, and allow flexibility in installation. Achieving this will get us closer to meeting the DOE cost and density targets.
- The relevance of this project is very high if the tank can achieve a 10% gravimetric capacity and meet the DOE ultimate target. Although it was not emphasized in the project presentation, the development of a conformable 700 bar tank would be a significant contribution to the industry with the assumption of improved volumetric density.
- The project has high potential impact to make hydrogen storage safe, make it affordable, and make it fit with current automotive designs.
- The project concept and goals are excellent. The ability to conform a hydrogen storage tank to an oddly shaped area would indeed be a game-changer. However, there are some key areas that do not appear to be part of the project. One major issue is safety: it does not seem that a vessel that will leak, releasing an extremely flammable gas, is realistically able to be qualified for automotive applications. Type V (linerless)

pressure vessels with conventional shapes have met with much resistance for similar reasons, so there is no reason to think that these vessels would be any different.
- The project has good alignment with DOE objectives, but there is no evidence that it can achieve those objectives.

Question 5: Proposed future work

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

- Task descriptions (i.e., titles) are laid out in a way that makes sense. However, it did not seem like details beyond the Task 1 go/no-go decision point have really been considered. Given that it seems that the project might already be behind schedule, having these detailed plans in place would enable a quick push forward assuming that the "go" is given. Safety should be considered as a part of the project.
- A few items, such as resin selection and testing of vessels, were fine, but there was no discussion of materials properties testing. There should be solid materials testing and evaluation prior to moving forward on vessel design and testing. There is no efficient feedback mechanism to understand materials properties.
- The work plan is rational and sound. Durability of the dry Kevlar braid as a function of road vibration is a concern. The project needs to include an automotive company to develop possible other showstopper tests.
- The future work description is effective in communicating the path forward to develop the technology. The project should also consider conducting a detailed cost analysis to provide information about its claims about achieving results near the ultimate cost target.
- The proposed work is focused on implementing a proof of concept. However, the proposed work lacks risk management plans, should challenges be encountered. For example, if the tanks are to fail the high-pressure tests, then thicker walls/other materials will be needed, translating to much lower weight percentage and higher costs, which could negate the advantage of the concept.

Project strengths:

- The idea of conformable hydrogen storage is a game-changer, allowing efficient usage of in-vehicle space.
- The novel approach shows great promise to reduce cost and achieve novel geometry to save trunk space.
- The strength of the project is the game-changing approach to store compressed hydrogen with a conformable solution.
- Strengths include the presence of existing technology and know-how to produce the proposed tank.

Project weaknesses:

- The weakness of the project is the lack of understanding regarding industry standards and analysis of failure modes associated with the technology. The project does not have any activities associated with cost analysis of the design. The concept utilizes a dry fiber without resin, which has a benefit for weight and cost but may not be practical over the life of the pressure vessel.
- Limited data are available to date on hydrogen permeability of polymer liner materials. If the permeability is not sufficiently low, the technology will not be useful. Safety—i.e., why leaking hydrogen is acceptable—is not addressed at all. There may be regulatory hurdles that are virtually insurmountable.
- The project needs to fully understand durability requirements and evaluate accordingly. Kevlar strength reduction as a result of vibration-induced abrasion is a known issue. It may be possible to overcome this issue through proper sizing.
- Weaknesses include absence of any preliminary proof of concept and also of a risk management plan to maximize the project's odds of success.

Recommendations for additions/deletions to project scope:

- The recommendations for this project are to revise the permeation criteria, include cost analysis at an early stage, and evaluate potential failure modes in respect to the certification and application. In addition, the project should highlight the benefits of the conformable technology for improvements in volumetric density in a practical vehicle package.

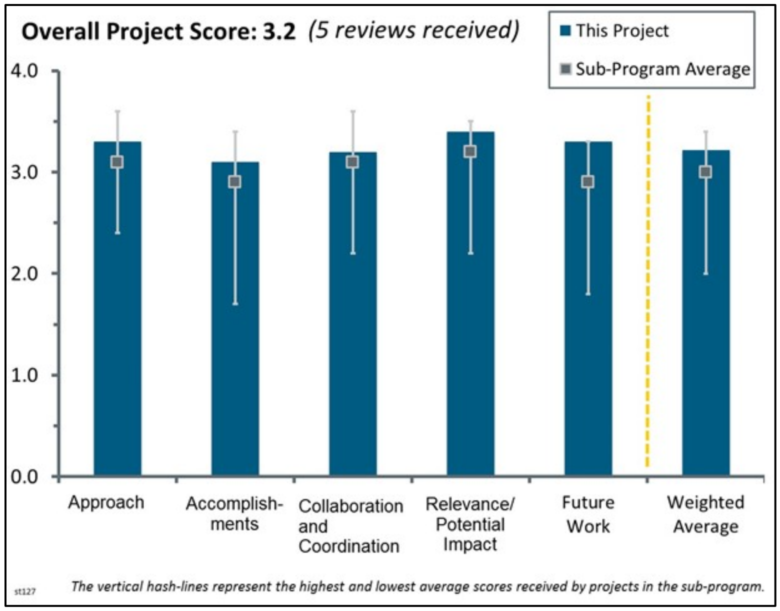
- Given the technical hurdles that may face this project, risk management plans using engineering trade-off solutions are needed in order to evaluate this concept's viability and potential. For example, if the tank wall needs to be thicker, then the project should determine how thick and how this relates to the weight percentage and cost, etc.
- The project needs to fully understand durability requirements and evaluate accordingly. Kevlar strength reduction as a result of vibration-induced abrasion is a known issue. It may be possible to overcome this issue through proper sizing.
- It would be good to see safety and regulatory concerns added to the project scope. There is no path forward to a commercial product if these are not addressed in conjunction with technology demonstration.

Project #ST-127: Hydrogen Materials–Advanced Research Consortium (HyMARC): A Consortium for Advancing Solid-State Hydrogen Storage Materials

Mark Allendorf; Sandia National Laboratories

Brief Summary of Project:

Critical scientific roadblocks must be overcome to accelerate materials discovery for vehicular hydrogen storage. The project objective is to accelerate discovery of breakthrough storage materials by providing capabilities and foundational understanding. Capabilities will include computational models and databases, new characterization tools and methods, and customizable synthetic platforms. Foundational understanding is needed for phenomena governing the thermodynamics and kinetics limiting development of solid-state hydrogen storage materials.



Question 1: Approach to performing the work

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

- This effort focuses on developing computational and experimental methods that enable and accelerate materials discovery. A straightforward and well-thought-out approach has been formulated that starts with validation of models and experimental tools using well-understood systems, followed by a rapid progression to understanding thermodynamics, reaction mechanisms, and structural transformations in more complex materials that show promise of meeting U.S. Department of Energy (DOE) goals. The approach is deliberate and logical. The approach focuses on development of advanced computational models and databases, improved characterization tools, and tailorable synthetic platforms. An important element of the overall approach is ultimately to make these advanced capabilities available to the hydrogen storage research and development (R&D) community. Assuming that a timely progression to more complex materials occurs, the approach should provide a solid foundation for achieving steady and meaningful progress.
- This is an excellent research consortium seeking to address systematically fundamental issues about solid-state storage.
- HyMARC’s approach represents a “reboot” in the thinking about developing viable materials-based storage approaches. As the “traditional” make-it-and-test-it approach has not yet yielded a material that can attain all of the DOE hydrogen storage target properties, HyMARC’s new approach is to develop a computational basis for understanding and developing the enabling foundational science of the key controlling features of hydrogen storage materials. Along the way, new characterization and synthetic tools will be developed to experimentally test some of the key strategies uncovered by modeling. The approach is focused on what many believe are the key barriers: achieving higher heats of sorption in sorbents and achieving vastly improved kinetics of dehydrogenation/rehydrogenation of complex hydrides. Both are problems that have not succumbed to the make-it-and-test-it approach. Developing the enabling methods, and not the materials, represents an innovative, high-risk approach with a large potential benefit if the project succeeds. Where the approach can be improved lies in quickly selecting the “best” prototype systems to answer the key questions, and being prepared to shed materials and redirect efforts that are not in line with achieving HyMARC/DOE goals as quickly as possible. This will largely be a management judgment issue. The choice of working on graphene nanobelts and Lewis acid zeolites was not well supported. It is not clear

how these are leading to useful prototype materials to support the theory, modeling, and characterization focal points.

- The proposed work takes on a "six-task" approach that is logical (and does not appear to miss any major categories). The work is inclusive of all types of hydrogen storage materials: sorbents, hydrides, and liquids. The overall strategy is to begin with materials that are well studied, such as $NaAlH_4$ and MgH_2. Although these materials have been well studied, HyMARC's role is for this team to develop new techniques and approaches. Validation of those techniques using well-studied systems is a logical approach. Two new techniques are low-energy ion scattering (LEIS) surface observation of hydrogen atoms (available only at Sandia National Laboratory [SNL]) and 3 nm ptychography at beamlines. The selection of $Mg(BH_4)_2$ comes from the desire to utilize the Characterization and Validation team to demonstrate the entire team's capabilities. This is a worthwhile objective. There is also a weakness: The proposers intend to examine complex systems such as nanostructured hydrides within graphene nanobelts. The proposers should provide a better explanation for the path of materials selection—why graphene nanobelts are chosen over other forms of graphene.
- It is not clear how data mining as described during the presentation is to benefit solving the storage problem. The following questions arise: what specific information is to be gleaned from this effort, whether nanoscale synthesis is a route that makes sense, and what is to be learned that has ultimate engineering applicability. Nanoscale materials presumably have a large surface energy component, and nature will work to reduce surface free energies via grain or particle growth. While this may contribute to aid in solid-state diffusion, which is the bane of complex hydrides, it seems that an approach that isolates surface facets would offer less ambiguous technical and scientific data. It is not clear how good the computational tools will be in ultimately developing engineering materials. It is not clear that there are any examples of a computational approach predicting the engineering performance of a new material.

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 HyMARC effort is off to a good start. The project comprises six inter-related tasks that comprehensively address the important challenges facing the development of high-capacity hydrogen storage solid-state materials commensurate with practical fuel cell applications. Preliminary data using LEIS (a unique and particularly promising diagnostic tool) and in situ soft x-ray photoelectron spectroscopy (XPS), x-ray absorption spectroscopy (XAS), and x-ray emission spectroscopy (XES) characterization methods are encouraging. Likewise, impressive initial results from the computation and theory effort are providing confidence that a more rigorous and comprehensive understanding of the primary thermodynamic and kinetic effects that control sorption reactions in increasingly complex systems will be forthcoming. Work on size control for complex hydrides and creation of a nanoparticle library for a prototype hydride material is in progress, and that should provide a means for evaluating nanoscale engineering approaches for enhancing sorption kinetics.
- It is likely a little early to have much of a discussion about progress on accomplishments for HyMARC, as the project is still in start-up mode. Access to various characterization tools is being ironed out, theorists are benchmarking computational models, etc., but it is early in the project, and things appear to be going well. The project appears well managed at this point, and the team that has been put together is technically very strong.
- The project has existed for less than one year. In this time, the proposers have achieved the following: two publications, three years of beam time for multiple beamlines, post-doctorate hiring from Princeton, hiring from the Massachusetts Institute of Technology and Dalhousie, and approval of user proposals.
- On slide 13, several different key results are noted. It would be good to know what paper is under review and what the results are that are not part of the manuscript. On slide 14, while hydrogen diffusion has been cited, it seems that diffusion of other species in complex hydrides is the actual problem. The relevance of hydrogen diffusion through alumina that was mentioned in the presentation is unclear. The project has just started, so it is difficult to gauge the utility of the tools that are being developed and how they will be used by the community at large. As the project has just started, accomplishments are limited to hiring people; presentation of actual data was limited.

- The reviewer agrees with the overall approach. Progress is as expected for a consortium that ambitious. The project should probably address more clearly why the materials to model were selected.

Question 3: Collaboration and coordination with other institutions

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

- Effective use of collaboration is evident. The Molecular Foundry and Advanced Light Source (ALS) are being accessed through a user program (with an increased amount of time). Computing at the Molecular Foundry and SNL permits multiscale computational models. Large team interactions are ongoing, with meetings occurring along the lines of tasks (on a regular basis). This is a well-designed construct for interactions. Many team members are co-located in the San Francisco Bay area. Face-to-face meetings can be used to enhance excitement among graduate students and postdocs—the project should take care not to examine a singular problem but to take a broad approach to ensure that the materials problem is being addressed. Regarding weaknesses, it would be useful to have a quantification of meetings; the description "on a regular basis" is too vague.
- Development of a framework that enables and facilitates effective collaboration among the many HyMARC participants is a critical management challenge. A robust organization structure is in place, and thus far, good coordination among activities and researchers in most of the six tasks is evident. Although the work being conducted on the companion consortium led by the National Renewable Energy Laboratory (NREL) directly complements the HyMARC activity, closer coordination between those two efforts will be essential to avoid duplication and to ensure that the "right" problems are being addressed. The challenge will become even more daunting as new participants are added to the HyMARC program. It will be critical for project management to address those challenges in a serious and focused way.
- The success of HyMARC will in large part depend on how well the team members collaborate among themselves and with the hydrogen storage community at large. So far, all appears to be going very well. They have clearly learned a good deal from watching the other Centers and appreciate the "Center Concept," in which collaboration is crucial to success. A key metric for how the collaborations are coming along in the future will be assessing how HyMARC has come together as a team to prioritize efforts and resources to most rapidly answer the key questions, as well as to redirect lagging efforts. It currently appears that appropriate collaborations outside of HyMARC are coming along nicely.
- The list of collaborators is large. The budget is rather limited, and so the degree of commitment by each of the members is unclear. The physical proximity of the laboratories is good, and being in the same time zone is good, but it is not clear how well geographic proximity reduces the activation barrier to communication, especially given the overlap of slides shown by partner laboratories.
- It is difficult to judge at this stage, as the project is new. The project should make sure to coordinate with other ongoing projects.

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.

- This project has the potential to be a "game changer" for DOE. Although prior efforts and activities in individual projects and in the materials centers of excellence provided useful results that had impacts on new materials discovery, that information was largely heuristic and phenomenological. The HyMARC project provides, for the first time, an R&D framework devoted solely to development of experimental and theoretical techniques that can be used to understand the fundamental kinetic processes and thermodynamic properties that enable new materials discovery and development. If successful, this could be a truly impactful project for DOE.
- Developing the foundational science to better understand, for example, the origin of the kinetics barriers in complex hydrides may be key in developing next-generation materials that can meet all of the targets. This is clearly highly relevant to DOE's goals and objectives. If the project succeeds in developing the enabling

science, the multiscale modeling, the characterization tools, etc., that underpin what makes a viable storage material, then this can have a tremendous impact on future hydrogen storage developments.
- Storage is a critical issue. Validating theory and simulation models is necessary at this stage to bridge the perceived gap between experiments and theoretical work.
- The surface science is exceptionally strong and leverages the Basic Energy Sciences program at SNL. Regarding weaknesses, no system should be selected without clearly defining its usefulness in the HyMARC testing and validation schema.
- The overall goal is to help with new principal investigators' projects that emphasize materials development and to help them with analytical/computational tools to which the investigators may not have access.

Question 5: Proposed future work

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

- Future work will initially focus on validation of tools and techniques on systems that are already (fairly) well understood. The intent is to rapidly progress to understanding more complex processes and systems. This overall approach is reasonable and prudent, assuming that the project participants fully understand that a timely transition to more complex systems that could potentially meet DOE goals is essential. It is easy to be lured into detailed studies of interesting processes in "simple" systems that may be largely irrelevant to more complex materials. It seems evident that the HyMARC team understands this distinction and that they will make a straightforward and rapid progression from simple to complex systems as the project evolves in the next year.
- Developing a set of criteria for developing a decision tree or something similar for selection of the optimal prototype materials seems as if it would benefit the library development of prototype materials. This will help the collaboration to also redirect efforts and resources that are not on track to answer the key questions posed to more promising areas that directly support achieving the goals of HyMARC and the technical targets of DOE.
- The database task will begin in year 2. The team has gained ongoing beam time for x-ray studies. This is useful. There are no weaknesses in the proposed future work.
- Perhaps some clarification should be given at the next Annual Merit Review (AMR) about R&D performed on new materials.
- Future work is unclear until new projects are initiated that collaborate with HyMARC.

Project strengths:

- A highly capable team comprising experts in theory/modeling, advanced methods for materials characterization, and materials/structure synthesis have been assembled. Co-location of team resources and facilities should enhance coordination and collaboration in such a large and diverse project. A solid plan is in place to tackle the difficult problems that underlie a detailed understanding of the critical processes operative during sorption reactions in adsorbent media and complex hydride materials.
- The project has strong management and a very strong technical team. The project has a wholly new approach: to circle back to develop the foundational science of storage materials.
- The team is well managed and has quickly organized into sub-teams along the lines of tasks. The team is poised to make advances in discovery for sorbents and hydrides.
- This is an excellent and impressive team.

Project weaknesses:

- It is not apparent that the work of the Lawrence Berkeley National Laboratory (LBNL) team is fully integrated into the overall project. There is concern that the LBNL/ALS group will operate in a semi-autonomous way on materials and advanced diagnostics that may be of special interest to that group but of less relevance to the Office of Energy Efficiency and Renewable Energy. It is incumbent upon the project management to ensure that all efforts are focused on achieving the principal goals of the project and that the overall project is coherent and cohesive. As pointed out at the AMR, it will be important to recognize expeditiously whether information obtained in validation studies on simpler or widely studied systems (e.g., $NaAlH_4$) will be readily extendable to more complex materials (e.g., $Mg(BH_4)_2$). (It should be noted that this comment should be viewed more as a "caution" than a "weakness.") The project must be sufficiently nimble so that mid-course corrections can be made accordingly. There is a notable lack of advanced nuclear magnetic resonance (NMR) and neutron diffraction capabilities. These diagnostics are essential for a project of this kind. Closer collaboration with the NREL-led consortium and "funding opportunity announcement (FOA) projects" is essential.

- The choice of materials selection should be better explained. With the onboarding of materials system projects, it is important to classify the materials system in relation to HyMARC goals (i.e., some materials are selected because they make a unique testing platform, and others are selected for development as hydrogen storage systems).
- There is a very minor weakness: a justification of the methodologies used or proposed would be appreciated.
- The choice of prototype materials not well developed and is disjointed.

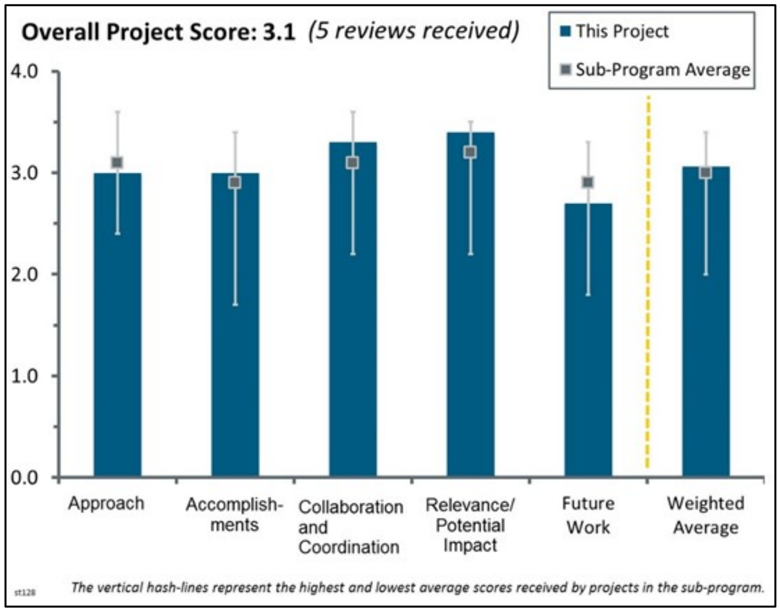
Recommendations for additions/deletions to project scope:

- It may be helpful to bring investigator(s) with more extensive synthetic and mechanistic chemistry expertise into the project. Although an impressive array of theory, modeling, characterization, surface science, and materials development capabilities exists in the project as currently constituted, the involvement of an individual/individuals having deeper "chemical intuition" could be beneficial in making the difficult decisions concerning evolving research directions (especially concerning kinetics and reversibility issues) in the project. Advanced NMR and other "H-centric" diagnostic capabilities, such as neutron diffraction (the National Institute of Standards and Technology), and VISION (Oak Ridge National Laboratory), are needed. This may involve a much closer connection with the NREL consortium and/or the addition of complementary DOE-funded projects that augment the HyMARC effort.
- The explanation of the HyMARC collaborative must be re-stated clearly for the hydrogen storage community to fully understand its role in developing testing and validation platforms (i.e., developing the toolset to improve materials discovery). Since some materials systems must be selected and used for developing test and validation platforms, it is recommended that the team identify the test/validation platform demonstrated by each materials system selected for future review cycles. No system should be selected without clearly defining its usefulness in the testing schema.
- It is unclear how graphene nanobelts and functionalized or otherwise zeolites are going to help HyMARC develop the materials systems that will assist the development of the foundational science of targeted sorbents and complex metal hydrides. It is recommended that HyMARC develop a defensible prototype materials strategy that the storage community at large can accept. That community is presumed to be the future customer of HyMARC output.

Project #ST-128: HyMARC: Sandia National Laboratories Effort

Mark Allendorf; Sandia National Laboratories

Brief Summary of Project:



This project addresses a lack of knowledge about hydrogen physisorption and chemisorption. Researchers will develop foundational understanding of phenomena governing the thermodynamics and kinetics of hydrogen release and uptake in all classes of hydrogen storage materials. Sandia National Laboratories (SNL) will (1) provide data required to develop and validate thermodynamic models of sorbents and metal hydrides, (2) identify the structure, composition, and reactivity of gas–surface and solid–solid hydride surfaces contributing to rate-limiting desorption and uptake, (3) synthesize metal hydrides and sorbents in a variety of formats and develop in situ techniques for their characterization, and (4) apply multiscale codes to discover new materials and new mechanisms of storing hydrogen.

Question 1: Approach to performing the work

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

- This investigation provides details of the SNL specifics with regard to computation (molecular dynamics [MD], density functional theory, and database), experimentation (metal–organic frameworks [MOFs], nano and bulk metal hydrides, and high-pressure synthesis), and characterization (gas sorption, soft x-ray synchrotron, in situ x-ray diffraction, Fourier transfer infrared spectroscopy, and x-ray photoelectron spectroscopy [XPS]). This broad set of experiments will benefit the Hydrogen Materials–Advanced Research Consortium (HyMARC) and those who provide materials for evaluation and validation.
- This project is a critical element of the experimental effort in the HyMARC project. The approach is very expansive—it addresses important issues relevant to hydrogen storage in adsorbents (mainly MOFs and doped carbons) and a wide range of bulk and nanoscale metal hydrides with catalytic additives over an extended pressure range. A suite of characterization instrumentation, mainly surface-sensitive probes, is being employed to study hydrogen sorption reactions at surfaces. The focus is on validation of the approach using well-understood systems, followed by a rapid progression to more complex systems. Although the approach is comprehensive, it seems unlikely that all of the proposed work can be accomplished with the funds and resources allocated for this project. The research and development (R&D) team is strongly encouraged to prioritize the efforts in all of the tasks and to address only those topics that have the most impact for the overall U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program). Without this information, it will be difficult to conduct an insightful review of the project approach as it evolves.
- The HyMARC team (and SNL as team lead) has a difficult balance to navigate. The most significant benefit the team brings to the Hydrogen Storage program is the ability to examine the issues that are holding back hydrogen storage, using a complementary suite of techniques and expertise and with a significant amount of effort. Although not a materials development project, HyMARC, of course, must choose materials to examine. The team also has the opportunity to bring new experimental techniques to bear in an effort to shed a new light on problems. The challenge is to keep the primary target in sight and not be sidetracked by materials or technique development unless these are firmly required to understand

issues that will benefit the wider Hydrogen Storage program. The approach in general has been constructed in a reasonable manner; however, some of the experimental techniques give the impression of being utilized more because they are new and available than because they are the best for the purpose. The team needs to keep a watch on this.
- The assortment of state-of-the-art theory, synthesis, and characterization capabilities being developed at SNL for HyMARC clearly has potential to be of value in the development of high-performance, high-capacity hydrogen storage materials. The exception to this is the Li_3N nanoconfinement work, which is something of a recycle of work that has been done elsewhere and seems highly out of place in HyMARC.
- The responsibilities within HyMARC, in terms of validating the modeling activities to provide support with materials preparation, are useful to the overall consortium activities. However, these studies will use only known, although not well-understood, select material systems, all of which are incapable of meeting the DOE targets. The strategy of this project toward meeting the DOE targets ideally needs to go beyond just understand existing materials, such as sodium alanate or magnesium borohydride, to inspiring or driving novel approaches for materials-based storage.

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 research has made developments in solid–solid phase transitions, paying particular attention to minor phases, byproducts, etc., in order to understand and tune reaction pathways. The development of a high-pressure system at SNL is underway to reach 1000 bar. SNL also has a low-energy ion scattering (LEIS) system.
- The upgrading of the high-pressure station and production of the clean transfer holders are excellent, technical, and very important steps forward toward achieving the level of technical excellence envisioned for HyMARC. The establishment that LEIS can be used to study surface hydrogen diffusion is also an important accomplishment. However, the studies—LEIS, XPS, MD calculations, and catalytic additive studies on Ti-doped $NaAlH_4$—seem only to add to the controversy about the fundamental processes occurring in this system and are tangential to the goals of HyMARC. Furthermore, it is surprising that there is no awareness that a large number of studies of Ti-doped $NaAlH_4$ by soft x-ray techniques were reported in the literature many years ago.
- Initial results have been obtained on all five tasks in the project. The work on tracking the surface composition of Ti-doped $NaAlH_4$ during hydrogen desorption is interesting and potentially useful. Moreover, it can be used to validate the utility of the unique LEIS capability at SNL for monitoring surface reactions. However, it will be critical for the R&D team to use that information as a foundation and to rapidly progress to metal hydride systems with volumetric and gravimetric capacities that meet current DOE goals. The ability to explore processes at higher pressures using the upgraded SNL reactor should provide important new information that can be used to guide future work. The relevance of the use of MD simulations to predict H diffusion barriers in Al is puzzling. The relevance of the effort was not clearly motivated in the presentation. Nanoscale effects and nano-interface engineering may prove to be invaluable for overcoming kinetics limitations. Initial results are promising. However, concerns remain about clustering and agglomeration that might ultimately limit reversibility. The effect of surface oxidation on hydrogen sorption reactions was recognized and is being investigated. This may be a critical component of the overall effort. However, surface preparation in an ultra-high vacuum (UHV) environment may be needed to fully assess the role of oxidation. Finally, the MOF studies are important to developing models for adsorption. The participants are strongly encouraged to actively collaborate with researchers in the National Renewable Energy Laboratory (NREL)-led consortium to avoid unnecessary duplication and overlap of efforts.
- These are the early days for the project, so it is difficult to assess, although progress appears good. As has been discussed at meetings with these researchers before, the interpretation of the $Li_3N/LiNH_2$ results may not be “fundamentally altering the reaction path” as suggested. Rather, the importance of surface energies in nanostructured materials means that thermodynamics and, in particular, phase nucleation are altered, which affects the kinetics of the reaction path so that intermediates may not be observed as discrete phases. The team should consider this as it seeks to generalize its findings to other systems.

- The consortium is only few months old, so the project has not made much progress.

Question 3: Collaboration and coordination with other institutions

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

- A large number of external collaborators are involved in the research. The collaborations will strengthen this HyMARC project by providing unique characterization tools and/or insight into relevant material systems.
- SNL and much of the HyMARC team have reached out to other groups to communicate and exchange samples, and early signs are positive with respect to collaboration.
- An extensive network of excellent collaborators has been established. The collaborations need to be better coordinated to enhance productivity and to keep these efforts focused on attaining the overreaching, foundational understanding of hydrogen absorbing materials that is the goal of HyMARC.
- Collaboration with several other institutes was mentioned; however, the direct impact on the current status is not very clear.
- Although solid collaborations with other HyMARC partners are evident, coordination of the work on this project with the Lawrence Berkeley National Laboratory/Advanced Light Source (ALS) HyMARC activities has not been clearly articulated. Close collaboration with the NREL-led consortium will be needed as the project evolves. This will be especially important for the MOFs/adsorbent work. The principal focus of the diagnostic effort in this project is on surface characterization. However, it seems that a more robust activity employing other "H-centric" diagnostic capabilities such as advanced (magic angle spinning) nuclear magnetic resonance (NMR), neutron diffraction and vibrational spectroscopy (e.g., the VISION spectrometer at Oak Ridge National Laboratory [ORNL]) will be needed as the project progresses. Although some of those external collaborations were mentioned (slide 21), the connections with the technical tasks in this project were not described.

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 strongly supports Program goals by attempting to identify key factors that may overcome barriers to meeting the storage targets. The potential for progress will become more apparent as the project matures and interacts with other independent projects.
- As a primary component of the HyMARC activity, this project is critical to advancing our knowledge about hydrogen storage in relevant materials. It directly supports Program goals.
- The effort to develop hydrogen storage materials that meet DOE targets has long been in need of improved fundamental understanding of promising new systems. The capabilities being developed by SNL have potential to provide the requisite insights into these systems.
- The work is highly relevant. Particularly, the clean transfer chamber and LEIS system being developed will have a broad impact on hydrogen storage research.
- The ability to experimentally understand fundamental phenomena in select materials and use modeling to predict the performance is useful to the science of hydrogen storage materials. However, it is difficult to envision the impact of these findings on helping meet DOE targets because these findings are likely unique to only these material systems.

Question 5: Proposed future work

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

- The development of an anaerobic interchange is suggested and is a strength. This development will lead to new tools designed for working with individual projects and instruments. There does not seem to be a plan in place to broaden the anaerobic interchange chamber architecture to the wide user community (which may pursue measurements at laboratories other than the ALS and the LEIS system).
- While the plan that relates to the selected materials is clear, the timeframe to achieving thorough experimental understanding of the behavior to precise modeling of these seems to be difficult to meet. In addition, a strategy toward translating these findings and this modeling to other material systems is necessary and is missing.
- Proposed future work is essentially a continuation of current work. There do not appear to be decision points or mitigation strategies in the material presented.
- The future work was addressed only in the most cursory way (slide 18). Consequently, it is difficult to assess the extent to which research priorities and future directions have been established for the project. Because this project encompasses such a broad array of R&D topics and issues, a more complete and compelling description of future work is needed.
- There was nothing about the future research plans in the presentation, and only a few vague remarks were made when the presenter was asked about this during questioning.

Project strengths:

- A strong and exceptionally capable R&D team is conducting the R&D work on this project. Unique and powerful synthesis capabilities (e.g., advanced high-pressure reactor) and surface diagnostic instrumentation (e.g., LEIS) are being used to address critical issues. Solid collaborations within HyMARC and externally are evident. The project is well managed and coordinated.
- The strong team of researchers at SNL has the ability to identify and develop overreaching capabilities for the development of all classes of hydrogen storage materials that were envisioned for HyMARC, such as the upgraded high-pressure facility and the development of the LEIS method for the study of hydrogen diffusion on surfaces.
- The team is competent with a wide range of experimental and computational tools.
- Some focus on foundational aspects of hydrogen interaction with materials that could explain limitations of current storage materials and offer pathways for improvement is a project strength.
- A broad set of tools will benefit HyMARC.

Project weaknesses:

- Some work seems disconnected; for example, the nanostructure investigation remains focused on Li_3N-based materials, while much of the other work is examining $NaAlH_4$. While there is a need to discover general principles, there is also value in characterizing a chosen system from the different aspects being considered.
- Broadened plans are needed, particularly where it pertains to the interchange system being developed. It is not clear whether researchers will have the opportunity to access the design plans in order to modify their instrument. If so, it is not clear how this exchange of information will be managed.
- Although it is necessary to benchmark new equipment and techniques, long-term studies of the old materials should not become all-consuming.
- Based on the project's approach, understanding of the selected known systems does not necessarily extrapolate to other materials. To meet the DOE targets, new ideas and material systems beyond those existing are needed.
- The project has an extremely broad scope, covering a variety of material classes, reaction pressures, structures and sizes, and characterization modalities. Without a more careful prioritization of effort, it is not clear whether real impact on any single topic will be forthcoming. The team is strongly encouraged to thoughtfully evaluate priorities, decide where rapid progression from a simple to more complex system(s) is needed, and look for gaps that can be filled by external collaborations. That information should be

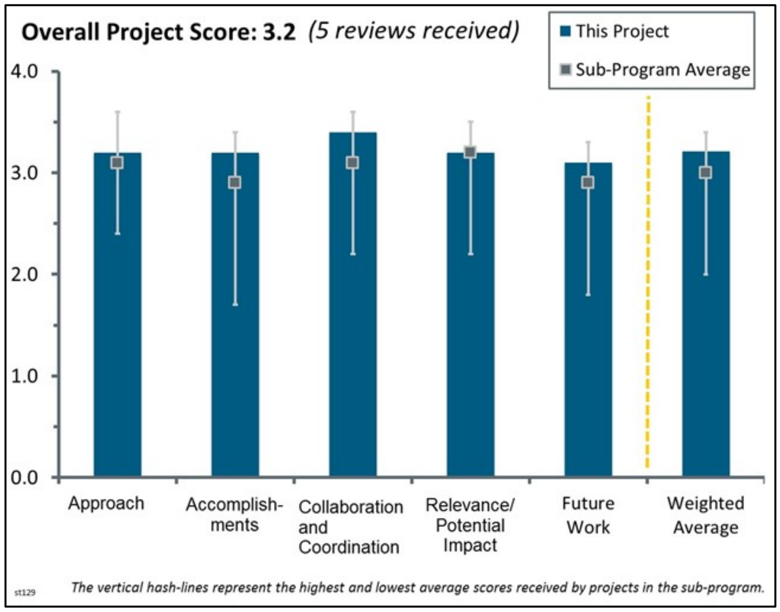
communicated to DOE project management in a timely way so that adjustments in scope can be implemented as needed.

Recommendations for additions/deletions to project scope:

- More robust NMR and neutron diffraction capabilities are clearly needed in the project. Hopefully, funds are available to support collaborations with Pacific Northwest National Laboratory and the National Institute of Standards and Technology so that these tools can be readily utilized. Also, vibrational spectroscopy using neutrons rather than photons (VISION spectrometer at ORNL) could greatly complement the information gained from use of the existing techniques. The role of surface oxidation could be important/dominant in controlling the kinetics of hydrogen sorption reactions. That is well recognized by the project team, and some initial plans are in place to study the effect. However, it does not seem that the stated approach will address the critical issues. The tools that are available to the SNL team are most impressive. If they could be coupled with a UHV reactor with surface-cleaning (e.g., ion sputtering) and oxygen-dosing capabilities, a truly definitive study could be conducted. This is a difficult endeavor that could be an unwanted diversion; however, its importance cannot be overstated. If preliminary work already planned produces results that warrant a more complete study, the team should explore the "UHV option" with DOE management.
- The explanation of HyMARC must be restated clearly for the hydrogen storage community to fully understand its role in developing testing and validation platforms (i.e., developing the toolset to improve materials discovery). Because some materials systems must be selected and used for developing test and validation platforms, the team should identify the test/validation platform demonstrated by each material system selected for future review cycles. No system should be selected without clearly defining its usefulness in the testing schema.
- It would be useful to define material systems/stages in this project with a clear added benefit and milestone for each stage. It is recommended that the team define a strategy for this project and give a clear idea of where it would be heading and how it could help meet the DOE targets.
- It is difficult to make recommendations at this early stage of a far-reaching project, but a greater degree of focus on a critical process among the HyMARC partners could be beneficial.
- The project should suspend all studies of Li_3N and Ti-doped $NaALH_4$.

Project #ST-129: HyMARC: Lawrence Livermore National Laboratory Effort

Brandon Wood; Lawrence Livermore National Laboratory



Brief Summary of Project:

The Hydrogen Materials–Advanced Research Consortium (HyMARC) is providing community tools and foundational understanding of phenomena governing thermodynamics and kinetics to enable development of solid-phase hydrogen storage materials. HyMARC team member Lawrence Livermore National Laboratory (LLNL) is conducting porous carbon synthesis; x-ray absorption/emission; and multiscale modeling including density functional theory (DFT), ab initio molecular dynamics (AIMD), phase-field mesoscale kinetic modeling, and kinetic and quantum Monte Carlo (QMC).

Question 1: Approach to performing the work

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

- This project provides a very wide variety of basic science services to the recently established HyMARC. These include theory and simulation, synthesis, and in situ simulation (with many subcategories). Most of the H-storage materials were at least experimentally studied in the past, and the idea is to provide much-needed fundamental understanding of these materials, as well as establish procedures for future materials development. As such, this effort is very important to the overall U.S. Department of Energy (DOE) Hydrogen Storage Materials effort.
- The assortment of theory, synthesis, and characterization capabilities that are being developed at LLNL for the HyMARC center clearly have potential to be of value in the development of high-performance, high-capacity hydrogen storage materials.
- This is an excellent initiative, an impressive team with impressive resources. The project should detail how and why the materials investigated were selected for modeling.
- This task is led by Brandon Wood, who is primarily a computational scientist at LLNL. The proposed work uses QMC—extended crystal examining non-local chemical effects (with the help of supercomputers). These studies will permit benchmarking the various DFTs and new models for charge and field effects on physisorption—physics of sorption is being developed for graphene oxide as sorbent materials. The team will use the partnership with the National Renewable Energy Laboratory-led Characterization and Validation team to validate this model experimentally using a platform that can be synthesized. This will provide direction of interaction—theory leading experiment or experiment leading theory.
- The project is conducting a number of detailed computational studies. Given the breadth of the effort, it is unclear which specific questions are being answered or what a successful outcome would look like. Overall, the project more resembles a Basic Energy Sciences (BES)-type effort, rather than an Office of Energy Efficiency and Renewable Energy-themed study. Specific examples of how the computational methods will be validated were not very clear. This is especially true for the phase fraction predictions. It is not clear that transmission electron microscopy (TEM) has been done to assess morphology changes of the different phases vs. time/hydrogen content.

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 in the synthesis of advanced porous carbons and in the development of advanced protocols to allow more accurate computation of hydride thermodynamic properties by DFT. The reviewer does not feel qualified to comment on the progress that has been made in the sorbent modeling studies, but the QMC computations seem to provide key insight into the development of physisorbents. However, the value of studies on Li_3N and PdH_x to the development of more promising systems is not clear, and the AIMD studies seem to be yielding highly questionable results. It is surprising that there is no awareness that similar studies of Ti-doped $NaAlH_4$ using soft x-rays reaching similar conclusions were reported in the literature many years ago.
- In the brief time the project has been in existence, several basic techniques have been explored and partially developed for widespread future use within HyMARC. It is too early to judge how this progress will affect progress toward DOE numerical goals. Certainly, such fundamental calculation experiment techniques will help the whole march toward the DOE goals. It is not completely clear why the particular materials for study were selected. Some are old and well established (e.g., PdH_x, $TiCl_3$-doped $NaAlH_4$, Li-N-H, etc.). Apparently, the idea is to use these as model materials to establish basic techniques.
- The proposers have developed a platform to examine the role of crystallinity (the Li-N-H nanoconfined system) and explain formation of Li_2NH (energy penalty too high for its formation in the nanophase), with a manuscript submitted. The proposers are now also doing this for the Mg-B-H system. The team has developed a statistical approach to defects: classes of defects in various fractions are used along with the likelihood of a reaction occurring. The example of the defect model given seems disconnected with the statement because the example examines nanometer size as a function of defect formation. It would be good to see more a detailed description of the defect model itself.
- Progress is good. Providing more details about the choice of materials would be nice.
- This is a new project; progress to date has been satisfactory. Many of the project components have kicked off.

Question 3: Collaboration and coordination with other institutions

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

- There are extensive collaborations, between not only the two main participants in this project (LLNL and Sandia National Laboratories [SNL]) but also other members of HyMARC. There are many faces new to the H-storage field.
- The team collaborates with the other HyMARC partners, particularly with SNL on ion-scattering (i.e., concentration gradients at interface). The team is developing collaborations with the Characterization and Validation team, particularly along the lines of examining borohydride chemistry with NMR.
- An extensive network of excellent collaborators has been established. The collaborations need to be better coordinated to enhance productivity and to keep these efforts focused on the attaining the overreaching, foundational understanding of hydrogen absorbing materials that is the goal of HyMARC.
- The project should improve or establish a relationship with Caltech work for graphene-related materials to optimize efforts.

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 modeling work is extremely important to HyMARC. Suitable progress on relevant systems is being made. Among other useful developments, the proposers are developing a part of the library system that will

include a library of nucleation barriers in the various hydride systems. If developed, this will be extremely useful to the field.
- The effort to develop hydrogen storage materials that meet DOE targets has long been in need of improved fundamental understanding of promising new systems. The capabilities being developed by LLNL have potential to provide the requisite insights into these systems.
- The project is highly relevant and could have an impact beyond hydrogen storage research and development.
- This project will not immediately address numerical Multi-Year Research, Development, and Demonstration Plan storage targets, but rather support techniques and basic science understanding for future materials development. In that sense, it is directly focused on the Hydrogen and Fuel Cells Program "chemisorption and physisorption understanding" goal and objective.
- The project may lead to improved understanding of some hydrogen storage materials. However, it is unclear whether this knowledge will be of much value in the development of improved storage materials. In other words, it is not obvious that this team is "asking the right questions."

Question 5: Proposed future work

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

- The project should emphasize new materials studies in the next presentation and justify the choices. The reviewer fully supports the choice of examining well-understood materials at this stage for model validation.
- One of the most important tasks identified in the future work is to develop an automated molecular dynamics set-up code (to speed up handling of projects that are coming online to the HyMARC computational team).
- The list of future work (slides 19–20) seems reasonable, but it is not clear why these particular tasks are the most important.
- The results of the preliminary AIMD studies should be critically evaluated before the project continues to build on what seems to be a shaky foundation.

Project strengths:

- This is an ambitious project that aims to address several critical issues in modeling and validating the properties of sorbents for hydrogen storage. This is an excellent team with excellent resources.
- The project provides much-needed basic science and experimental procedures for the general understanding of hydrogen storage materials: theory, specialized synthesis, and in situ measurement. New scientists are being added to the H-storage field.
- There are many unique aspects of the modeling proposed. The defect model is one that will be extremely useful once developed.
- (1) The efforts for the synthesis of the advanced porous carbons and computation of hydride thermodynamic properties by DFT and (2) the QMC calculations on sorbent charge effects seem to be the strengths of LLNL's effort within HyMARC.

Project weaknesses:

- There are no major weaknesses. Justification of the choice of simulation tools and modeling approaches would have been appreciated.
- The proposers must develop and define a well-communicated platform for these "theory-leading-experiment or experiment-leading-theory" interactions.
- It is difficult to understand why the specific materials and techniques are the most important to study at this time.
- The AIMD studies are giving questionable results. Soft x-ray studies have not provided any new insights.

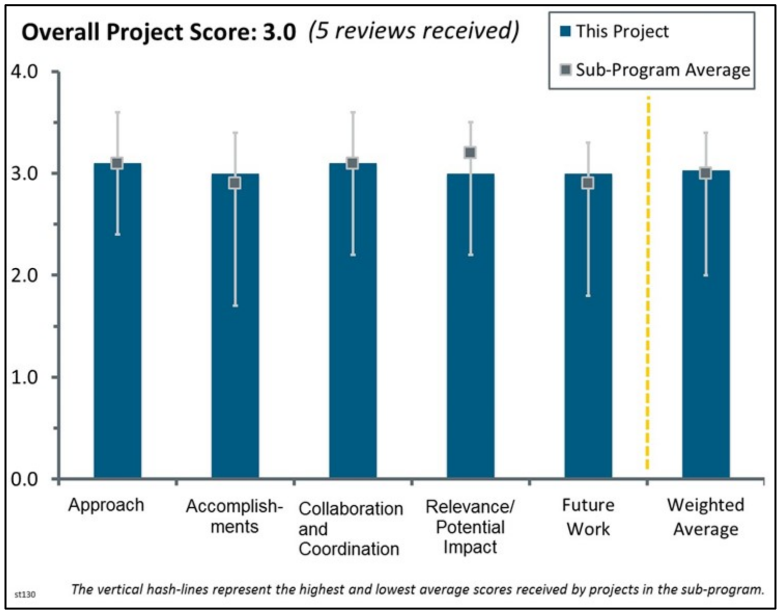
Recommendations for additions/deletions to project scope:

- Some level of work is recommended on comparing various levels of complexity of interaction potentials and how well they perform to predict high-density uptake. It was not clear whether very detailed interaction potentials are really needed for adsorption uptake calculation at high pressure/low temperature, especially with high throughput. The project should use a semi-classical Feynman-corrected potential to perform QMC calculations; it is not clear whether a full-path integral approach is needed, and under which circumstances. A “best practices” document for simulations would be a nice outcome of this project.
- The explanation of HyMARC must be restated clearly for the hydrogen storage community to fully understand its role in developing testing and validation platforms (i.e., developing the toolset to improve materials discovery). Since some materials systems must be selected and used for developing test and validation platforms, it is recommended that the team identify the test/validation platform demonstrated by each material system selected for future review cycles. No system should be selected without clearly defining its usefulness in the testing schema.

Project #ST-130: HyMARC: Lawrence Berkeley National Laboratory Effort

Jeffrey Urban; Lawrence Berkeley National Laboratory

Brief Summary of Project:



The Hydrogen Materials–Advanced Research Consortium (HyMARC) is providing community tools and foundational understanding of phenomena governing thermodynamics and kinetics to enable development of solid-phase hydrogen storage materials. Lawrence Berkeley National Laboratory (LBNL) will (1) focus on light materials and synthesis strategies with fine control of nanoscale dimensions to meet weight and volume requirements; (2) design interfaces with chemical specificity for control of hydrogen storage/sorption and selective transport; (3) explore storage concepts; (4) develop in situ/operando soft x-ray characterization capabilities in combination with first-principles simulations to extract details of functional materials and interfaces; and (5) refine chemical synthesis strategies based on atomic-/molecular-scale insight from characterization/theory.

Question 1: Approach to performing the work

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

- This work emphasizes interconnection between theory, synthesis, and in situ characterization within the LBNL efforts. The strengths include the primary focus on computation, spectroscopy, and databases. All of these are important and noteworthy efforts individually, but combined, these efforts are poised to have an impact on storage systems. The team chooses to focus on (1) nanoencapsulation (Jeff Urban) and (2) multiple characterizations at the user facility at LBNL and ion scattering.
- Like ST-129, this LBNL project provides a very wide variety of basic services and materials studies to the recently established HyMARC. These include theory and simulation, synthesis, and in situ simulation (with many subcategories). This LBNL effort concentrates on the thermodynamics and kinetics of nanoscale, lightweight materials (especially Mg), and basic scientific synthesis and characterizations of various such materials. As such, this effort is also important to the overall Hydrogen Storage program. It is based more on understanding theory, techniques, and characterization than on achieving U.S. Department of Energy numerical goals within its three-year life.
- This is an important initiative. The scope seemed to overlap the work done with the other simulation team (ST-129). It would be better to clarify or differentiate the objectives of this group and explain them within the context of the overall project.
- It is difficult to judge at this early stage and with HyMARC investigating concepts and methods that address the barriers. The project does show some integration with other efforts, although some (e.g., encapsulation) appear standalone at this stage.
- There appears to be an emphasis on encapsulation of Mg. In the presentation on HyMARC by Sandia National Laboratories (SNL), it was stated that HyMARC was not established to engage in materials development; this effort appears inconsistent with that pronouncement. The meaning of the first project objective on slide 5 related to “synthesis strategies with fine control of nanoscale dimensions to meet weight and volume requirements” is unclear. It is not clear why nanoscale dimensions will help meet weight and volume requirements. The physical principle behind this focus is not clear, and it is not clear

why there is an emphasis on encapsulated Mg. At the size dimensions given in the presentation of 3.5 nm, no thermodynamic change in desorption is anticipated to take place. Given differences in the nature of faceting, surface free energy and interfacial energies between this material and one that is actually suitable from a thermodynamic standpoint, whatever information that is gleaned by looking at encapsulated Mg will likely not be readily transferable to materials of actual relevance. Given that McPhy Energy already produces a commercial system based on Mg with carbon additions, this particular objective should be redirected. Also, the goal of measuring kinetics of this system is not relevant given the thermodynamic barriers, and kinetics would nonetheless be expected to be better in any system in which the diffusion distances are small. As was pointed out by one of the reviewers, hydrogen uptake in Mg has been studied extensively for the past 40 years. The encapsulation effort described here is of questionable relevance to this program. The goal of the zeolite and mesoporous SiO_2 system is also unclear. One generally knows that localized charge effects will influence adsorption. It is not clear whether the work on oxides led to schemes that are relevant to developing a material with an already known strategy to improve the adsorption process. Oxides are relatively heavy, and given the ionic or covalent nature of bonding in these materials, if the goals of this effort are met, it is not clear what strategy is in place to apply this to a material system of relevance. Finally, the goal of the computational exploration of metal–organic framework (MOF) isotherms is unclear. It is not clear what computational approaches are to be used, e.g., density functional theory or path integral methods, or why this computational effort is directed in a way that has not already been pursued by the group at Northwestern.

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 are several strengths. The team has worked on solid–solid interfaces, functionalizing interfaces to promote desorption or use interfaces as barriers or membranes to prevent interaction with environment. The team is working to develop a soft x-ray hydrogen storage user community to develop from this effort at the Advanced Light Source. The x-ray absorption spectroscopy being done examines sample preparation for new materials at the beamline. These techniques will also be developed for transfer of samples for ptychography and other measurements. The team has already located discrepancies for known materials and made efforts to reconcile theory versus experiment. The weaknesses are that the scanning tunneling microscopy (STM) is only 20% complete, and no explanation is given for why this number is so low.
- In the brief time the project has been in existence, considerable work has been done according to the work plan and substantial data developed for widespread future use within HyMARC. A significant fraction of the effort has concentrated on encapsulated nano-Mg (Tasks 1, 3, and 5). Given the largely unsuccessful efforts to change the thermodynamics of Mg over several decades (crystalline, nanocrystalline amorphous, and catalyzed), the present results are not convincing that this effort is going to make much practical advancement toward DOE goals for light vehicles.
- There have been some interesting results, although performance is short of DOE metrics (rates for Mg, capacity for sorbents), with no clear strategy outlined for discovery of what factors are critical for the storage community to make progress. It is difficult to assess at this early stage.
- It is too early to judge. The progress is satisfactory at this stage (six months).
- Slide 10 shows no temperature data. The kinetics of uptake are relatively slow under whatever conditions the figure in the upper right is meant to describe. The achievement lists graphene oxide and reduced graphene oxide (rGO). It is not clear which it is. It is not clear whether enough is known about the structure of either to match to the Task 1 milestone to computational studies. While some differences in activation energies were presented, no desorption data were presented at 3.5 nm dimensions, and the thermodynamics of Mg desorption do not seem to be altered, as noted by prior work in this area that spans back 40 years. Slide 12 shows results from the graphene nanoribbon (GNR) functionalization. Not much difference is seen as a result of this functionalization. There is a depiction of the atom placement and of the repeat structure, but it is not clear how these were determined. Again, for this system, if the desorption thermodynamics have not been altered from their typical 76 kJ/mol value, it is not clear what relevance this correlation has. There was a suggestion that 95% of the density of bulk Mg uptake was achievable and that the encapsulation did not contribute substantially to loss of volumetric density. At the same time, it was suggested that the Mg particles made here were monodisperse at 3.5 nm. If these particles are spheroids, a

perfect packing density of material closer to 74% would have been expected. Slide 14 shows isotherms from materials that have presumably been modified. The assertion on the slide that a positive slope at low pressure may show an increased capacity can be extracted from the surface area data and will not go beyond one weight percent excess for these materials. What appear to be more gradual, low-pressure slopes for MCF17 and Al-MCF indicate that the Henry's law value for these materials is in fact small and the results of the modification have done nothing to improve the adsorption enthalpy over that of ZSM-5. The literature is full of data on absorption by materials of this type, and the investigators should see what has been done previously.

Question 3: Collaboration and coordination with other institutions

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

- There are excellent collaborations between LBNL and many members of HyMARC and a few external organizations. New scientists have been added to hydrogen storage research and development.
- Project results presented appear largely self-contained so far. Evidence of interaction with external partners (e.g., South Africa) appears stronger. This should improve as x-ray absorption near edge structure (XANES) modeling capabilities extend across HyMARC and hydrogen uptake measurements with SNL continue or expand.
- The strengths are that the team has developed collaboration with the University of Cape Town, South Africa. The team will use existing collaborations with the Long group and with the Energy Frontier Research Center "on campus" to extend this work. The weakness is that it remains unclear how the team will interact with the Characterization and Validation team partners.
- It is difficult to judge at this stage; the project seemed to be well coordinated, though.
- The emphasis with other institute collaboration appears to be related to the nanoribbon work. This is of limited value in connection with an effort using Mg. Randy Snurr at Northwestern has already done an extensive computational screening of MOFs. It is not clear what is to be gained through this 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 **3.0** for its relevance/potential impact.

- This project is focused on advanced theory, synthesis, measurement techniques, and advanced science, and it is well oriented toward those aspects of the DOE Hydrogen and Fuel Cells Program (the Program) goals and objectives.
- HyMARC has been designed to advance progress in the Program by understanding phenomena inhibiting development of materials, meeting targets, and by developing tools and methods to investigate new candidate materials. The project supports these goals, and future progress may provide impact toward this.
- Developments of nanoconfined high systems have been made, which is a strength. Following the Nature Communications work by Cho, Ruminski, Aloni, Liu, and Guo for graphene oxides, which was done in nanoconfinement to 3.5 nm in size and was looking for pressure effects at 200°C to 300°C, Mg-GNR is the next system that may be used to effectively encapsulate. Another unique idea is the field approach, i.e., using silica support that has local charge with Lewis acid and Bronsted acid sites. This is unique and broadly applicable to many materials. The question about scale of the investigated systems remains a weakness. It is not clear whether these materials are so esoteric that they will not drive the development goal. The leadership should identify broadly applicable aspects of the "boutique" materials for furthering the goal of HyMARC.
- It would be interesting to address the justification behind the choice of materials (model validation, relevance to automotive applications, etc.) during the presentation.
- It is not clear how the studies performed here will lead to the design or improvement of materials that have not already been established empirically or theoretically. Some of the analytical tools are potentially useful if they, in fact, offer insight on hydrogenated materials/surfaces.

Question 5: Proposed future work

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

- The project should continue as planned. The proposed future work should help clear up some old mysteries and provide some more firm experimental techniques for use in HyMARC and other DOE projects.
- Some of the proposed work is a generic continuation of current studies. The nanoscale $Mg(BH_4)_2$ proposed work may integrate well with other projects (ST-129); interpretation of metal hydride spectroscopy and interfacial electronics may provide valuable understanding of reaction mechanisms and kinetics to guide progress in materials design.
- A series of proposed experiments to further validate and test systems is suggested. Although computational work is suggested for XANES data validation, it would be useful to see computational work described for other areas of future study.
- The work described on Mg will be of no value to the Program. The work on borohydrides is of interest. The effort on plasmonic studies is unclear. It is not clear what energy inputs are required to initiate plasmons and why this is of interest.

Project strengths:

- The project will improve our fundamental knowledge of a variety of solid-state hydrogen storage materials and will help add new scientists to the hydrogen storage field.
- The rigorous approach to model validation is a project strength.
- Unique experimental approaches are being taken. The materials systems under investigation are relevant.
- The project has some interesting materials properties and has made progress in understanding of x-ray spectroscopy.

Project weaknesses:

- There is possible overlap with ST-129 on C-coating of hydrogen storage materials.
- The team has yet to define specifics with respect to broader goals—such as how it will interact with the Characterization and Validation team.
- It would be important to further clarify the scope of the project within the context of the overall initiative. The project should justify more clearly the selection of materials.
- The project appears too defocused for a relatively small project and has too many thrusts. Much of it seems directed at materials development (e.g., encapsulated materials and aluminosilicates) with less emphasis on an understanding that will provide a foundation for new materials across the Program. The transition metal doping of Mg could provide new knowledge, but the encapsulation with rGO confounds interpretation, and future plans appear to have shifted to alanates. It appears that HyMARC could be better focused and coordinated to address materials bottlenecks in a new way. The somewhat scattered nature of this project is part of that impression.

Recommendations for additions/deletions to project scope:

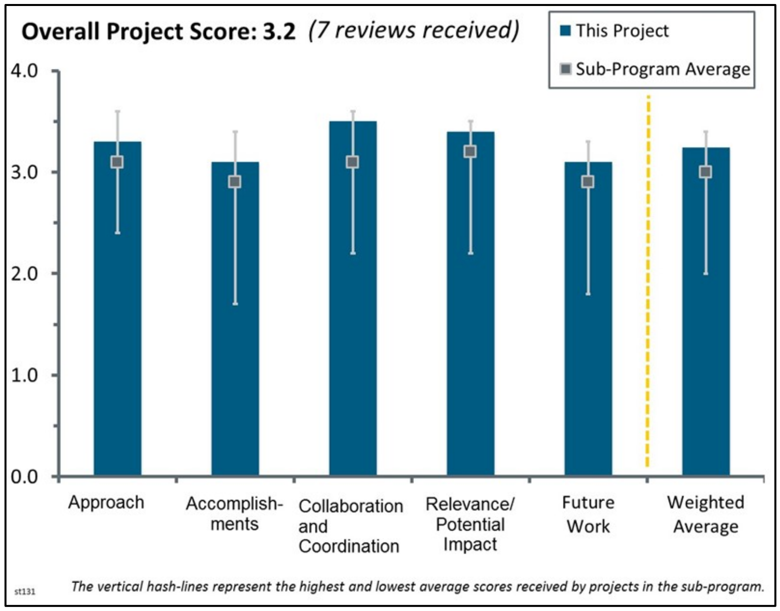
- The explanation of HyMARC must be restated clearly for the hydrogen storage community to fully understand its role in developing testing and validation platforms (i.e., developing the toolset to improve materials discovery). Because some materials systems must be selected and used for developing test and validation platforms, the team should identify the test/validation platform demonstrated by each material system selected for future review cycles. No system should be selected without clearly defining its usefulness in the testing schema.
- The project should remove the metal hydride encapsulation effort and remove the Lewis acid effort on silica templates. While plasmonics are trendy, it is not clear why their application is relevant to the Program, and they should be deleted.
- There does not appear to be a need to investigate the aluminosilicate materials proposed here. The control and understanding of Bronsted versus Lewis sites appear poorly conceived, and the nature of the sites has apparently not been investigated. The performance of the one material examined is significantly inferior to

existing sorbents, and the work does not integrate with the rest of the project. It appears that this could profitably be dropped to make more rapid progress in other areas.

Project #ST-131: Hydrogen Storage Characterization and Optimization Research Efforts

Thomas Gennett; National Renewable Energy Laboratory

Brief Summary of Project:



This project represents a collaboration between national laboratories to investigate the properties of promising new hydrogen storage materials. The National Renewable Energy Laboratory (NREL) leads the collaboration, which includes Lawrence Berkeley National Laboratory (LBNL), Pacific Northwest National Laboratory (PNNL), and the National Institute of Standards and Technology (NIST). The objectives are to (1) develop new characterization capabilities such as nuclear magnetic resonance (NMR) spectroscopy, diffuse reflectance Fourier-transformed infrared spectroscopy (DRIFTS), calorimetry, diffraction, and scattering; and (2) validate performance claims and theories critical to the design of new hydrogen storage materials.

Question 1: Approach to performing the work

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

- This project combines the unique diagnostic capabilities primarily at NREL, LBNL, PNNL, and NIST to extend and complement assessments of candidate hydrogen storage materials with the Hydrogen Materials–Advanced Research Consortium (HyMARC) and independent Fuel Cell Technologies Office (FCTO) projects. Providing this resource for comparative assessments of experimental measurements of the critical gravimetric and volumetric capacities of candidate hydrogen storage materials is imperative if progress in future discovery projects is to be successful. Widespread and timely dissemination of this work is needed, along with explicit documentation of all sources of errors and development of practical protocols.
- The approach focuses on validation and characterization of hydrogen storage properties in sorbent materials. An extensive array of advanced diagnostic tools and computational capabilities is being employed by the NREL-led team to address critical issues and major barriers. The approach is well formulated, and it provides a solid foundation for achieving rapid progress that should affect and enhance our understanding of physiochemical properties and reaction mechanisms in hydrogen storage materials. A qualified team capable of validating performance claims and theories serves an important role in the overall U.S. Department of Energy (DOE) Hydrogen Storage effort.
- This project is very relevant to DOE barriers, especially relative to characterization techniques and to validating claims, concepts, and theories of hydrogen storage materials developed by others. It is primarily a four-party effort, with an NREL lead coupled with LBNL, PNNL, and NIST. As such, this helps to avoid duplication with other DOE efforts, especially those active within HyMARC. Validations of problematic outside claims, including those of other DOE contractors, are very important. The overall approach is sound. International standardization of pressure-composition isotherm (PCT) testing is very important; this project is the main worldwide effort in this area. Development of thermal conductivity measurement techniques is valuable to all DOE projects involving the engineering applications of solid-state H-storage materials.

- NREL has developed a number of tools for the measurement and analysis of adsorbent materials necessary for the Hydrogen Storage program. The work on validation that NREL performs for other laboratories is an important service, and the variable temperature work will be of value, if adsorbents with reasonably constant isosteric heats are ever developed.
- This is a solid approach to developing more in-depth techniques for characterizing adsorbed hydrogen, including a broader temperature range for PCT measurements, DRIFTS, and calorimetry measurements; incorporating neutron scattering and diffraction techniques; and overall, providing a capability for the validation and verification of hydrogen storage materials and/or concepts. The effort also supports a very good NMR effort at PNNL, which can be valuable in helping to characterize complex metal hydride systems, but this effort seems to lie outside of the core interests of the overall collaboration, which is adsorbed hydrogen. The stated objective is to provide validation. The group should be judicious in selection of the materials chosen to validate concepts to ensure (1) that they are making the right model material choice but (2) that there is appropriate involvement of appropriate materials synthesis experts to provide said material. The catecholates as probes of metal binding energetics is perhaps defensible; the extension to placing metal catecholates on nanotubes was not well supported and seemed to stretch the validation of the concept theme.
- Development of core capabilities would be of more value if there were more users in need of these capabilities. Given the rather small size of the DOE storage portfolio, there does not seem to be a critical need for these facilities.
- The approach of the group seems rather random and focused on the existing capabilities or interest of the partners rather than identifying the characterization method gaps and high-priority needs of the researchers to accelerate material discovery.

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.

- To date, the effort has successfully collaborated in reproducing LANL capabilities at NREL in variable temperature and variable pressure calorimetry, and is developing a low-temperature/high-pressure PCT instrument that will be valuable to the community. The effort has been valuable in bringing a vastly better understanding of how to accurately and precisely measure hydrogen sorption, and how to report the results. This is not a trivial task, but it is one that the project has done exceedingly well and that helps improve the science being done in the area of sorbents. The round robin on volumetric capacity using standard materials that they have run has turned out to be highly enlightening and valuable to the sorption community, and brought greater confidence to DOE and DOE’s ability to assess progress and claims among various projects.
- The thermal conductivity work is continuing and will provide data of value to modeling efforts, especially with regard to electronically insulating materials. The volumetric data of slide 11 is particularly informative and is illustrative of the range of data that should be reported by all studies involved with work of this type. The round robin may be of some use in informing particular laboratories of the reliability of their measurements, if the participating laboratories take the time and effort to evaluate their data. The Ca oxalate data are particularly intriguing. It is hoped that the observed effects and explanations are published soon.
- There has been much useful progress in the first nine months of this project. This is expected, given the large number of researchers involved. This work has covered a wide variety of materials and techniques, including development and use of characterization hardware (PCT and thermal conductivity). All of the progress is clearly useful to other DOE activities. Management of the round-robin testing effort has been excellent.
- This is a new project; progress has been satisfactory during the first few months of operation.
- The accomplishments of the project are good for just having started many of the initiatives. The inconsistent effects of the metal on desorption temperature should be further studied. It would be helpful to identify the current state of characterization and the progress of the team to improve or enhance these methods.

- The project is off to a strong start. Initial results, especially in the areas of characterization and validation, are very encouraging. The results on the effect of metal cations on desorption temperature in oxocarbons are intriguing (albeit puzzling). Although results obtained from that work might motivate additional study, the low gravimetric capacity in these systems essentially eliminates them as relevant candidates for further work (unless a dramatic new approach to enhance the capacity is forthcoming). If those materials are indeed eliminated, a fallback position involving other systems will need to be developed by the team and communicated to the FCTO. Good preliminary results have been obtained on single-walled carbon nanotube (SWCNT) sorbents and other ultra-microporous materials. However, the same concern about their relevance to supporting DOE goals applies. Close collaboration with NIST is facilitating solid progress.
- Since this team started most of their collaborative tasks during fiscal year 2016, the progress on joint work involving complementary techniques is really quite good, although establishing productive interactions with the HyMARC partners should receive some additional attention. Granted, it is not always straightforward to define common goals and plans. NREL has done well with both the thermal conductivity apparatus development and updating PCT methodology for addressing the challenges of reliably measuring the volumetric capacities. The improvement to the PCT systems for more direct determination of heat of reaction from variable temperature measurements should be an important new resource. It is less clear how much value there can be gained from the physisorption studies reported on slides 14–16. With gravimetric uptakes of ~1 wt.% or lower, even if desorption temperatures are higher; these materials seem to be very poor practical candidates to achieve the DOE hydrogen storage targets.

Question 3: Collaboration and coordination with other institutions

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

- There are numerous outstanding collaborations among the four partners, HyMARC, and other outside researchers and organizations.
- Excellent collaboration was exhibited in the round-robin sample exchange for sorbents.
- Strong collaborations are evident.
- The potential for very productive joint studies is high from these partners. A number of mutually beneficial joint efforts are either starting or currently underway between NREL and its partners on some specific materials such as the metal–organic frameworks (MOFs). Collaboration with the current HyMARC team appears a little slower but is improving.
- The project involves a high level of collaboration among the partners by the nature of the team. The internal collaboration effort description was helpful in identifying the interaction of the team. It was also useful to have the information about the collaborations with HyMARC.
- Extensive collaborations among researchers in this project and external investigators are evident. The project is well managed, and the activities appear to be well coordinated. This effort directly complements work on the companion HyMARC project. However, at this stage in both projects, the collaboration and cooperation by researchers in the two projects appears limited. It will be important to improve/enhance collaboration between the two projects in the near future.
- Collaborations with laboratories with differing expertise are ongoing.

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.

- Bringing accurate and reproducible sorption measurement protocols to the community is invaluable, as it inspires greater confidence in interpreting reported, well-performed measurements.
- While the combined NREL team and HyMARC efforts are still evolving, the reviewer is very optimistic that significant characterization and validation results will be achieved through diverse and rigorous utilizations of in situ measurements with neutron scattering, NMR, and DRIFTS studies that complement enhanced PCT determinations of hydrogen storage capacities. Such information should provide critical

insights into the various theoretical efforts on identifying and verifying reliable mechanisms for promising hydrogen storage materials.

- This project is an important component of the DOE Hydrogen and Fuel Cells Program (the Program) portfolio. It directly complements and supports the companion HyMARC effort, and it provides a valuable resource to DOE for expert validation of performance claims and theories relevant to emerging hydrogen storage materials.
- There is no question that the research and development activities are aligned well with the Program and DOE RD&D objectives and have the clear potential to advance progress.
- This project has high relevance since the team can provide valuable tools for researchers (variable-temperature PCT, high-pressure thermal conductivity, etc.) to evaluate materials. The linkage between the characterization techniques and the targeted attributes for improvement could be highlighted to further improve the relevance of certain work streams within the effort.
- Relevance will be driven by how often the new measurement capabilities are used. At this stage, the user base is small.

Question 5: Proposed future work

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

- The NREL-led round-robin activity on determination of volumetric capacity for a standard adsorbent would be a very valuable contribution, as the reproducibility of published values is much too often contradictory and/or misleading. Combining powerful neutron and NMR assessments to probe the actual reactive species within high-potential borohydride phases and MOFs is very worthwhile. Assessing the potential for the in situ thermal conductivity and DRIFTS techniques is appealing. More emphasis on preparing to evaluate new candidate materials from HyMARC and future independent funding opportunity announcement (FOA) projects should be made with less effort devoted to those much less promising systems with hydrogen capacities <2wt.%, even though they could serve as models.
- The plan for the future is quite reasonable. It is a little hard to be sure all of the activities do not overlap with other DOE-funded projects, past and present. The close coordination among the four partners, HyMARC, and other collaborators seem to minimize the chances for that.
- Proposed future work follows straightforwardly from initial efforts. A complete and compelling description is provided. A candid evaluation of the metal-oxocarbon and SWCNT work is needed. Given the limited time and resources that are available, focusing on low capacity systems may be ill-advised.
- The focus on the metal catecholate work seems to be shifting from validation to much more detailed characterization than was justified.
- The proposed future work appears to be focused on material development rather than the characterization techniques. It would be beneficial to highlight the linkage between the future work in the area of material development with the advancements in characterization.
- Most of the future work that does not rely on outside participation is logical.

Project strengths:

- A well-qualified team with extensive experience in all areas relevant to the project objectives is conducting research on this project. The project is well managed, and good collaboration among participants is evident. The project fills an important need for DOE, especially with regard to unambiguous validation and testing of performance theories and claims in emerging hydrogen storage materials. The access to advanced characterization tools developed on this project will be of great benefit to the hydrogen storage community.
- Excellent sorption characterization capability is being augmented with new capabilities for characterization of sorbed hydrogen. The project has done an excellent job of bringing a greater understanding of sorption characterization to the materials synthesis community, which will improve the quality of data being reported.
- There are strong characterization and materials property validation efforts. New people are involved and working with more established materials researchers. The work on sorbents is important.
- NREL expertise in characterizing and validating the hydrogen storage capacities for diverse classes of materials is a valuable attribute, and enhancing the ability to perform measurements of isotherms at

multiple temperatures would be a great benefit for obtaining heats of reactions. It would be especially useful if NMR, neutron scattering, and infrared/Raman spectroscopies could substantiate whether conflicting empirical or model interpretations could be either verified or dismissed for these macroscopic properties.
- The strength of the project is the highly collaborative effort with the key researchers in the field of characterizing materials for hydrogen storage. The scope of the project is important to have the necessary toolset to understand the materials characterization in order to optimize parameters.

Project weaknesses:

- There are not really any weaknesses.
- A careful consideration of research directions relative to the ultra-microporous materials is imperative. The pathways that have been selected thus far in the NREL project (e.g., oxocarbons and SWCNTs) may have serious limitations that might require a mid-course correction. A detailed evaluation needs to be done, and alternative plans (if needed) should be formulated.
- The weakness of the project is the unclear division and direction between a focus on characterization techniques and materials development. The prioritization of the effort should be coordinated based on the necessity, rather than the interest of the partners.
- The major concern with NREL efforts is their continuing focus on low-potential candidates for adsorption since similar work could be done on more promising MOFs, etc.

Recommendations for additions/deletions to project scope:

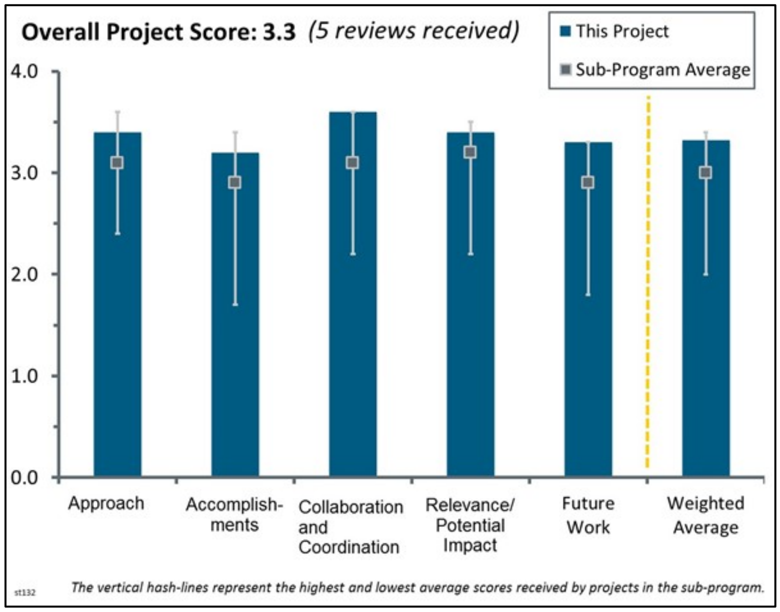
- Since the extent of characterization and validation requests from the HyMARC program and new FOA projects remains to be seen, it would be premature to make any significant changes in scope at this time. However, NREL and its partners should be preparing detailed specification and protocol documentation that facilitates submission and handling of samples suitable for characterization without compromising integrity of the materials during processing or the experiments. A review panel (perhaps with outside advisors) should assess and prioritize candidates to select those most likely to benefit from the use of the available instrumentation. Continuing to evaluate either oxy-carbon or catechol-based materials is not recommended unless they have the potential for storing more than ~5 wt.% hydrogen, as there are several more promising candidates worthy of such detailed assessments.
- Closer collaboration with HyMARC investigators is strongly encouraged. Although increased inter-project cooperation is part of the project plan articulated by the NREL team, each group appears to be operating primarily independently. In order to achieve significant impact in the “rational design of new materials,” the NREL team and HyMARC investigators should be engaged in a more direct way. This can be a potent collaboration that directly supports the overall Hydrogen Storage program goals.
- The project should be judicious in choosing areas that can be adequately justified in terms of either providing greater understanding of a concept or providing validation of a material. Other efforts in materials synthesis may be distractions from the main stated objective.
- The recommendation for the project scope is to evaluate the current state-of-the-art characterization methods and determine the gaps or areas of improvement to make the most significant impact in advancing materials discovery for hydrogen storage.

Project #ST-132: Hydrogen Storage Characterization Research Efforts

Tom Autrey; Pacific Northwest National Laboratory

Brief Summary of Project:

This project is part of a collaboration between national laboratories to develop new characterization capabilities to investigate the properties of promising new hydrogen storage materials. Pacific Northwest National Laboratory (PNNL) will focus on nuclear magnetic resonance (NMR) spectroscopy and calorimetry to complement parallel efforts at other national laboratories. The project will also work toward validating claims and theories critical to the design of new hydrogen storage materials that show promise.



Question 1: Approach to performing the work

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

- In this new fiscal year 2016 project, PNNL will primarily perform specialized high-resolution NMR experiments in order to identify reaction pathways for hydrogen absorption, adsorption, and desorption processes. In contrast to most prior NMR assessments, the PNNL measurements will be conducted largely in situ over large ranges of both temperatures and pressures that correspond much more closely to operating conditions in hydrogen storage applications. This information can potentially provide very detailed insights into the atomic-scale behavior in practical environments and also test whether theoretical mechanisms are valid. In some cases, the NMR studies will be in conjunction with reaction calorimetry and modeling efforts at PNNL, as well as collaborative neutron scattering, infrared (IR) spectroscopy, and other techniques with the National Renewable Energy Laboratory (NREL)-led Characterization and Validation team and the Hydrogen Materials–Advanced Research Consortium (HyMARC). Eventually, promising materials from the new funding opportunity announcement (FOA) project would be provided for the advanced in situ NMR characterizations.
- The approach has a rational design—understanding chemistry to characterization—and the team will interact with groups developing new materials using its tools. The work brings together complementary research tools, including NMR and calorimetry, to complement NREL, neutron scattering at the National Institute of Standards and Technology (NIST), and studies done at Lawrence Berkeley National Laboratory (LBNL). The current focus is on validation of the cycles between BH_4 and $B_{10}H_{10}$ on a Mg system. Additional focus is being placed on the development of additives using rational design.
- This project focuses on a number of U.S. Department of Energy (DOE) barriers, in particular N (Understanding of Hydrogen Physi-and Chemi-Sorption) and O (Evaluation Facilities). Characterization efforts include NMR and reaction calorimetry of both physisorption and chemisorption materials. This project aims at developing the fundamental understandings of hydrogen storage materials to help validate others' claims and to aid future DOE efforts toward practical solid- and liquid-state hydrogen storage materials. The scope of the work is nicely compartmentalized to complement the related objectives of the partners NREL, NIST, and LBNL. The approach is very reasonable.
- PNNL's approach to working within the Characterization and Validation team and collaborating with HyMARC is to bring state-of-the-art NMR capabilities to bear on materials of interest ranging from sorbents to complex hydrides to chemical hydrogen storage materials.

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 science is very strong. Because the NMR Pake pattern width depends on hydrogen dynamics and the work will collaborate with inelastic neutron scattering (INS), it provides a broader view of hydrogen dynamics than would be possible without this collaborative effort.
- At this point, the project is demonstrating the viability of the approach of using NMR techniques to gain information about reaction enthalpies of sorbents, hydrides, and chemical storage materials. Additionally, the researchers have demonstrated that low-temperature solid-state NMR may be used to help characterize hydrogen on metals bound to sorbents and have shown that these data are highly complementary to INS. It will be interesting to see how much information can be extracted from the NMR data at variable low temperatures. Using solution NMR and reaction calorimetry in a commercial calorimeter to characterize hydrogenation products or heats of hydrogenation/dehydrogenation is too routine to be considered novel or innovative, and researchers who would be interested in these sorts of materials systems would very likely have such capability. On the other hand, the high-pressure in situ solution and solid-state/MAS NMR capability using PEEK sample tubes is very nice, and that would seem to be readily transferable to those interested in adapting similar high-pressure NMR sample containers for the solid or solution state to their own facilities. So that is a valuable contribution to the field and will likely find a lot of use by those interested in the Mg-B materials system. The hypothesized reaction network in the Mg-B hydrogenation system mapped using Wade's Rules is thought-provoking and may help to decipher the reaction network. Perhaps there will be some clues regarding kinetics barriers among one or more of those possible intermediates that may shed light on the underpinning kinetics issues of the hydrogenation/dehydrogenation of Mg borohydride.
- Much of the PNNL effort has been to adapt or develop the specialized NMR instrumentation required to permit the desired in situ studies over the extended environmental conditions. These are generally not trivial modifications and resulted in PNNL mostly performing various initial feasibility experiments. The expectations are to generate more tangible results within the next several months that will explicitly provide insights on the detailed compositions of species involved during reactions with the desired hydrogen storage conditions.
- The project is new; good progress has been made in less than one year's time.
- Good progress has been made in the first eight months of this project in areas of NMR, reaction enthalpy, surfaces, and reaction paths. There is a wide range of material-technique combinations involved in this work. The presentation does not make it completely clear why these particular combinations were chosen from among other possibilities.

Question 3: Collaboration and coordination with other institutions

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

- There is an outstanding list of partners, collaborators, and interactions with HyMARC (slide 2). It is very encouraging to see some international collaboration with International Energy Agency–Hydrogen Implementing Agreement Task 32 and Japan's National Institute of Advanced Industrial Science and Technology (AIST).
- The project clearly has very good collaboration within PNNL in the NMR area and is bringing that capability to HyMARC and the Characterization and Validation team.
- PNNL has already initiated several collaborations to utilize NMR to probe details of bonding and reactions of hydrogen with adsorption media and the magnesium borohydride–boranes–borides. A caveat is to ensure that sample processing and handling do not compromise the integrity of the often highly reactive and air-sensitive materials. Presumably, PNNL will make the appropriate instruments available for measurements to members of the HyMARC and others.
- Strengths include that the work involves a broad network of collaborations on $Mg(BH_4)_2$ systems, including with Craig Jenson, Brandon Wood, Vitalie Stavila, Bob Bowman, and international groups. Other key and strategic collaborators include visiting professor Gary Edvenson to develop Wade's Rules to predict

structures and reaction pathways. A weakness is that the work clearly overlaps with the other Characterization and Validation team groups—but overlaps with HyMARC are not so obvious.

- Very strong collaborations are evident.

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.

- Adding NMR signatures to the overall set of signatures in the broad family of hydrogen storage materials is highly relevant and can have an impact by providing guidance and validation to experimental and computational efforts.
- In situ NMR spectroscopy of the hydrogen isotopes as well as host elements (and catalysts in favorable circumstances) has outstanding potential for providing quantitative details on the microscopic species and competing reactions that determine both reversible and irreversible reactions of the hydrogen storage media. These studies should strongly complement other methods, including neutron scatting, vibration spectroscopies, and electronic spectroscopies to verify or defunct theoretically proposed mechanisms. Clarifying the responsible and possibly competitive processes should help establish the potential and limitation of proposed storage candidates, including the roles of additives/dopants.
- The strengths include that a unique toolset is being used (to examine relevant storage systems: $Mg(BH_4)_2$). Likewise, rational design of the selection of additives (and criteria for selection of them) is a nice feature of the project. A weakness is that little discussion has been given to systems to be studied beyond the Mg-B-H system.
- The project clearly supports and advances progress toward the DOE Hydrogen and Fuel Cells Program goals and objectives as delineated in the most recent Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. However, it should be noted that progress within the three-year projected life of the project will be mostly understanding fundamental science and techniques rather than achieving the practical DOE targets of weight and volume.
- The project is mostly focused on addressing important phenomena involving relevant materials.

Question 5: Proposed future work

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

- The specific tasks being conducted or initiated at PNNL via NMR and other methods all have merit to either demonstrate feasibility or test proposed models and concepts. It is important that modifications can provide suitable pressure and temperature conditions to mimic those found in practical hydrogen storage applications to permit consistent comparison with empirical and theoretical reaction mechanisms.
- PNNL has a good plan to map onto the Characterization and Validation team needs and HyMARC needs and fill gaps in capability. It also brings some chemical science expertise and intuition to the overall effort.
- The addition of computational studies, particularly on the binding energy of hydrogen, is suggested. INS and QENS will be undertaken to complement the NMR studies to get more information on the important effect of hydrogen dynamics. These are a useful set of future plans.
- The proposed future work seems logical. The two go/no-go decision points (slide 29) may be difficult to achieve.

Project strengths:

- PNNL possesses numerous advanced NMR spectrometers that should be capable of greatly enhancing the sensitivity and selectivity of measurements on viable hydrogen storage media. The breadth of capabilities of these instruments should provide much greater potential to probe details of processes responsible for their behavior. Many PNNL staff members possess strong expertise in NMR and other characterization methods, and have knowledge and experience with diverse hydrogen storage materials.

- Project strengths include the strong emphasis on extending the state of the art of NMR technology for adsorbents and chemisorbents, strong national and international collaborations, and the new scientists in the hydrogen storage materials area.
- Overall, this is a very strong project. The plans for studying storage-relevant systems are broad and are poised to provide unique insights into material dynamics.
- There is an extremely strong institutional effort in state-of-the-art NMR at PNNL, and this project can provide some access to that capability.

Project weaknesses:

- Perhaps this is not a weakness, but the calorimetry and conventional solution-state NMR capabilities presented are likely available at any institution where there would be an interest in addressing hydrogen storage materials such as chemical hydrides.
- Little discussion beyond the Mg-B-H system is provided. The additives being developed (and/or the rational design approach to their development) could possibly work well for other storage systems.
- To obtain the comprehensive data necessary to derive in-depth assessments of the hydrogen storage materials, substantial time on specialized NMR systems probably will be needed. This requirement may be in conflict the with “user facility” status of many instruments that operate in PNNL’s Environmental Molecular Sciences Laboratory, limiting the needed time to perform the experiments.
- There are unclear justifications for all the materials selected for study.

Recommendations for additions/deletions to project scope:

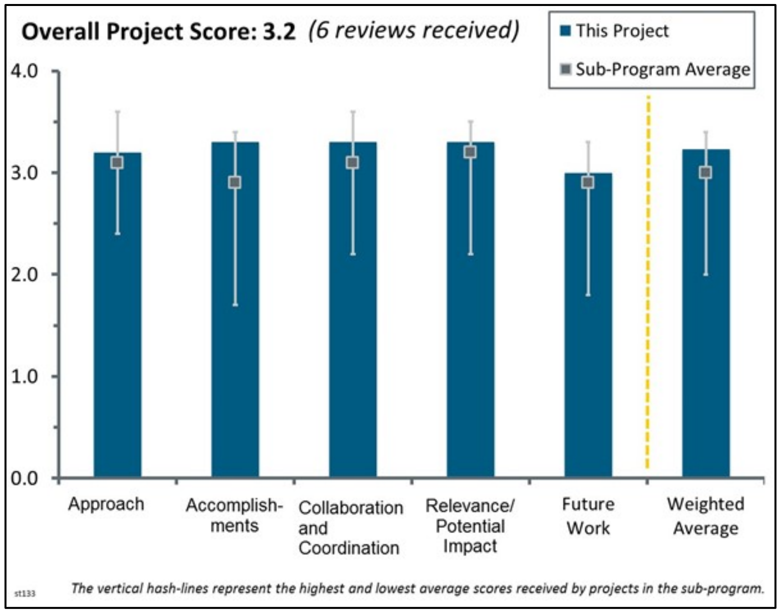
- Assuming the feasibility experiments with the adapted NMR systems are successful, at least one or more carefully integrated investigations involving the combinations of in situ characterization techniques should be conducted with PNNL partners to address issues such as whether more than two hydrogen molecules can bond to metal sites in a well-defined metal–organic framework or whether catalysts can promote kinetics and improve reversibility for a high-capacity borohydride.

Project #ST-133: Hydrogen Storage Characterization and Optimization Research Effort

Jeffrey Long; Lawrence Berkeley National Laboratory

Brief Summary of Project:

This project is part of a collaboration between national laboratories to develop new characterization capabilities to investigate the properties of promising new hydrogen storage materials. Researchers will also validate new concepts for hydrogen storage mechanisms in adsorbents and provide accurate computational modeling for hydrogen adsorbed in porous materials. Specifically, Lawrence Berkeley National Laboratory (LBNL) is developing in situ infrared (IR) spectroscopy as a tool for characterizing emerging hydrogen storage materials as well as developing metal organic framework materials that will allow for more than one hydrogen molecule per open metal site that will increase hydrogen capacities for sorbent materials.



Question 1: Approach to performing the work

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

- Leading-edge work on high-capacity hydrogen storage in metal–organic framework (MOF) sorbents is being conducted in this project. The approach is innovative and is keenly focused on long-standing barriers to achieving high-capacity storage at elevated temperatures in sorbent systems. The M_2(*m*-dobdc) and M_2(dsbdc) systems are showing great promise for high-capacity storage. The approach that has been employed to exploit the unique properties of these materials is novel and is producing useful results in a timely way.
- The approach to MOF synthesis is to focus on designing and synthesizing MOFs with strong binding sites, such as metal cations with incomplete coordination spheres. This strategy is well known to these investigators and builds on prior work over several years. A structural isomer of MOF-74 is the initial focus. Synthesis, characterization, and modeling are being carried out to determine where hydrogen binds and the nature of the interaction.
- The approach is good overall.
- The approach was focused on materials development and mechanistic validation of binding multiple hydrogen molecules per metal cation, which was important to demonstrate, but the project seemed separate from, rather than part of, the Characterization and Validation team.
- This is a relatively new project designed to bring in situ IR capabilities to hydrogen materials analysis. This effort also continues a prior effort to research coordination polymer-type materials. There were too many introductory slides. Slide 10 shows a plot for pure hydrogen that is incorrect. Hydrogen density is not linear over this range, and at 100 bar, it maxes out at 31 g/L.
- The title of this project conveys the impression that it will have a rather broad scope; however, the LBNL presentation itself indicated that the work is apparently limited to only specific transition metal-containing MOFs with the tenuous potential of bonding two or more hydrogen molecules on the metal locations. Comparisons of the specific MOFs that would nearly meet the DOE “SYSTEM” volumetric target shown on slide 8 are very misleading. For example, the principal investigator (PI) states on slides 17, 18, 21

(although he does say it is “maximum” capacity), 40, and 50 that the MOFs are close to the DOE volumetric target of 40 g H_2/L, but this will be reduced by a minimum factor of two when the storage volume is included.

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.

- Excellent progress has been made on the project during this reporting period. Important results that are greatly improving our understanding of hydrogen binding to metal-containing MOFs have been obtained. The studies of hydrogen sorption on Ni_2(*m*-dobdc) and Mn_2(dsbdc) are especially noteworthy. The first example of multiple binding of hydrogen molecules to a single metal center in a MOF (Mn_2(dsbdc)) is an important and impressive achievement. The results obtained from variable temperature/pressure in situ IR spectroscopy have highlighted the importance of that characterization technique for measurement of site-specific adsorption enthalpies. On a separate note, given the unique information that can be obtained from those measurements, it is surprising that the variable temperature/pressure in situ IR measurement capability is available only at Oberlin College and not at one of the partner institutions (e.g., NREL). The availability of the instrument at one of the project collaborators would obviate the need to develop the capability at LBNL.
- The project made excellent progress in confirming the multiple hydrogen binding to an open metal site (OMS), which was a significant step toward the viability of this mechanism, even though the results of the multiple binding did not provide advanced performance for the particular material evaluated. It appeared that some of the MOF-74 material effort with *m*-dobdc was developed previously.
- The effort to develop the DRIFTS spectrometer is on track. An instrument has been ordered following evaluation of several designs. The Ni_2(*m*-dobdc) MOF shows higher uptake at all T < 100°C than compressed hydrogen up to 100 bar. Capacity of this material is 58% of the system target if a temperature swing is used. Collaboration with NREL verifies accuracy of high-P isotherms measured at LBNL. A very significant result is the demonstration that two hydrogen molecules can bind to a metal center in a MOF. The MOF used is not relevant to hydrogen storage because of gravimetric capacity limitations, but nevertheless this work gives hope that multiple hydrogen binding is at least possible. Modeling is providing binding geometries, and these were compared with neutron data obtained at NIST. It is interesting that the models over-predict the strength of the hydrogen–metal interaction (distances are shorter than observed). This is consistent with the predicted binding energy, which is much higher than the experiment. These calculations show the limitations of present theoretical methods and are pushing the investigators to make improvements to the models. Modeling does show that replacing Mn in the structure with Ca should increase the binding energy. New catecholate MOF structures look promising, although the nearly identical binding energies predicted by theory are curious and seem unlikely to be borne out in practice. Other MOFs with potential for binding more than one hydrogen per metal are being synthesized.
- This effort has just begun, so accomplishments are somewhat limited at this point. A number of the viewgraphs covered prior research efforts and were not necessary. Expanding on details of the Mn_2(dsbdc) structure would have been of more value. It appears that the structure, as an isomer of the *m*-dobdc material, has a similar loading to the *m*-dobdc material, which should not be surprising. What is curious is the low but nearly constant isosteric heat. This appears to run counter to the higher isosteric heat that was calculated as shown in slide 29. The charge transfer in this case was presumably calculated using a fragment of the actual structure. Given that two molecular hydrogens can be accommodated at the Mn site, one would expect a higher initial isosteric enthalpy of adsorption (Q_{st}) than shown in slide 26. The availability of an open metal center site for this structure appears to have made no difference to the adsorption properties over that of a dobdc structure.
- The LBNL team has an extended theoretical basis for optimizing multiple hydrogen bonding on a single metal atom in a MOF compound and has evidence for bonding two hydrogen molecules. Because a nearly constant (and low) 5.5 kJ/mol binding was shown in slide 26, schemes for significantly stronger binding will be needed to raise operating temperatures to circa 150+ K. The delays in obtaining and setting up the DRIFTS instruments are having a negative impact on the schedule.

- The demonstration of two hydrogen molecules per metal center is an important accomplishment. There is a large discrepancy between the calculations and experiments regarding the adsorption of two hydrogen per Mn in Mn_2(dsbdc). Both the energies and geometries differ significantly; the origin of this discrepancy is unclear. Several slides covering M_2(dobdc) and M_2(*m*-dobdc) were presented last year. This repetition makes it difficult to assess the quantity of new work performed.

Question 3: Collaboration and coordination with other institutions

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

- Collaborations with NREL and NIST are well established and very productive. The close link between the synthetic efforts and the theory and characterization is proving very powerful in guiding synthesis of modified structures. This is clearly the "A team."
- Extensive collaborations with partners at NREL, PNNL, and NIST are evident. Those collaborations have been important to the success achieved thus far on the project. As this project continues, it will be important to engage more actively with investigators in HyMARC who are pursuing related approaches.
- Strong collaborations exist with several partners.
- The collaboration with NIST is very good and needed for accomplishing the mechanistic validation of binding multiple hydrogen molecules per metal cation. The coordination with the other members within the Characterization and Validation team is not as defined.
- LBNL has worked very well with both NIST and Oberlin College for neutron scattering and DRIFTS characterization, respectively, of their materials. NREL appears to have provided independent confirmation on the storage capacities of the LBNL-made materials. It was not apparent from the PI's AMR presentation that roles and potential for nuclear magnetic resonance (NMR) characterization are appreciated or being included in the planning and modeling efforts. This would be an oversight.

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 assessment of predicted capacities on slide 40 appears to show marginal improvements to material capacity on the basis of crystal bulk density. Given the normal packing density of materials of this type, it is not clear that any of the newer UiO structures offer possibilities that suggest substantive improvement in volumetric density over that of MOF-5.
- The project is clearly focused on the critical limiting factor for hydrogen storage on sorbents: inadequate volumetric capacity at the temperature required (ambient, non-cryogenic) needed for practical implementation. The new IR spectroscopic tool will provide a new capability to the Hydrogen Storage program and will provide information previously unavailable except by neutron techniques. The theory effort will, for the first time, identify MOFs that have the potential to bind more than one hydrogen per OMS. OMSs are difficult to model accurately, so this effort should be able to generate binding energies for a range of structures with better accuracy than any of the previously used models. Clearly, this is critical for determining which direction to go to develop sorbents (not just MOFs; the result could be potentially relevant to doped carbons as well) that meet DOE targets. It seems likely that the project will be able to determine the limits of multiple hydrogen binding and determine whether MOFs having this property can store enough hydrogen to meet DOE targets. This project currently has the best chance of solving the sorbent storage problem of any known to this reviewer.
- If the LBNL team can prepare and demonstrate MOF compounds that allow multiple hydrogen adsorptions on most metals in the host lattice and also have binding energies factors of three to four greater than conventional physisorption materials, these materials may be viable candidates for vehicle storage. On the other hand, results obtained to date seem to indicate they are far below those required levels for both capacities and binding energies.

- The project is closely aligned with the goals and objectives of the Program. Development of high-capacity sorbent materials for hydrogen storage is an important DOE objective. This project is at the leading edge of that effort.
- This is a highly relevant project.
- The project is relevant in addressing the barriers and understanding of hydrogen storage adsorbent materials, although the support of the Characterization and Validation team is less clear within the project work.

Question 5: Proposed future work

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

- It is good to see that the LBNL team is going to concentrate on improving and adapting synthesis methods to form the MOF compounds with predicted and modeled desired properties.
- Proposed work follows logically upon the discoveries made thus far. The project definitely has its eyes on the prize, and there is little in the way of extraneous activity that is evident. The new DRIFTS instrument should be online in the near future and will be a valuable tool for other projects as well as this one.
- The proposed work is a logical and straightforward extension of the research conducted in the first year of the project. The experimental effort is well formulated and will likely result in solid progress toward project goals. However, the computational effort is not clearly described. It is not apparent whether additional theory and modeling work is needed or will be done in the next reporting period. A clear pathway to the discovery of a sorbent material that meets DOE storage goals is not readily apparent (the reviewer fully recognizes the daunting challenges that exist).
- The proposed future work is very appropriate for a materials development project, but the scope should also include support of the Characterization and Validation team.
- On slide 44 for proposed future work, the effort that is directed at dsbdc analogs is understandable for the sake of systematization, but given that these materials are isomers of dobdc structures that have limited surface area and volume to accommodate hydrogen, it is not clear that this class of structures is worth pursuing through the project. Even if higher Henry's law values are obtained, the marginal improvements to volume density suggest that other structures with OMSs or charge transfer effects that effectively alter adsorption enthalpies should be the primary goal of further effort.
- Experimental details are well described, but the computational effort is not mentioned at all.

Project strengths:

- The excellent, top-notch team integrates all needed disciplines: synthesis, characterization, and theory. The deep history working with MOFs as gas storage media is another project strength. The highest levels of theory are being brought to bear on a difficult problem. All elements of the project inform the others—theory-guiding synthesis, characterization assisting in validation, and synthesis responding to knowledge gained from the other two.
- The LBNL team has identified and prepared several MOF compounds that point to improved performance toward these materials meeting the DOE targets. Furthermore, comprehensive neutron scattering results have proven to be very valuable to validating or repudiating theoretical predictions of hydrogen adsorption mechanisms and structures. Adding new in situ NMR studies into the project should also complement and extend these assessments.
- The research and development team assembled for this project is first-rate, and the results obtained thus far on the project are most impressive. There is a clear understanding of the barriers and obstacles, and a solid, comprehensive project plan is in place to address them. Extensive internal and external collaborations are advancing the pace of progress.
- The strength of the project is the accomplishment of providing the field a first example of multiple hydrogen molecules on a single metal site. This result could provide an opportunity to increase the capacity of adsorbents significantly. Another strength of the project is the depth of the researchers on this project and their disciplined pursuit of advancing MOFs.
- The project has strong collaborations and is focused on materials of high relevance to the Hydrogen Storage program. A successful outcome will have a large impact.

Project weaknesses:

- No weaknesses were identified.
- Although notable progress has been made in this project, the fact remains that none of the materials investigated thus far meets the DOE hydrogen capacity targets. This presumably will require either an MOF having metal sites capable of binding more than two hydrogen molecules per site or a MOF with a much higher metal density. Obtaining either of these is a serious challenge, requiring an approach and material system(s) that have not yet been identified.
- In many ways, this “new” project appears to be a direct continuation of the prior LBNL work on MOFs with a down-scoped focus on multiple hydrogen molecules adsorbing on metals. The role LBNL will have to support other researchers is not apparent. It was not clarified how much time will be available for the DRIFTS instrument to perform those critical measurements on hydrogen binding energies for other research groups investigating hydrogen adsorption mechanisms.
- The weakness of the project is the focus on materials development rather than on supporting the Characterization and Validation team.
- Overall, the work presented is of very high quality. Nevertheless, this project is one of the largest in the storage portfolio, and by that metric, the quantity of research should also be high. Much of the presentation focused on older results presented at last year’s AMR. In addition, the number of new compounds explored appears to be small. Together, these observations suggest that progress is below expectations. It is recommended that the presentation more clearly call out the new work performed since the last AMR.

Recommendations for additions/deletions to project scope:

- Cycling and reversibility studies are important to fully evaluating the efficacy of these materials in a practical hydrogen sorption environment. Those studies are not currently included in the future plans. It is strongly recommended that they be incorporated as part of the ongoing work.
- There appears to be little need for continuing or extending the theoretical aspects of this project until some MOF compounds with previously predicted multiple hydrogen adsorption on single metal sites have been synthesized and are characterized with hydrogen capacity measurements at NREL along with neutron scattering, NMR, and DRIFTS evaluations. Assuming the DRIFTS instrumentation becomes operational at LBNL, it should also be used to investigate suitable samples from HyMARC and other DOE-funded researchers.
- The recommendation for this project scope is to increase the activities in conjunction with the Characterization and Validation team or allow it to be a materials development project, but it is not clear whether the project is identified as part of the team itself.

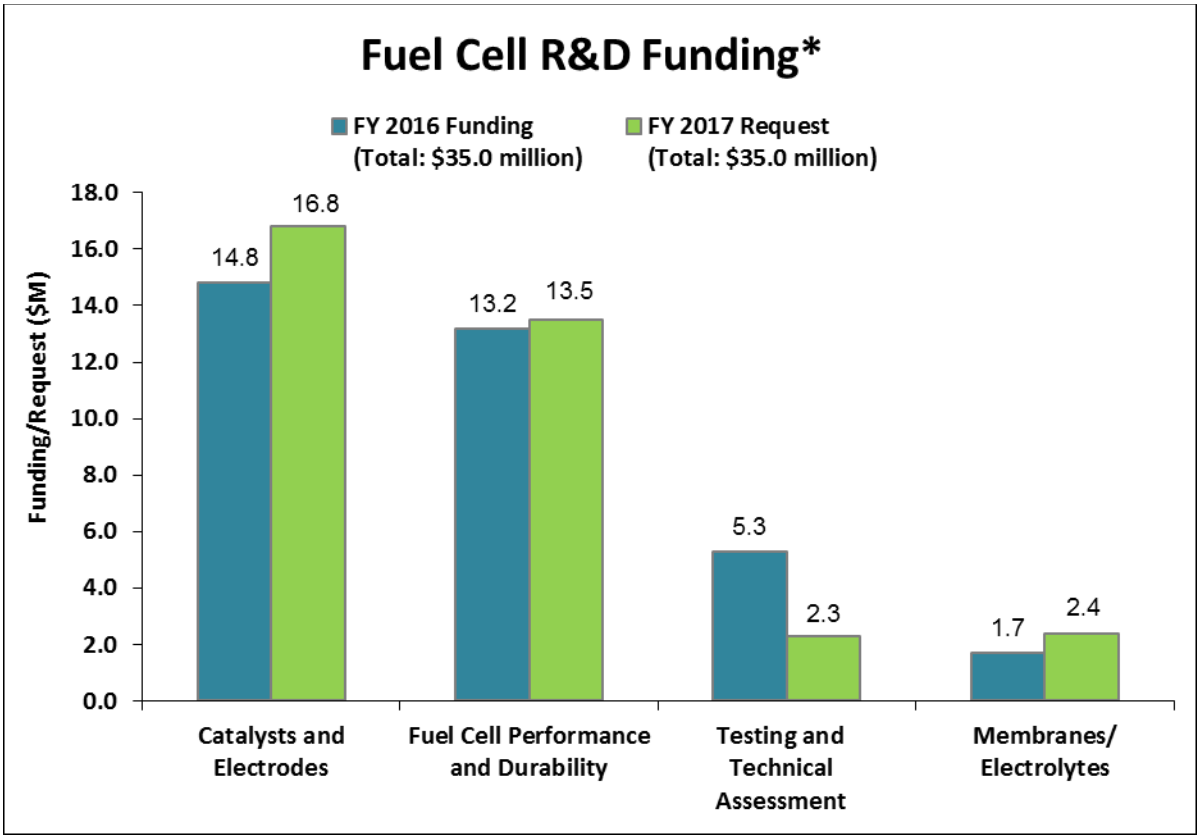
2016 — Fuel Cells
Summary of Annual Merit Review of the Fuel Cells Program

Summary of Reviewer Comments on the Fuel Cells Program:

Reviewers felt that there was a good balance between near-, mid-, and long-term research and development (R&D) in the Fuel Cells program, and they agreed that cost and durability are the major technical challenges. Reviewers praised the program's approach to identifying and addressing these issues and noted its well-structured, focused, and well-managed projects as a strength. In particular, the consortia established by the program were lauded for their potential to transform fuel cell technology. However, some reviewers commented that progress on fuel cell cost reduction has stagnated in the last few years. Key recommendations include increasing focus on technologies that will build on progress achieved thus far, decreasing emphasis on alkaline fuel cell technology, focusing on developing better transport properties for platinum-group-metal (PGM)-free catalysts, and establishing clear and ambitious go/no-go criteria to allow for ending projects not meeting these criteria. Also, one reviewer suggested setting aside a portion of each year's appropriation to support smaller projects, with particular encouragement given to new applicants.

Fuel Cells Funding:

The program received $35 million in fiscal year (FY) 2016. The request for FY 2017 is $35 million. The program focuses on reducing fuel cell costs and improving durability. Efforts include approaches that will achieve increased activity and utilization of low-PGM catalysts, PGM-free 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 advanced fuel cell performance and durability by addressing mass transport and degradation issues. The FY 2016 funding opportunity announcement will result in funding for new fuel cell performance and durability, as well as catalyst and electrode projects. There is no funding in FY 2016 or planned funding in FY 2017 for balance-of-plant (BOP) component projects.



* 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:

At this year's review, 48 projects funded by the Fuel Cells program were presented, and 33 were reviewed. Projects were reviewed by between four and eight reviewers, with a median of six experts reviewing each project. Reviewer scores for these projects ranged from 2.6 to 3.5, with an average score of 3.1. This year's highest score of 3.5 and average score of 3.1 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 2016 was a modest increase from 2015's low score of 2.5.

Catalysts and Electrodes: The scores for the nine catalyst projects ranged from 2.7 to 3.2, with an average of 3.0. Reviewers praised the highest-rated project for the progress the project has made in improving durability over its lifetime and the diversity and quality of its team members. However, reviewers commented that the project team had paid inadequate attention to the engineering of thick catalyst layers that resulted in inadequate transport properties. For the lowest-scoring projects, reviewers noted that the project teams had failed to address fundamental barriers in the technology. In one case, this was the over-reliance on PGM catalysts, and in the other, the technical problems with metal supports.

Fuel Cell Performance and Durability: Six projects, all part of the Fuel Cell Performance and Durability (FC-PAD) consortium, including the consortium overview, were reviewed. Three projects received a high score of 3.3, two projects received a low score of 3.1, and the remaining project received a score of 3.2, for an average score of 3.2 for FC-PAD overall. Reviewers praised the highest-rated projects for the relevance of their focus on catalyst and support durability, the strength of the teams and their access to a large number of characterization tools, and the design of their approaches. However, reviewers noted that the projects would face challenges without fostering stronger collaborations with suppliers, other DOE-funded projects, and original equipment manufacturers. Reviewers felt that the lower-scoring projects demonstrated strong project teams and that their approaches were reasonable, but suggested that the projects shift focus to a foundational understanding of degradation causes and novel fuel cell testing techniques.

Testing and Technical Assessment: Eight projects were reviewed and received scores between 2.7 and 3.4, with an average score of 3.2. Reviewers lauded both the highest-rated projects for their collaborations across the program and with industry, as well as for their focus on addressing specific problems and answering specific questions raised to the program. Reviewers commented that the lowest-rated project's reliance on nanostructured thin film (NSTF) systems was a weakness and recommended that DOE reconsider its focus on NSTF catalyst systems.

MEAs, Cells, and Other Stack Components: Four projects were reviewed in this area, with one project receiving a high score of 3.1, two projects receiving a low score of 2.7, and the remaining project receiving a score of 2.9, for an average score of 2.9. Reviewers felt the highest-rated project's systematic approach and excellent collaboration resulted in an impressive amount of data and helped identify several challenges confronting de-alloyed Pt-based oxygen reduction reaction (ORR) catalysts. For the lowest-scoring projects, reviewers were unimpressed with the accomplishments of each project and with the reasoning behind the approach for each project. They commented that the first project continued to work with a catalyst system, NSTF, that may be reaching a limit of diminishing returns without a major reworking of the system; and that the second project had selected, in perovskites, a class of materials that is well known and had failed to develop the selected materials or to demonstrate a path to improvement.

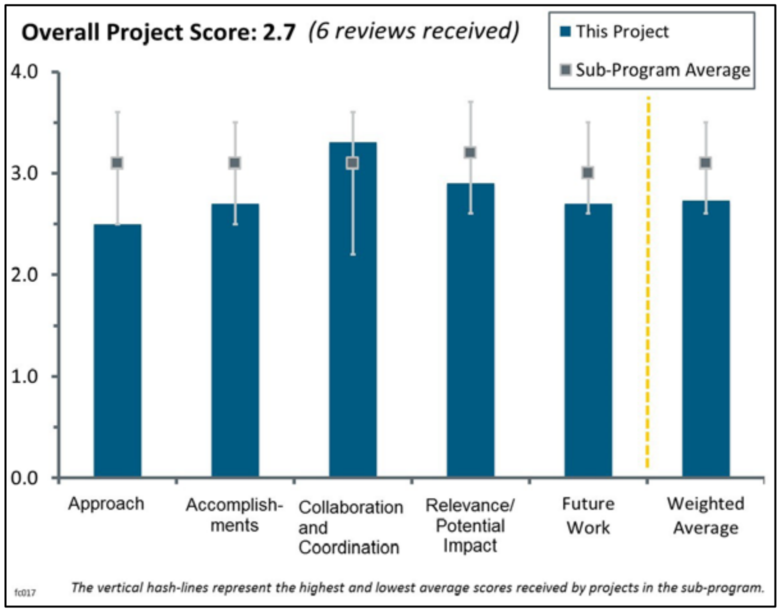
Membranes/Electrolytes: The six membrane projects reviewed received scores between 2.6 and 3.5, with an average score of 3.2. The highest- and lowest-rated projects in this topic area were the highest- and lowest-rated projects in the program. Reviewers were impressed with every aspect of the highest-rated project and found that the excellent team, with good coordination and a valid and novel approach, had led to quantifiable progress toward meeting DOE's membrane targets simultaneously with a single system. They recommended that the project proceed by concentrating on the viability of the perfluoro imide acid (PFIA) side chain. Reviewers were impressed with the potential of stable phosphonium-based anion exchange membranes and the approach the lowest-rated project team had taken, but they were concerned that the degradation tests used are not the most accurate. In addition, reviewers expressed concern about the results achieved, particularly with respect to conductivity and stability.

Project #FC-017: Fuel Cells Systems Analysis

Rajesh Ahluwalia; Argonne National Laboratory

Brief Summary of Project:

The overall objective of this project is to develop a validated system model and use it to assess design-point, part-load, and dynamic performance of automotive and stationary fuel cell systems. Argonne National Laboratory (ANL) will support the U.S. Department of Energy (DOE) in (1) setting technical targets and directing component development, (2) establishing metrics for gauging progress of research and development (R&D) projects, and (3) providing data and specifications to DOE projects on high-volume manufacturing cost estimation.



Question 1: Approach to performing the work

This project was rated **2.5** for its approach.

- The approach is good. ANL develops models and applies them to issues of current interest, as advised by the U.S. DRIVE Partnership technical teams and by DOE. ANL also validates the models with laboratory data. The focus in fiscal year (FY) 2016 seems to be very 3M-centric, as outlined by the four quarterly progress measures and milestones.
- ANL uses a balanced and proper approach combining detailed models and data validation. ANL is encouraged to continue with the same modeling/validation approach with the new state-of-the-art materials as they become available through the Fuel Cell Consortium for Performance and Durability (FC-PAD).
- The overall approach was satisfactory. On slide 4, it was not clear which tasks DOE requested for analysis. The way the slide was presented, it could be assumed that DOE requested all of these tasks.
- ANL has reverted to basing stack performance on a 3M nanostructured thin film (NSTF) catalyst, in spite of the lack of any evidence that 3M NSTF has been found applicable for a realistic automotive fuel cell system. The choice of 3M NSTF as the catalyst may have led to a detour into the degradation mechanics of this catalyst that would not have been necessary had a dispersed catalyst system been chosen for the project.
- In general, the project is very dependent on the remainder of the DOE portfolio. While it is understandable that DOE would be fairly selective in choosing catalyst and balance-of-plant (BOP) projects that may provide an opportunity to find breakthroughs in technology, it is not understandable that the system analysis project would be confined to more experimental component sets, especially given the role that the system analysis project plays in establishing the existing status for fuel cell technology. It is not clear that the NSTF catalyst represents the status of the technology, nor that the Roots compressor represents the status of the technology. Regarding the NSTF catalyst, the answer is no, it does not represent the status of the technology. Regarding the Roots compressor, better air machines can probably be found.
- The modeling approach is semi-empirical and may be missing some elements of being predictive for fuel cell performance. Heavy dependence on data for tuning/calibrating the model might be limiting and may not be applicable for novel material sets. Further, the project has been primarily focused on NSTF and limiting the operating window to the optimum for NSTF (e.g., forklift application at a super wet ~80% relative humidity [RH] *in* and 140% RH *out*), thus ignoring most commonly used operating conditions.
- The approach is dominated by the 3M catalyst materials. The latest design is to put a layer of NSTF catalyst on top of a platinum-containing interlayer that could by itself be considered an electrode. Perhaps

this is because of the failure of the NSTF catalyst to adequately promote the removal of product water. It is not clear what the difference is between a de-alloyed catalyst (such as the 3M NSTF Pt_3Ni_7) and a catalyst that is not ordered (such as PtNi). The Fuel Cell Technologies Office (FCTO) should evaluate its commitment to the 3M NSTF catalyst and report what fuel cell organizations (transportation or otherwise) are using the NSTF catalyst after over 10 years of FCTO support.

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.

- There was a good deal of technical progress in FY 2016. The models are especially useful for predicting performance and durability in stacks, as durability experiments are time-consuming and very costly. Accomplishments and progress were many and range widely, from membranes to catalysis; from air management to fuel management to thermal management to water management; and from stack performance and durability to system cost.
- ANL consistently follows through on its planned work and does a thorough job of capturing the system status. The correlation of NSTF degradation with fluoride emission rate (FER) is an interesting insight, but it is not clear that it has been corroborated. More such evaluations are encouraged, as it is nice to see ANL attempt to move the bar (progress), not just measure the height of the bar (status). However, it would be good to see more focus on dispersed catalysts.
- Accomplishments seem to be primarily focused on understanding the impact of FERs on NSTF irreversible degradation. Such correlations have been previously published, and it looks like a mere correlation rather than FERs being the cause. There has been good progress on the bipolar plate contact resistance coordination activity.
- Data for irreversible degradation of NSTF appear inconsistent. Voltage loss rates for 0.6 V, 90°C in the V Series do not match the rates for 0.6 V, 90°C in the T Series. There is no indication that the model accounts for hydrogen concentration on the anode. To explore why differential cell models do not match load-following cell results, the investigators may want to look at water balances and proton resistivities (perhaps with proton pumping for NSTF, if possible) for both types of cells. Perhaps the differential cell does not allow water generated to escape toward the anode because of the short flow path.
- The principal investigator claims that "Optimal power determined by high frequency resistance and oxygen reduction reaction activity rather than mass transport overpotentials." However, there is at least a 10 mV difference at 2 A/cm^2 for the model including mass transport versus a model without. It is assumed that the model including mass transport was used in calculating stack and system costs, which would make it strange to think that a mass transport effect at least as large as the cell-to-stack voltage loss does not make a difference. Results from this past year differ from last year in one key respect: system cost now appears to go up at air inlet pressures greater than 2.5 atm. Because stack cost goes down at higher pressure, it appears that the system cost increase directly relates to the cost of the air machine.
    - It is hard to tell what the accomplishments were. During the presentation, it was very difficult to follow the data explanations because of the extensive use of acronyms and abbreviations. SRc was not defined, nor was SR(c)—perhaps they are the same—both were used. There was no statement identifying federal urban driving schedules (FUDS) and federal highway driving schedules (FHDS). It was not clear whether the cost correlations from Strategic Analysis, Inc. (SA) are at production rate of 500,000 units per year or less, or how valid SA's projected cost correlations are. It is not certain that industry has agreed with these values down to the tenth of a dollar.
    - The Summary of Technical Accomplishments slide does not make clear what these terms refer to: "results #1," "results #2," "results #3," and "results #4." A search of the presentation for the word "results" revealed it only on the Summary slide or when referring to "modeling results." It is not clear whether slides 6 through 11 consider only PtCoMn/NSTF on an 850 equivalent weight (EW) perfluorosulfonic acid (PFSA) membrane. The reviewer looked for labels on slides but remains unsure what catalyst was used because Pt_3Ni_7/NSTF was discussed before slide 6.
    - The term TC was identified after being used several times as thermal conditioning, not thermocouple, which was confusing. The thermal conditioning cycles were not defined.
    - It is not clear whether a supported membrane is the same as a reinforced membrane, and if so, why two different terms were used. It is not clear whether PtCoMn/NSTF was operated with a cathode

interlayer (CI) or was tested on a supported membrane (725 EW PFSA). It is not clear how direct comparison is made with Pt_3Ni_7/NSTF and 850 EW PFSA.
  - The hold potential for potentiostatic tests on slide 9 is unknown. Experimental conditions were not fully stated. It is not clear whether the CI replaces the gas diffusion layer or whether both are present, nor what exactly the CI does. It is not clear whether the improvement using d-Pt_3/Ni_7 + CI with 725 EW membrane is due to the 30% reduction in thickness of the membrane (725 compared to 850) or to the change in EW.
- On slide 18, it is not clear whether all of the Pt_3Ni_7 electrodes have a CI while the $Pt_{68}(CoMn)_{32}$ does not. It is not clear how the effect of the CI could be isolated.
- The presentation was much too technical. The information was dense, and many of the acronyms were not defined, which made it very difficult, if not impossible, for a layman to understand. The researchers need to distill out the essence of the results and then explain it in plain English so that a layman who is not an expert in the field can understand.

Question 3: Collaboration and coordination with other institutions

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

- The project has excellent collaboration, partners, and interactions with industry, university, working groups, and technical teams, domestically and internationally.
- There was a high degree of collaboration with multiple suppliers in this project. The work appears to be exclusively focused on transportation applications, although the overall objective includes stationary applications.
- There are many good interactions.
- ANL has had to collaborate with other DOE investigators (e.g., 3M and Eaton) to gather information for modeling stack and air machine performance. The project relies on these collaborations as well as those with SA for cost modeling. This project plays a key role in defining what DOE recognizes as the status for automotive and bus fuel cell technology.
- Ideally, ANL should be able to explore technology outside what DOE has funded. There is very little connection thus far between the system being analyzed in this project and the systems that are being deployed in vehicles. It would make sense to expand collaboration to understand systems that are on the road. It would be good to see more than one collaborator for certain components, similar to what is already being done with 3M and Johnson Matthey on membrane electrode assemblies (MEAs). It would be preferred to see more than one air machine collaborator, more than one anode subsystem collaborator, and multiple collaborators on other components.
- The project maintains a strong collaboration effort with outside partners, though Oak Ridge National Laboratory's diagnostic/characterization laboratory is not mentioned. Perhaps this project's modeling efforts can shed light on results coming from that laboratory (Karren More's project) or from National Institute of Standards and Technology data. Project integration with the FC-PAD projects and deliverables should be a key focus in the upcoming years.
- ANL is working with several collaborators, and the interaction is obviously generating useful information. It is not clear whether the interaction with SA is a collaboration or ANL is just receiving data from SA. It is not clear how SA contributes to a life-cycle cost—whether SA estimates a beginning-of-life cost or calculates the recovery cost at end of life.

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's annual status determination is a key output of this (and SA's) project and the Hydrogen and Fuel Cells Program (the Program) and, as such, rates an "excellent." The project meets its purpose as an independent assessment well. However, it is of lesser benefit to original equipment manufacturers/

developers. The Program should continue to seek out areas where it can do more of the latter (durability studies, mass transport limitations, etc.).
- This is the primary source of performance modeling for fuel cells. The project should publish the experimental test matrix and the corresponding NSTF results through the FC-PAD website to increase impact.
- Funding of $550,000 seems high for modeling work. The project impact is broad, ranging from quantifying the source of decrease in power density and increase in cost due to heat rejection constraint, identifying the dominant NSTF catalyst degradation mechanism and determining the upper limit of cumulative fluoride release (CFR) to meet the durability target, projecting increase in power density and decrease in cost by reducing the anode Pt loading and replacing cathode catalyst type, and using a thinner membrane with a lower EW.
- There are so many fuel cell designs that are evolving that it is difficult to keep abreast of all of them. If the areas of evaluation were specified by DOE, then the project is well focused on those specific tasks.
- Automakers that are seeking to commercialize fuel cell electric vehicles will not rely on this study to do so. This study is most relevant to DOE's own ability to assess the status of fuel cell technology and to be able to assign a research portfolio accordingly. The project might be more relevant to automakers if it were able to begin answering questions that SA still has regarding bipolar plate welding time and length, inspection rates, and other plate-related issues.
- It would be good to be able to say the relevance/potential was outstanding; however, a high confidence level in this project remains in question based on the information provided in the presentation.

Question 5: Proposed future work

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

- The field is so diverse that it is very difficult to determine what the follow-on priorities should be. It would be interesting to understand *why* the new tasks are being undertaken. The proposed future work on bus systems is important and welcome.
- Future work on catalysts should include PtCo. This is especially true given the usage of PtCo in the Toyota Mirai and the new General Motors project focusing on PtCo. Air machine studies need to be expanded beyond just Eaton. Future work needs a much greater emphasis on bipolar plates. If the NSTF work is to go forward, the future work should focus on the gaps between the differential cell model and the load-following cell data.
- Proposed future work seems too broad. The value of continuing to work on NSTF is uncertain, given the problems with scaling up this solution. Near-term focus (2016 toward the Program's 2017 Annual Merit Review) should be d-PtNi/C and also any other state-of-the-art MEAs through FC-PAD. Also, the project should ensure the entire dataset is available through the FC-PAD website.
- The project should place a higher priority/emphasis on dispersed (non-NSTF) catalysts. Project integration with the FC-PAD projects and deliverables should be a key focus in the upcoming years.
- The proposed future work should improve understanding and development of advanced fuel cell systems.

Project strengths:

- ANL has been responsive in the past to feedback to add parametric studies, to add BOP equipment most relevant at low current density, and to add thermal restrictions. ANL does use a performance map to look at voltage response over a wide range of operating conditions. ANL has improved its coordination with the SA cost model over the past year.
- Modeling work is relevant and impactful, and progress seems to cover a wide range of topics. Modeling can be a cost-effective way to predict stack and system performance and durability, if done correctly, and can accurately predict these phenomena.
- The technical resources and expertise that have been brought to bear on evaluating the various tasks are a significant strength of this project. The key is to select tasks that advance the overall progress of the industry.
- Strengths include deep modeling capabilities and integration with DOE projects (with good access to those data).

- There is a solid model and a good team.
- The presenter and the ANL team are all well established, experienced researchers. The quality of the presentation was very disappointing.

Project weaknesses:

- In the past, access to state-of-the-art materials not associated with other DOE projects has been an issue (as it has been with the other DOE projects). It is hoped that this improves with the formation of FC-PAD and its emphasis on state-of-the-art materials access.
- The project lacks manufacturing details that would assist in understanding cost minimization for many BOP components and for plates. The project is basing its analysis on a catalyst system that is not known to be used in existing vehicles. ANL rarely seeks out more than one supplier for a given component. This means that the project misses numerous opportunities either to explore the actual status of the technology or to learn how cost can be minimized versus the existing status. ANL can sometimes miss on critical system assumptions. One example is the assumption of constant pressure drop from compressor outlet to expander inlet.
- The FCTO commitment to 3M's NSTF should be questioned. The commitment was valid 10 years ago; however, more recent data generated by FCTO projects suggest a re-evaluation of the benefits of NSTF should be done. It is hard to think of any other catalyst system for PEM that has received as much funding over a long period with so little industry acceptance.
- The results that were presented were not presented in a form that was understandable to a layman. The report needs to extract the essence of the results and reduce them to simple English so that a person who is not an expert in the field can understand what is being reported. A single slide is needed that identifies all the acronyms used. The lack of acronym definitions was very frustrating.
- The choice of NSTF is a weakness.
- The project cost seems high.

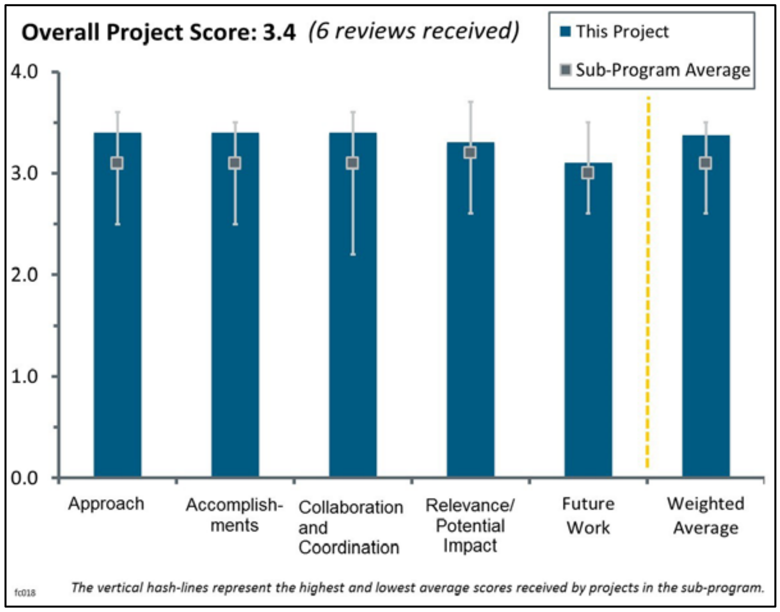
Recommendations for additions/deletions to project scope:

- The scope needs to be expanded to include suppliers outside of what DOE is funding or has funded. It would be good to survey multiple suppliers per component. The scope needs to expand greatly on understanding bipolar plates. This includes how plates are made as well as how plates affect performance. The project needs to focus on what system status actually is, so long as status is being used for cost modeling. A dispersed catalyst layer needs to be the basis for the analysis, regardless of the cost consequences. Analysis of NSTF durability belongs in a 3M project, not in a system analysis project. If the particular failure modes associated with NSTF are not an issue for catalyst layers that are actually in practice, then having a system analysis project dive into this detail is counterproductive.
- The project should resolve whether the performance enhancement of d-Pt_3Ni_7 + CI over $Pt_{68}(CoMn)_{32}$ is the result of the CI. The project should resolve whether the improvement in power density of d-Pt_3Ni_7 + CI with the 725 EW membrane compared to the d-PT_3Ni_7 + CI with the 850 EW membrane is due to the difference in membrane thickness (~40%) or difference in EW.
- There should be greater emphasis on modeling and understanding a dispersed catalyst, with less priority on NSTF. Regarding the anode gas recirculation, the project should investigate the physics and impact of pulsed ejector flow.
- NSTF should be deleted from future work (2016 and 2017). The project should figure out a way to make the test matrix and datasets available online.
- It is not clear how tasks are defined. Perhaps they are a result of a formal meeting between DOE and the laboratory to develop the next set of tasks based on the needs of industry. An annual review should take place to clearly define priorities.

Project #FC-018: Fuel Cell Vehicle and Bus Cost Analysis

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:



The objectives of this project are to (1) project a future cost of automotive and bus fuel cell systems at high manufacturing rates; (2) identify low-cost pathways to achieve the U.S. Department of Energy (DOE) 2020 goal of $\$40/kW_{net}$ (automotive) at 500,000 systems per year; (3) focus on low-volume production (1,000–5,000 systems/year) and near-term applications; (4) identify fuel cell system cost drivers to facilitate Fuel Cell Technologies Office (FCTO) programmatic decisions; and (5) identify impacts of technology improvements on system cost.

Question 1: Approach to performing the work

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

- The project takes a good approach to providing a cost analysis using the Design for Manufacture and Assembly (DFMA) method. The benchmarking against the Toyota Mirai was very useful. The feedback from the stakeholders is very valuable and helps with the validity of this approach.
- The project has a well-documented and reasonably wide scope of possible alternate methods for lower cost fabrication, particularly with respect to catalyst and bipolar plates (for the former, the project shifted to a lower cost approach and for the latter, the team recognized that prior estimates may have been low). The project team displayed a reasonable recognition of practical limitations in some instances.
- The project takes a solid approach in cost modeling and is very good at reaching out to collaborators and seeking the most relevant information. Some cost models seem way too optimistic (e.g., for the gas diffusion layer [GDL], but new results to understand sensitivity are encouraging to see). The addition of the anode recirculation pump has added cost, which may not be the most optimal use of money.
- The project continues to adjust to changes in the market and needs as in the case of the benchmarking against the new production vehicles.
- Strategic Analysis, Inc. (SA) uses established costing and scale-up methods to provide annual updates. SA is intimately in contact with stakeholders—original equipment manufacturers (OEMs), raw materials and component suppliers, equipment suppliers, laboratories, end users, systems analyzers, stack and systems integrators—to ensure the validity and timeliness of materials, processing, and operating conditions assumptions. SA uses DFMA techniques, vendor quotes, supplier estimates, and user experience to estimate costs bottom-up including raw materials and manufacturing equipment (both type and quantity).
- SA's DFMA approach to the cost status evaluation and their interaction with the fuel cell stakeholders is proper and adequate in addressing the project goals. It is unclear how the low-volume cost efforts were approached. Specifically, it is unclear if the processes were appropriately modified or optimized for each volume. The final report should list detailed assumptions for processes and their respective costs for each major operation and component.

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.

- This project has made good progress with collaborators in seeking to understand the impact of dealloyed catalysts on total system cost. The cost increases from reconsideration of various stack components show how frequent evaluations of each component is important. The benchmarking against the Mirai was very interesting although the cost analysis appears to be more subjective than objective due to a lack of publicly available information.
- The project has made good progress in modeling the cost of competitive vehicles (e.g., Mirai); however, near-term solutions for commercialization cannot be used to change the long-term goals (e.g., ARP, cell voltage monitor).
- The project was well-organized and presented a summary of alternatives. Slide 13 is particularly useful in explaining the basis for change. Note that rate of change of costs is lower than needed to achieve DOE goals.
- An updated cost analysis has been provided for the car and bus fuel cell systems. The benchmarking exercise against the Mirai showed very good results in terms of the accuracy of their projection. The risk identification exercise in terms of industry manufacturing readiness level (MRL) and fuel cell MRL is also very valuable for DOE.
- The team has been responsive to DOE’s requested deliverables. The timely incorporation of Argonne National Laboratory’s (ANL’s) power density model results, which are key to the annual cost status projection, is appreciated. The sensor cost update (listed in 2015 future work) does not seem to have been covered. This may be an area where cost estimates are too high.
- SA is responsive to DOE and industry needs as the technology evolves. Alternative technology scenarios are run in a timely manner. Although SA does not develop or demonstrate technology, they identify where costs need to be addressed thus helping to guide technology development priorities.

Question 3: Collaboration and coordination with other institutions

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

- The partners have done an excellent job in engaging the industry (over 30 companies) and receiving manufacturing inputs, which is no easy task. The engagement of Toyota was a very positive feature in this project.
- SA is in constant contact with stakeholders (especially systems analysis stakeholders) to ensure that the latest materials and process technologies are incorporated into the analysis. SA performs many “side jobs” and trade-off studies to evaluate specific component costs and the impact of novel technologies.
- The project has achieved strong, ongoing engagement from several industrial collaborators.
- The accuracy of the cost model depends heavily on the performance model from ANL (FC-017) and hence, there needs to be a close collaboration between these two teams. There is a need for collaboration with non-U.S. suppliers (e.g., for GDL, plate coatings) to understand best-in-class processes for cost reduction.
- SA appropriately works with ANL’s systems analysis project and collaborates with many of the stakeholders to attempt to evaluate a representative status of the technology.
- Contact Edison Welding Institute in Columbus, Ohio, for additional input on the bipolar plate welding process. Contact Oak Ridge National Laboratory for development work on carbon fibers that may be applicable to GDL. GDL remains a significant cost item for all sizes of polymer electrolyte membrane fuel cells—any progress in cost reduction would be appreciated.

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 very relevant to DOE Hydrogen and Fuel Cells Program (the Program) and addresses the cost related barriers very well. The project can be very helpful to decide how to prioritize research areas and to highlight the areas in need of attention.
- The relevance and impact of the project are high. SA's costing work helps guide the Program by identifying high-cost areas and evaluating technology options.
- This project is the single source of information for projected cost for fuel cells. Therefore, the project is highly relevant.
- This well-documented study provides the basis for understanding cost drivers at high production rates. The path to get from current production rates to high production rates is a bit unclear and the confidence level that high production rate costs can be achieved is not clearly defined.
- By its nature and charter, the project does not advance the progress towards the goal, but the project is intended to give a cost status. In that effort, the project does a good job. Adding a section highlighting areas, components, and processes that can be improved (in a pre-competitive environment) to enhance cost-affordability would be helpful. To that end, the presentations should incorporate a listing of major component and operation (e.g., stack assembly) costs (in dollars, not just dollars per kilowatt) in table form so that the project end-users and reviewers can more clearly see what items are major cost-contributors, and how they change from year to year.
- More targeted direction or feedback, such as the suggestion for a roll-to-roll bipolar plate manufacturing process, will further increase the impact of this work.

Question 5: Proposed future work

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

- The proposed future work is good. It might be good to add details about which balance-of-plant (BOP) costs are driving the overall system cost, and how what type of work at the stack level can bring the BOP costs to less than $\$15/kW_{net}$.
- The future work is dependent on changes in the technology status. SA quickly responds to alternative scenarios. Trade-off analyses of alternatives will continue (e.g., ejector versus hydrogen recirculation blower). An annual update will be performed.
- The future work is not ambitious but it is in line with project priorities. The final report should detail the following: detailed costs versus components and operations; main system parameters (as shown on slide 37) versus year; and for the different volumes, key process assumptions.
- The proposed future work is very relevant and will be useful for the project's results.
- The proposed future work appears to be appropriate.
- The project ends in three months; the future work is primarily documentation.

Project strengths:

- The project did very well to reduce the wide range in estimates of GDL and bipolar plate costs, identify forces driving cost up, and make suggestions for directions of future efforts for further cost savings.
- The team has a long history with the project and has demonstrated the capability to perform the task. The team also works well with ANL (Systems Analysis) and OEMs to provide the input assumptions, which are vital to a fair estimation of the cost status.
- The solid analysis using the DFMA framework is a project strength. The project has very good collaborations with industry.
- The project has excellent communication with and responsiveness to stakeholders, especially DOE.
- The project has well documented and refined cost estimates as well as good analysis.

Project weaknesses:

- There are no notable weaknesses. The project benefits from a solid history allowing improvements each year as new technology and fabrication methods develop or are evaluated.
- There are no strong weaknesses.
- The project is limited to what information is public and what information the developers are willing to contribute (confidential or otherwise) and thus, one must accept that there are significant uncertainties in any such cost status projection. But that is to be expected.
- Overcoming the inherent subjective nature of some of the analyses continues to be a challenge of the project.
- The project relies too much on nanostructured thin-film catalysts.

Recommendations for additions/deletions to project scope:

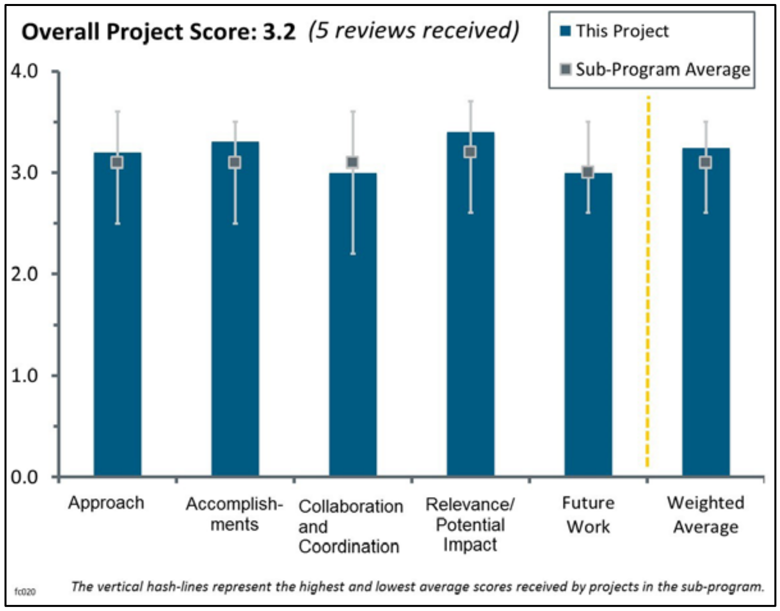
- Continue to monitor developments including the Mirai and other fuel cell electric vehicles.
- Consider the cost of state-of-the-art membrane electrode assemblies (MEAs) at low volume production (around 1,000 systems per year). The components in the imaginary system are all using low cost materials without consideration for any interactions due to integrating these components. It would be beneficial to model the cost of state-of-the-art MEAs along with some cell configurations as tested.
- If continued evaluations of cost at low volume are planned, it would be of benefit to show where processes were volume-optimized including the impacts of capital, labor, quality control, etc.
- DOE should note that it might be informative to compare all of the costing projects and identify major differences.

Project #FC-020: New Fuel Cell Materials: Characterization and Method Development

Karren More; Oak Ridge National Laboratory

Brief Summary of Project:

The objectives of this project are to (1) identify and develop novel high-resolution imaging and compositional/chemical analysis techniques and unique specimen preparation methodologies for the micro- to Angstrom-scale characterization of materials comprising fuel cell membrane electrode assemblies (MEAs); (2) optimize imaging/spectroscopy methodologies toward specific fuel cell materials, including electrocatalyst atomic-scale structure and chemistry, ionomer mapping in catalyst layers, and three-dimensional (3-D) electron tomography; and (3) share unique capabilities and expertise with fuel cell researchers outside of Oak Ridge National Laboratory (ORNL).



Question 1: Approach to performing the work

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

- The use of advanced electron microscopy and four-dimensional tomography to elucidate the atomic-scale structure of electrocatalysts and for mapping the ionomer contents/coatings on electrocatalysts is a great approach, which the team has taken to help increase understanding of the FC-PAD (Fuel Cell Consortium for Performance and Durability) MEAs sourced from different suppliers. The team's approach of collaborating with the fuel cell community is nice and will help the community to use newly developed advanced analytical techniques and implement these techniques across the industry, hence achieving an actual comparison between the samples.
- This project continues to develop useful advanced characterization techniques and approaches for identifying critical fuel cell MEA materials issues. The principal investigator (PI) identifies a general focus on standards before and after use that is effective for suggesting materials changes that can be connected with changes in performance.
- The extent to which the characterization methods and their advancement are pushing the state of the art (SOA) for fuel cell characterization is difficult to assess. That said, the PI has clearly been in this community for some time and demonstrates an overall knowledge of the current relevant issues for fuel cell development. Evidence for this comes, in part, from continued high ratings at Hydrogen and Fuel Cells Program Annual Merit Reviews (AMRs), as was the case last year. This represents high value for DOE investments in national laboratory capabilities and (importantly) expertise.
- The approach is excellent and contributes to overcoming some key barriers related to having a better fundamental understanding of the MEAs and MEA components.
- The approach to develop new techniques using advanced microscopy is critical for better understanding key MEA components and their role in performance and durability.
- The use of scanning transmission electron microscopy (STEM), scanning transmission x-ray microscopy (STXM), and spectral analysis to distinguish elements in the resulting images has become more widely known over the past few years. Other researchers have been able to make use of the Advanced Light Source at Lawrence Berkeley National Laboratory or the Canadian Light Source to accomplish similar

results in two dimensions. The PI in this project has been able to stay a step ahead by performing analysis in three dimensions. The approach would be enhanced if it were clear that the ultimate goal is to provide similar analysis with a wet sample. Perhaps even better would be to provide an in situ technique, allowing imaging on materials in an extremely small-scale fuel cell.

  - The project occasionally diverges into making presumptions about fuel cell catalyst layers, although the sample being discussed may be just one result from a wide range of possible parameters, including ionomer-to-carbon ratio, ionomer equivalent weight, deposition technique, and ink solvent and composition ratios. With so much work in the system and cost analyses premised on 3M nanostructured thin film (NSTF), it is refreshing (although inconsistent) to see one DOE project that focuses on dispersed catalyst layers.

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 on 3-D imaging of ionomer dispersion in catalyst layers and electrocatalyst distribution in 3-D. These techniques can be very helpful to correlate the MEA performance to catalyst layer (CL) design and optimization.
- As usual, the project has produced new and interesting results that continue to provide valuable insights.
- Because the project started in 2016, not much in the way of technical accomplishments is expected during this short period before the AMR. However, the team has taken a great approach of utilizing its collaborators and past contacts to obtain new and baseline MEA samples for analysis. The team should try to engage some of the mainstream MEA suppliers and automotive companies to obtain their SOA MEAs for FC-PAD analysis. This will help the team to correlate the structure of the commercial MEA to other MEAs.
- Research accomplishments and progress, as assessed by the presentation highlights and publication numbers, are good. Publication numbers of five to six are good (but not outstanding), and the PI is regularly presenting work at national and international meetings (including one prestigious invited presentation). This looks similar to last year's "quantitative" productivity.
  - The results described during the presentation represent substantial new information, and the approaches to characterizing ionomer distributions in catalyst layers seem to be a real advance. On the other hand, the catalyst metal 3-D imaging is applying SOA methods to synthesized fuel cell catalyst materials.
  - It is difficult to assess how the "split" in this project will affect progress in the future. Developing advancements in the methods should be inextricably linked to addressing critical issues in fuel cell catalyst development. It appears this project has effectively done this in the past. Connecting part of the activity directly to the FC-PAD activity is outstanding; it is not clear why the whole activity is not part of FC-PAD. That is, it has been a goal of this project to use the advancing methods on realistic materials via collaborations. Those have been occurring, and, understandably, many of these are with other national laboratory activities where materials are more readily obtained and where the materials issues are clearly identified via various performance changes and/or synthesis processing procedure changes. These types of important collaborations for an activity aimed at advancing SOA characterization methods would be optimum for the community, so, again, FC-PAD seems a most appropriate "umbrella" for the project to ensure that these collaborations become even more productive. Furthermore, FC-PAD, as a consortium, may help with some of the difficulties in getting SOA materials, rather than just having this ORNL characterization project ask for them from industry partners. Therefore, reconnecting this activity with the "split-out" FC-PAD one is recommended. Also, in this way, it would avoid potential confusion in trying to assess which part of the methods development and application work should be attributed to which part of the activity.
- What has been accomplished has been the development of 3-D imaging and spectral analysis for catalyst layers under ex situ, dry conditions in order to understand where ionomer and platinum agglomerates reside. This has been good and can be helpful toward assisting developers in understanding how catalyst layers should appear to extend performance and durability. While the images are impressive, a proper evaluation of the project must keep the barriers to fuel cell commercialization in perspective. Over the past

few years, it has been apparent to nearly all developers that more graphitized carbons can compromise performance as well as resistance to Pt agglomeration. Furthermore, it has also been apparent that high-surface-area carbons allow for Pt deposition within agglomerates, as opposed to Vulcan and graphitized carbons. Repeating these findings does not advance the technology. The project must focus on learning something new about catalyst layers that has not already been uncovered. The results of the ionomer study are very interesting but must be advanced to understand what happens with the ionomer under wet conditions, or under some conditions that might be described as in situ.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- Obviously the project is well coordinated with the FC-PAD team, and it also includes a decent list of external materials suppliers. DOE's 50% cost share offer should help to increase the level of external collaborations.
- Collaborations have been good ones and are poised to be even more productive via the FC-PAD umbrella. That said, it will be interesting to see if the goal of attracting many (perhaps 10) new industry collaborations can be achieved. In many ways, the collaborations via FC-PAD might well be more productive ones. Certainly, critical new information about materials properties should be of interest to industry, but research activities in the consortium may make the most use of these advances, rather than industry directly.
- The collaborators in the project are very broad, encompassing a university, national laboratories, and industry. The cooperative research and development agreement (CRADA) is expected to result in more collaborators and sample suppliers. It seems that more time is needed to observe the benefit of the CRADA and others who may join the team for evaluating their MEA samples.
- Collaboration with partners is not evident from progress shown. Many strong partners are mentioned, but details of collaboration are missing, so it is hard to judge the extent of collaboration. More industry partners and strong collaboration with FC-PAD Thrust Area 2 for CL integration will be helpful to get the meaningful information about various CL designs using these new 3-D techniques.
- The project has historically relied on collaboration. Indeed, the project is limited by the samples provided to it by outside collaborators. However, the list of collaborations this year appears much smaller. The list of partnerships established in the past year is confined to three national laboratories (which are now a given, especially with the emergence of FC-PAD), the nearby University of Tennessee, and two small companies (Ion Power and IRD Fuel Cells). The question then remains as to whether the project is actually engaging developers that are actively trying to move the technology forward with a linkage to vehicle or product application. It would be interesting to know the identity of the partners involved in the CRADAs. The 50% cost share rule may have discouraged some partnerships. It will be interesting to see whether FC-PAD restores ORNL's access to a wider range of developer materials.

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 microscopy techniques developed and demonstrated by ORNL are very relevant to understanding MEA performance and durability. The microscopy procedures developed by ORNL are aligned with DOE's goal to address the barriers of the commercialization of fuel cell technology. The focus of the activities is on better understanding the interaction between different components in the MEA (e.g., catalyst, membrane, catalyst support, and gas diffusion layer) that affects the performance and durability of the MEA. Understanding this interaction is one of the critical challenges in successful commercialization of fuel cells.
- The understanding that can be gained regarding the ionomer dispersion and electrocatalyst distribution can guide the catalyst layer development and optimization using the SOA catalysts.
- Clearly, this work is focused on issues of primary concern to the development of fuel cell MEAs.

- The project certainly is relevant; the only criticism is the PI seems to have opinions about what the community thinks the catalyst layer looks like, when these simple representations (e.g., sphere-shaped pores) are just convenient geometries to utilize for modeling efforts.
- The relevance of atomic-scale characterization to the future commercialization of fuel cell electric vehicles is without question. At present, much of what happens to create catalyst layers is a black art—ionomer, platinum, and pore sizes find themselves distributed in an almost random fashion, given the indirect methods of control. Therefore, advanced materials characterization is necessary to provide feedback as to how both materials and processes have affected the resulting catalyst layer. The big question with regard to relevance is whether it is still necessary for DOE to fund a distinct characterization project in light of (1) the emergence of FC-PAD and (2) the wider availability of techniques through other U.S. national laboratories or through laboratories in Canada, Japan, Korea, or Europe. Unless the project demonstrates an ability to stay a few steps ahead of the work that is being done elsewhere, it is difficult to say that a distinct characterization project is relevant to eliminating the barriers to fuel cell commercialization.

Question 5: Proposed future work

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

- The proposed research by the team is logical and fits with the nature of the project. As proposed, the team should work with academia and industry to optimize the catalyst-support-ionomer structure and establish correlations between them. The team should also try to determine how this correlation impacts the performance and durability of the MEA when the catalyst is changed while keeping the support (e.g., carbon) and ionomer (e.g., Nafion®) constant in the construction of the MEA.
- Future work plans are most appropriate, although it is unclear whether the goal to establish many new collaborations is practically achievable, or even advisable. A strong connection of this activity within the scope of a productive FC-PAD umbrella, in which this project's goals are highly connected to other research goals of the consortium, seems like an optimum approach.
- Future work highlights the challenge of getting SOA materials from the industry, which is a very valid concern. However, many more studies can still be done on catalyst ink characterization and correlating that to catalyst layers. Catalyst ink is still considered as black art, so these new techniques can surely help ink optimization.
- The future work slide lacks any discussion of how materials characterization techniques will be improved to obtain even more sophisticated quantification of catalyst layers than what already exists. While the National Institute of Standards and Technology neutron-imaging project focuses heavily on what needs to be done to obtain better spatial and temporal resolution, this project is not as focused on what could be done to make its own characterization techniques better in the future. The optimization of fuel cell catalyst layers should be understood as something that catalyst-coated membrane suppliers or even automakers are attempting to accomplish. This project provides the feedback loop between performance and durability results and the processing improvements needed to make improved catalyst layers. However, this project should not assume the optimization responsibility itself.

Project strengths:

- The team's instrumental capability, established scientific network, and vast experience in MEA analysis are certainly assets. With such a combination, the team is well positioned in the forefront of the technology and capable of conducting world-class research.
- The project has been able to facilitate 3-D imaging of a dry catalyst layer ex situ. The project has access to considerable resources for characterization within the national laboratory system. The project has been responsive to past requests to image different types of carbon, as well as to image ionomers in the catalyst layer.
- ORNL and the PI's capability are the main strengths of the project.

Project weaknesses:

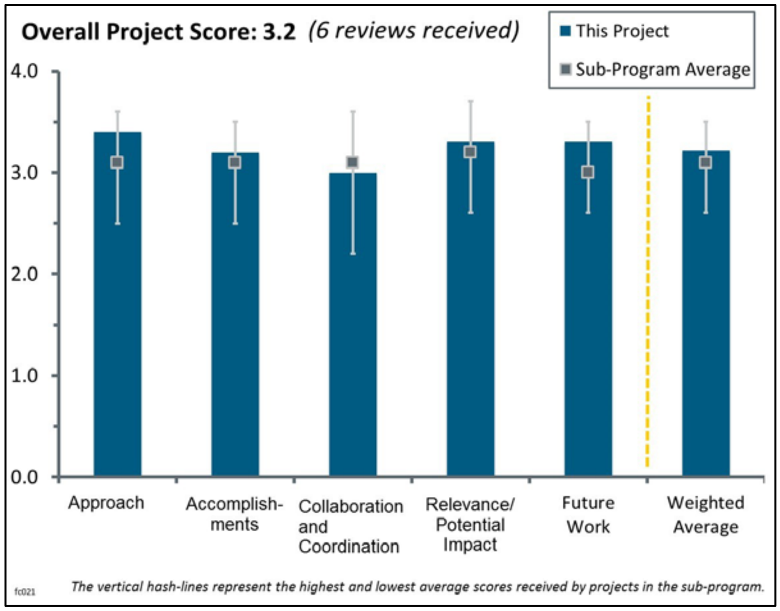
- Lack of SOA materials is a weakness, but that is highlighted by the PI, and one hopes the team will get good support from industry stakeholders.
- The team needs to find a suitable way to obtain MEA samples from automotive and commercial MEA companies for comparison. It is understandable that most of these companies are hesitant to share their SOA MEAs for outside evaluation and publication of those results. With all its resources and knowledge base, the team should encourage these companies to be a part of this endeavor and determine a pathway to share the analytical results with the fuel cell community while respecting the intellectual property sensitivity of the companies.
- A potential weakness may be the split of this project into an ORNL-only activity and an activity carried out as part of FC-PAD. This split might create an artificial and inefficient "barrier" between the actions carried out by these two activities. It will probably also make it very difficult to assess progress in future AMR reviews (in fact, it already has; it is not clear whether this project represented all of the activities carried out this past year or how the proposed future work might be different and distinct from the PI's FC-PAD activities).
- The project needs to direct its focus toward improvement of characterization techniques, not just application, to stay ahead of other facilities worldwide. Being able to go further toward an understanding of how catalyst layers behave under wet conditions, or even in environments that would represent in situ conditions, may be beyond this project's capabilities. The collaborations appear to be limited to the national laboratory network, a nearby university, and a few small companies. The project needs to re-expand the collaboration network to include entities directly involved with commercializing fuel cell technology.

Recommendations for additions/deletions to project scope:

- Overall, the project looks good. No further additions/deletions are needed.
- Some focus on ink-to-catalyst-layer correlation would be great.
- To ensure productive and efficient progress in the future, the Program and ORNL might want to reconsider the decision to create two separate activities out of this project.
- The project scope should be directed toward imaging catalyst layers under wet conditions or in situ conditions. There may be limitations with existing equipment; nevertheless, the goal should be to develop the world's best materials characterization for fuel cell materials. As STEM, STXM, high-angle annular dark-field detection, etc., become more commonplace, this project must stay ahead of other efforts. As carbons have become well understood, the emphasis on different carbon types and how they affect performance and durability can be lessened. In general, this part of the project should be more of a concern to developers. The emphasis here should be on developing new and improved microscopy techniques.

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:

The objectives of this project are to (1) study water transport in single cells and stacks, (2) enable the fuel cell community to study water transport phenomena using state-of-the-art neutron imaging, (3) tailor neutron imaging to the needs of the fuel cell community, and (4) improve the spatial resolution to provide more detail of the water content in commercial membrane electrode assemblies.

Question 1: Approach to performing the work

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

- The National Institute of Standards and Technology (NIST) is continually innovating to meet the water-imaging needs of the fuel cell community by pushing to improve spatial resolution (ultimately to 1 µm) to allow studies of electrode layers, reducing imaging time for faster, more dynamic studies, and incorporating new capabilities (such as complementary x-ray tomography). This approach is sound and has excellent near-term and long-term vision.
- NIST maintains a national user facility for neutron imaging of fuel cells. It consults with the fuel cell community to plan facility improvements that would be useful in future studies. The facility provides free access for open research or fee-based access for proprietary research. NIST operates the neutron imaging facility and test stands in a user-friendly environment.
- The progress achieved to increase the geometric resolution was clearly highlighted. It is recommended that NIST equally highlight progress in relation to the time-scale resolution and compare progress with time scales associated with water processes such as a water drop traveling through the cell, membrane wetting and dehydration, and water accumulation in and removal from the gas diffusion electrode.
- The approach of using neutron imaging to study the water transport in single cells and stack has proven to be very good and very insightful. Every year, the NIST team works hard to add more capabilities and to increase the spatial resolution.
- NIST is attempting a wide variety of ways to advance both spatial and temporal resolution for neutron imaging. Furthermore, the team is attempting to satisfy customers both in the short term (grating, centroiding) and in the long term (cold neutrons). The approach focuses mostly on improving the analytical technique itself, as it should. NIST has wisely not confused the approach of this project with the approaches of fuel cell projects that seek to develop new material or optimize material design.
- The one criticism of the approach is that the efforts to improve resolution appear to be taking a while. Perhaps more resources could be spared to help. Until then, much of the fuel cell community appears to have lost interest.
- The overall approach is sound, although progress seems to have stalled compared to previous years.

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.

- In September 2015, NIST commissioned a new cold imaging facility with higher resolution than previously available and with potential to resolve ice and water. Last year, NIST commissioned a complementary x-ray imaging system and made it available to all users. NIST is also making the beamline available for hydrogen storage experiments. NIST continues to develop methods to improve spatial resolution. The ongoing improvements include an image intensifier, centroiding with detector microscope resolution <9 µm, 4 µm grating resolution, and a neutron microscope with 1–20 µm spatial resolution with 10-second to 10-minute time resolution.
- Most of the accomplishments and progress this year seem focused on technique and hardware development. The new cold neutron instrument, complementary x-ray tomography system, and slit and centroid imaging are all examples of ongoing development aimed at meeting the requirements of the fuel cell community. The application of these new capabilities to fuel cell systems has been limited, but slow and steady progress might be expected when pushing the boundaries of spatial and temporal resolution.
- Progress made toward the spatial resolution is excellent, and it looks like resolution is on its way to 1 µm by 2018. New cold neutron imaging is commissioned and ready, which will help researchers understand and, one hopes, resolve cold startup issues.
- Installation of the cold neutron imaging facility has been a very positive development and represents a possible future of high spatial and temporal resolution. The possibility of separating ice and liquid water provides hope. Although the slit imaging can provide resolution down to 4 µm, the 17-hour collection time limits what can be done within the course of assigned beam time. Many researchers will probably not be interested in 17-hour collection periods. The centroiding imaging is much better for collection time (four times) versus the slit imaging, but the gain in resolution is small versus the incumbent techniques. It is difficult to say whether advancing from 9 to 5 µm resolution will increase interest. Combined neutron and x-ray imaging is a good idea, although it is confined to cells of just 0.6 cm^2.
- Three milestones were completed, and work is ongoing for another milestone.
- Progress toward lower spatial resolution is good, but one must be cognizant of time resolution as well. The overall facility upgrades are quite interesting, but their use in experiments for understanding transport is not as compelling.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- Collaboration is excellent across the board, including many industries and academia. Close collaboration with General Motors is great to study fuel cells in operation.
- NIST listed a number of partners, users, and collaborators from academia, national laboratories, and industry. The presentation also highlighted results from a user program with University of California, Merced.
- Twenty percent of the beam time is allocated to fuel cell and hydrogen storage experiments suggested or requested by the community. Exemplary data for non-precious-group-metal catalysts were provided.
- The project is very collaborative and dependent in terms of getting materials. It would be good to see more coordination with new consortia and with state-of-the-art materials and designs.
- Collaborations have been focused on method and hardware development, with a very limited number of user collaborations reported, the one example being liquid water saturation studies in diffusion media with University of California, Merced. The reported allocated beam time was down from 43% last year to 20% this year. It is important that the team better balance its efforts between user work and instrument development in the upcoming year.
- The way in which collaboration with partners has been expressed in the slides is somewhat casual; nearly all partners throughout the course of the project are listed early in the presentation, but it is difficult to see which collaborations have been ongoing in the past year. It would be useful to understand which collaborations pertain to the true work of this project, which is the advancement of the neutron imaging

technique to improved spatial and temporal resolution. In this regard, collaborations are more useful to note than the collaborations with fuel cell customers (e.g., General Motors). It would be interesting to understand the depth at which collaborations exist with Commissariat àl'énergie atomique (CEA), the National Aeronautics and Space Administration (NASA), and others that have expertise in detectors and beam line technology.

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.

- NIST is actively engaged in developing state-of-the-art neutron imaging capability for fuel cell hardware and making it available to the community of fuel cell researchers and developers. The capability has been successfully applied to studying the dynamics of water transport in flow fields and manifolds. Further improvements in spatial resolution are needed to provide more details of the water content in catalyst layers.
- The project is very relevant in terms of understanding where the water is in the cell. The overall impact depends on others and the samples and experiments provided. It is not clear how much time is proprietary versus nonproprietary.
- With a series of new capabilities and techniques under development, the NIST project is well positioned to have a substantial impact on the Hydrogen and Fuel Cells Program in the upcoming years.
- The project addresses several barriers: durability, performance, and water transport within a stack.
- This project has hit an interesting juncture, which has a direct impact on its relevance. Much of the knowledge that could be gained from imaging fuel cells at resolutions down to 10 μm has already been gained, as evidenced by the decrease in fuel cell customers using neutron imaging. The future relevance of the project actually depends upon the project's ability to deliver higher resolution so that water can be imaged within catalyst layers, membranes, and other thin components. As x-ray techniques develop that are able to image water at higher resolution and at more widespread locations than neutron imaging, neutron imaging will have to provide unique advantages such as operation on a relatively large cell. Another inherent advantage is the lack of neutron cross-sectioning with iron and other materials of construction. Trying to merge both neutrons and x-rays together as complementary techniques is a good idea.

Question 5: Proposed future work

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

- The proposed future work is very similar to last year's, with aims to further drive resolution to 1 μm and improve detection limits and imaging time. Progress has been slow but steady, as might be expected for these challenging endeavors. There continues to be strong multiyear vision. In the upcoming year, demonstrating the application of these new systems, such as cold neutrons and complementary X-ray tomography, to fuel cell studies should be an area of strong focus.
- The future work in terms of both new resolution and techniques and new capabilities is compelling, although there seem to be multiple pathways. It is not clear how much is feasible or supported by the Fuel Cell Technologies Office rather than other programs. Segmented cell and similar capabilities would be good.
- Future work describes the efforts to increase the spatial resolution, continue refining current methods, continue with the neutron microscope to improve the spatial and temporal resolution, and combine X-rays with neutron imaging. It would be nice to see how the new cold neutron imaging will be used in collaboration with industry to understand and separate water and ice formation and management in fuel cells.
- NIST outlined the ongoing three-year project on a neutron microscope to improve the spatial resolution to 20 μm in 2017 and to 1 μm in 2018. It would be useful to understand how NIST decides the direction of the future work, what specific recommendations have been received from the users, and the directions from DOE and from NIST.

- The possibility of combining centroid imaging and neutron microscopy to further increase geometric resolution should be considered.
- Establishing 5 µm centroiding at 4-hour (or 1-hour) temporal resolution may be valuable to some. The future work involving the neutron microscope is a large part of where this project now is. Many developers are awaiting the 2017 milestone of 10-second/20 µm resolution, and especially the 2018 milestone of 20-minute/1 um resolution. NIST may need to begin contemplating how to handle the pent-up demand for testing when these capabilities become ready. It would be good to hear whether collaborations with other laboratories or with NASA are contributing to the development of the neutron microscope. It is not clear whether NASA actively participates. Perhaps there is some way that the development could be accelerated.

Project strengths:

- NIST has been very responsive to user needs and understands that its scope is the development of a technique, not the optimization of fuel cell materials. The project leverages a fairly large facility for neutron beams at NIST. NIST has personnel that proactively seek out ways to make neutron imaging better with advanced detectors and other equipment. Neutron imaging can be done on large cells with little cross-section with cell structural materials.
- Neutron imaging capabilities are impressive at NIST. Strengths include the team's efforts to keep improving, refining current methods, and also developing new methods to add more tools for water management understanding in fuel cells.
- The project has a balanced approach combining method development to improve geometric and time-scale resolutions, and there are multiple ways for users to access equipment, solve issues, and study water transport phenomena.
- This represents the best technique for imaging fuel cells in a nondestructive fashion, especially the water. Progress and plans toward better resolution are also strengths.
- There is a very good multiyear vision and good near-term progress in development of new techniques and instrumentation.

Project weaknesses:

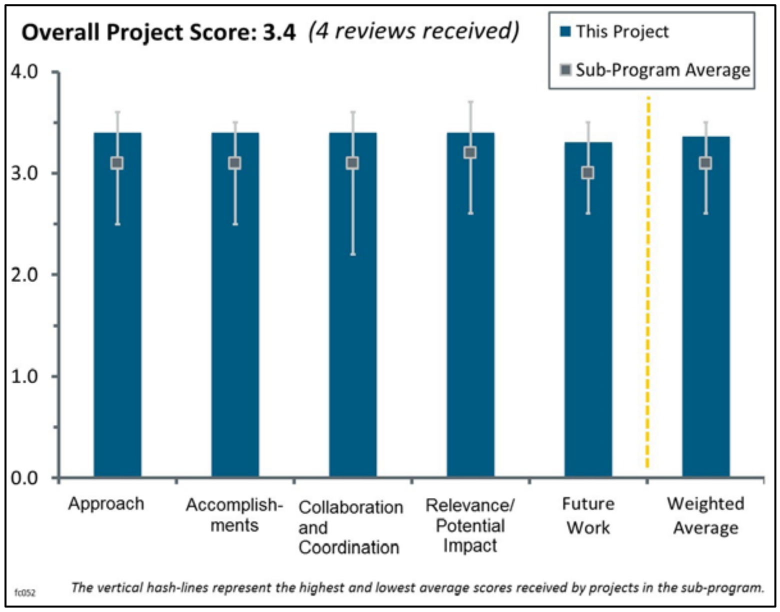
- The experiments that researchers wanted at the existing resolution have slowed down before higher resolution can be developed, which has diminished the interest in neutron imaging for now. Neutron facilities are difficult to access. Making use of the neutron scattering technique will always require travel to a beam line. There will still be a wait until 1 µm resolution can be obtained. In the meantime, fuel cell technology has advanced to a place where 1 µm resolution is necessary to extract information about catalyst layer performance and durability.
- User work seems to be down this year. While the focus on future work is important, the current capabilities of the facility seem to be underutilized.
- The operating principle of the opaque gratings (Gadolinium oxysulfide) for slit imaging should be given.
- The project is dependent on others for experiments and guidance.

Recommendations for additions/deletions to project scope:

- The progress achieved to increase the geometric resolution was clearly highlighted. It is recommended that the project equally highlight progress in relation to the time-scale resolution and compare progress with time scales associated with water processes such as a water drop traveling through the cell, membrane wetting and dehydration, and water accumulation in and removal from the gas diffusion electrode. The possibility of combining centroid imaging and neutron microscopy to further increase geometric resolution should be considered.
- It may be useful to add resources to accelerate the pace at which 1 µm resolution is being developed. The continuation of work on x-ray–neutron combined experiments should be predicated on interest. If users do not show interest, the work stream should be removed. Similar principles should be applied to slit and centroid imaging.
- NIST should reach out for guidance about what the critical techniques and information required are.

Project #FC-052: Technical Assistance to Developers

Tommy Rockward; Los Alamos National Laboratory



Brief Summary of Project:

Los Alamos National Laboratory (LANL) will test catalyst materials and participate in the further development and validation of single-cell design and test protocols. LANL will also provide technical assistance to working groups, the U.S. Council for Automotive Research (USCAR), and the USCAR/U.S. DRIVE Partnership Fuel Cell Technical Team.

Question 1: Approach to performing the work

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

- LANL applies its broad and deep fuel cell knowledge and facilities to a wide array of specific fuel cell materials, operating, and processing issues as requested by stakeholders and approved by the U.S. Department of Energy (DOE). Examples include bipolar plates, platinum-group-metal (PGM)-free catalysts, membrane electrode assemblies (MEAs), and stack testing. LANL applies accepted industry practices and procedures and innovative approaches to obtain meaningful results for stakeholders.
- Use of LANL's infrastructure and expertise for evaluation and diagnostics of commercial stacks/MEAs/catalysts is uniquely appropriate for this project.
- This is an excellent use of national laboratory resources to help solve specific problems submitted by industry and other laboratories.

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.

- LANL provided technical support in the areas of bipolar plate coatings and their application, PGM-free catalyst assessment, powder properties, stack testing, cell architecture, and catalyst testing for a wide range of entities. This support enhanced progress toward DOE goals. LANL provided not only data but also technical insights based on years of fuel cell experience.
- Each task was well planned and well executed. The project fully supports DOE goals. Results were clearly presented in a form that a layman could understand.
- This is a very good array of investigations.

Question 3: Collaboration and coordination with other institutions

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

- LANL has provided technical support for many stakeholders, including powder manufacturers, catalyst and membrane developers (conventional and alternative), plate producers, automotive original equipment manufacturers, and stack integrators.

- This whole project is all about collaboration with organizations that brought a specific problem for LANL to investigate.
- LANL needs more collaborators on the project or needs to do better job reaching out for user facility service.

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.

- LANL's work is relevant to development of several of the cell components that comprise polymer electrolyte membrane (PEM) stacks. The project work expedites PEM technology development by entities that do not have the necessary equipment and facilities or expertise. This approach reduces development cost and time to DOE.
- All of the sub-projects addressed a DOE research and development objective.

Question 5: Proposed future work

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

- Future work continues previous and existing collaborations and recognizes the unknown character of future tasks.
- Future work is a continuation of the effort. No doubt new tasks will be introduced.

Project strengths:

- Deep and broad knowledge of PEM technology is a strength.
- This project is an excellent use of LANL facilities and expertise. It is a focused effort on solving specific problems that progress DOE objectives. The description of the activities and results was clear and concise.
- Collaboration with a variety of companies on focused research that LANL is uniquely qualified to conduct is a major plus. LANL participation in the Fuel Cell Technology Team is a strength.

Project weaknesses:

- The narrow focus on PEM technology is a weakness.

Recommendations for additions/deletions to project scope:

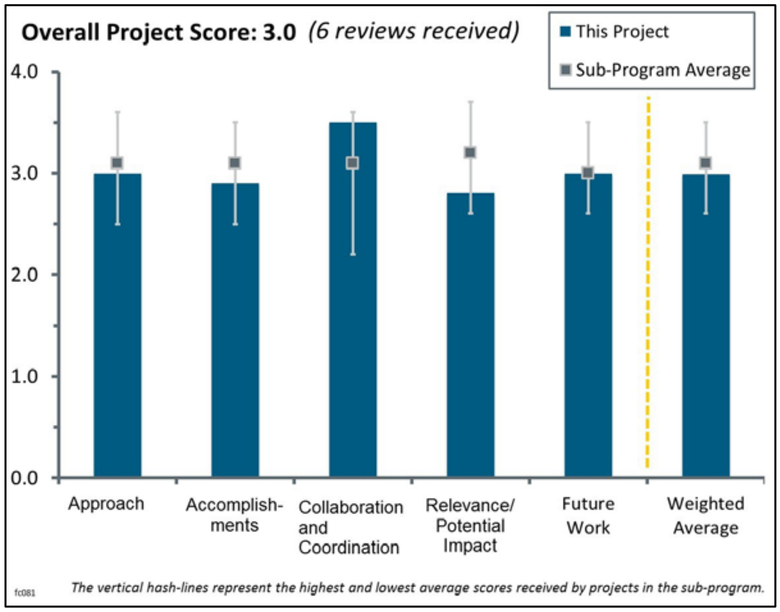
- This project should be kept going and expanded, if possible. The approach should be applied to other national laboratories.

Project #FC-081: Fuel Cell Technology Status: Degradation

Jennifer Kurtz; National Renewable Energy Laboratory

Brief Summary of Project:

The fiscal year 2016 objectives of this project are to (1) receive and analyze new laboratory durability data, (2) update and publish the durability results, and (3) include electrolysis data. The National Renewable Energy Laboratory (NREL) will (1) develop a snapshot of the state-of-the-art fuel cell durability, (2) uniformly apply analysis methods to developers' voluntarily supplied data from laboratory testing, and (3) provide an independent assessment and status of state-of-the-art fuel cell technology.



Question 1: Approach to performing the work

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

- Given the constraints required to protect proprietary information and the reliance on voluntary submission of data from industry, the project has managed to achieve a good level of participation. The whole project relies on achieving this balance.
- The approach in terms of statistical analysis is solid and provides valuable information. The approach based on receiving voluntary information, although very difficult, is starting to pay off, and incentives through the provision of feedback were very interesting. The feedback provided in terms of ranking will also be very valuable to the industry.
- The project aims to gather data from voluntary data suppliers (mostly industrial) and analyze the data to produce both detailed data products (DDPs) and composite data products (CDPs). Receiving such a large number of data from partners is a real challenge, especially for durability data. The project employs a statistical approach that analyzes data with many heterogeneities (different technologies, suppliers, operating conditions, testing protocols, etc.). Considerable work in preprocessing, pre-selection, and standardization of data is done prior to analysis. The analysis of data is based on statistical analysis/fitting. The degradation fitting is based on segment linear fitting, which is not always appropriate. Using more physics and applying some adequate degradation models may give more accurate values of the projected voltage at 10% nominal voltage and therefore more accurate durability value (20% is more compatible with stationary applications in the Multi-Year Research, Development, and Demonstration Plan [MYRDDP]).
- Collecting real-life operational data from the developer is the best way to measure the industry technology status. On the other hand, as stated in the presentation, industry is looking not only for the highest performance but rather for the balance between performance and cost. It would be more useful to develop a combined standard to measure the progress.
- This is a far more difficult and uncertain task than most people recognize. The approach needs to be bolstered to ensure validity and accuracy.
- This is a generally well-constructed and ambitious project. However, the volume of data collected and (in many cases) the lack of detailed information makes data analysis extremely difficult.

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 project does not address any objectives set in the MYRDDP, such as increasing durability or lowering cost, but it is meant to provide an assessment of the status of fuel cell and electrolyzer durability and cost by gathering valuable information from industry. The project is a valuable tool for DOE to assess the evolution of the technology regarding the targets. Data about electrolyzers have been integrated and would be a good support for DOE to set targets for this technology.
- The project does not address any of the barriers on its own but it does so by indirectly providing feedback to DOE. The datasets collected so far are an impressive accomplishment, given that industries are often very reluctant to provide such confidential information. The addition of the set of electrolyzer information is also a positive feature.
- While team members may have extracted as much as possible out of the data they were given, it appears that there is limited NREL evaluation of the data. The analysis is almost a meta-analysis to show general directions rather than a set of conclusions drawn from careful examination. Ideally, there would be enough knowledge about the systems to make observations and insightful conclusions and categorizations. It is concerning that the degree of battery hybridization may be a major factor in (helping or hurting) the longevity of the stacks in ways not captured by the project’s methodology. The results are interesting and worthwhile but ultimately they are of limited usefulness because of the complexity and variations of designs considered (and lack of knowledge about each system). The data inappropriately lumps all fuel cell technologies (solid oxide fuel cells, direct methanol fuel cells, polymer electrolyte membrane fuel cells, etc.) together in one data file. It is hard to envision meaningful or nuanced conclusions coming out of such co-mingled data.
- Given the diversity of technologies and system applications, it is very difficult to analyze the data when all the different systems in a specific group (e.g., automotive) are lumped together rather than looking at a specific supplier and application to see the trend over time. Therefore, it is important to question the value of the analysis to DOE in making decisions on goals and investments in research and development (R&D). However, that is for the end-use customer, DOE, to decide.
- The data from testing need to be standardized to a common set of conditions when possible. If this is not possible, models could be used to project received data to a common basis.

Question 3: Collaboration and coordination with other institutions

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

- The project has done an excellent job in interacting with the industry for data collection. It is evident that participants are very active in data collection and their outreach includes international developers.
- Owing to the voluntary requirement, a high level of collaboration with industry is required to obtain data. The project has done well to get as much information as it has.
- There was good participation from a variety of suppliers.
- Up to now, the project team has been very successful in convincing several partners (data providers) to share data (22 new data sets have been gathered since 2015, and 174 have been analyzed from the start of the project in 2009). However, the fact that the data are provided on a voluntary basis makes the project very dependent on the good will of other institutions (leading to issues with data quality; incomplete information about the testing conditions, incidents, and testing history; etc.). This can lead to unreliable results despite the good analysis of the team. Some partners have provided data for several successive years, which means they are satisfied with the resulting analysis quality of the DDPs. The project certainly has good collaboration with other partners, given the facts cited above.
- U.S. and international fuel cell developers who will supply data voluntarily and review published results are the collaborators. It is unclear what the response rate was and whether statistical evaluation is needed. It is also unclear how vendor veracity is checked.

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.

- The impact of the project on DOE is indirect but present. It has been useful to see a comparison between different areas of the world given that there are international fuel cell developers in the datasets. The comparison between application areas can provide feedback to DOE as to where to focus its efforts.
- Tracking the evolutions of durability and cost will allow DOE to assess the technologies' advancement and correlate their current status with the objectives and means that have been put to achieve them (funding). This work can support DOE in more efficiently identifying the topics in which to put more effort.
- Given the constraints, the analysis has produced the best possible results. However, it is still unclear whether the results are good enough for DOE to make informed decisions about its R&D program. That is DOE's call. Based on the outputs presented, it seems that the trends will be difficult to identify.
- The project provides useful insight into the overall fuel cell status but the value is severely limited by the complexity of the data, lack of knowledge concerning the circumstances of each data set, and the co-mingling of data from multiple fuel cell types.
- The impact depends on many factors, and these need to be discussed. The workers do recognize some limitations.

Question 5: Proposed future work

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

- The proposed work is reasonable and expected. The cost analysis will also be useful in seeing the relationship between cost reduction and durability.
- Given the constraints on the project, the future work is satisfactory.
- The proposed future work is in line with the results shown. However, it is strongly dependent on whether data are supplied by partners and on the quality of the supplied data (availability of all needed information for analysis).
- The approach should be reviewed to determine merits and limitations.

Project strengths:

- There was good industry engagement in the collection of confidential data and useful analysis showing the trend for the year in each application area.
- There is very good collaboration with industry to obtain voluntary data, which is the core requirement for this project to be in existence.
- This is an ambitious project undertaken by an NREL team uniquely qualified to perform the task. There is a logical approach and competent execution, given the complexity and volume of data involved.
- This project's approach is an independent and uniform analysis of valuable data from key stakeholders. The project offers access to the data without jeopardizing confidentiality, which is a big issue, especially with data linked to durability. Independent and uniform analysis is an important tool for DOE to assess the status and progress of the current technologies regarding the objectives set in the MYRDDP and the funding involved. Analysis is also an important tool for the data suppliers to assess the evolution of their technologies (DDPs) and compare it objectively with the market evolution (through CDPs).

Project weaknesses:

- The fact that the analysis is applied uniformly (no matter what the technology is) makes interpretation and comparisons difficult. For instance, the degradation functions of different technologies are not necessarily the same. In the current analysis, the projected value for stationary applications seems to be underestimated: a value of 20% voltage degradation seems to be a more adapted metric to assess voltage degradation (MYRDDP). Even if the project team submitted a detailed metafile to the data suppliers, data suppliers cannot guarantee what the data "experienced" during operation. For instance, several faults could occur during operation and affect the durability. If the task of gathering technical data has been solved by setting a clear metadata template (though all needed technical data are not supplied), it should also try to include the history of the data (incidents, recovered faults, etc.). However, this kind of information is not easy to transfer.

- The data are too scattered for meaningful data analysis. Insights and conclusions regarding the data are not made in sufficient quantity. The fuel cell technologies are all lumped together. It is possible that "old" stack data are combined with "new" stack data, thereby merely averaging the values, whereas conclusions discerning the performance differences would be preferable.
- The measurement of the yearly progress is not clean. Industry does not look only into performance, so the analysis should include other factors to show industry trend. This trend could also guide technology development in the Hydrogen and Fuel Cells Program.
- The "vanilla" approach to analyzing data required to preserve supplier proprietary information makes it difficult to clearly identify progress and where resources need to be applied.
- It is disappointing that there is no breakdown by technology for both fuel cells and electrolyzers.

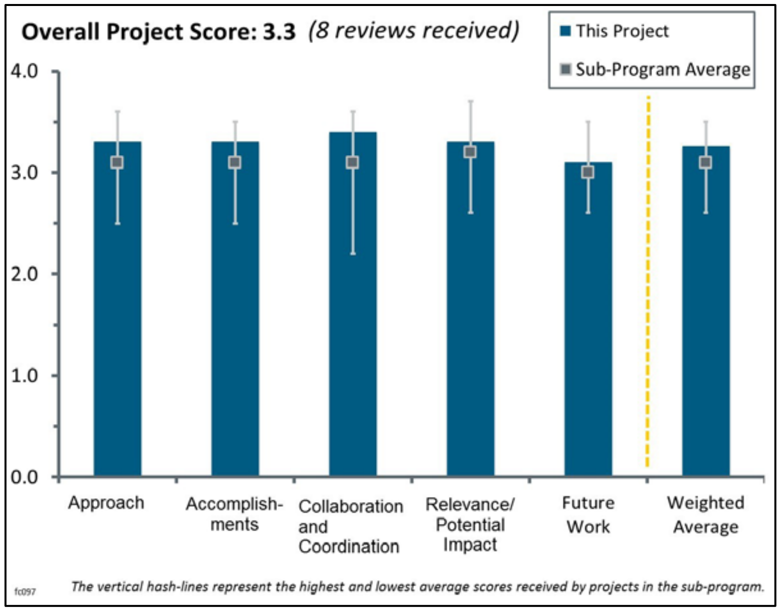
Recommendations for additions/deletions to project scope:

- Grouping the data by technology for different applications and slightly adapting the analysis to each technology/application could help with interpreting the results (20% is more compatible with stationary applications [MYRDDP]). Instead of a basic linear fit for the degradation models, adding more physics behind the fitting function would give a more precise value of the durability. Approaches of data-based prognostics and health management are very good tools for useful lifetime estimation. The DOE-funded projects could be highly encouraged to share a part of the generated data with the project team. The metadata template should include information about incidents that may have an impact on the durability. This information should be taken into account in the analysis. Data about electrolyzers have been integrated and would be of great help for DOE to set targets for this technology. It would be interesting to create categories of electrolyzer technologies so the analysis is run by technology category.
- Given the presence of international fuel cell developers in the datasets, it would have been nice to see a comparison between these regions (e.g., United States vs. European Union vs. Japan) to see the competencies in each application area. Outreach to other funding entities for collaboration would be extremely useful as more participants could be urged to join the data collection exercise.
- The project should collect real-life data from industrial developers. These data are more representative than laboratory testing data. The project should cover multiple types of fuel cells.
- The project team needs to dig deeper into the data to remove the scattered and blended nature of the voluminous datasets. This may/will require additional data from the suppliers—data the suppliers may not be willing to give.
- DOE should analyze the value of the project as currently constituted.

Project #FC-097: Stationary and Emerging Market Fuel Cell System Cost Analysis – Primary Power and Combined Heat and Power Applications

Vincent Contini; Battelle

Brief Summary of Project:



The overall objective of this project is to assist the U.S. Department of Energy (DOE) in developing fuel cell systems (FCSs) for stationary and emerging markets by developing independent model and cost estimates. The project goals are to (1) identify major contributors to FCS cost, (2) quantify potential cost reductions based upon technological improvements, (3) identify major contributors to FCS manufacturing cost, (4) identify areas for manufacturing research and development (R&D) to improve quality and/or throughput, and (5) provide a basis for consideration of transition from other industries.

Question 1: Approach to performing the work

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

- The objectives of the project are well aligned at addressing the barriers of cost reduction of fuel cell components and materials. The methodology that has been followed so far is well planned, and the system design for other technologies is well broken down and detailed.
- The project approach appears to be well organized and focused around estimating the manufacturing cost of small to medium-sized FCSs for stationary and backup power. The study takes an agnostic approach to specific fuel cell chemistry, providing a useful point of comparison for polymer electrolyte membranes (PEMs)and solid oxide fuel cells (SOFCs). Addressing a couple minor issues would improve this project. First, the parasitic load is pegged at 20% of the gross power. This is too high for a well-designed system and leads to balance-of-plant (BOP) components that are oversized in addition to an oversized stack. Second, one of the stated objectives is to identify areas of manufacturing improvement. One of the strengths of cost estimation is that it highlights existing manufacturing practices that are inefficient. The other strength is that it can be used to identify components and materials that drive cost. Neither of these discussion points was addressed at the level or depth they deserve. Maybe there was not enough time in the presentation, but this is the kind of insight that is needed for this work to have its maximum benefit.
- The project does an adequate job of identifying the main contributions to the cost of the two FCSs. Connecting with additional commercial suppliers currently selling combined heat and power (CHP) systems should be a priority for the approach. While not directly contributing technical solutions to the barriers, this project is helping answer questions about acceptance and focus areas moving forward.
- The project as conceived—independently evaluating costs of low-temperature PEM (LTPEM) and SOFC systems—would provide additional insight into which technologies are best suited for stationary CHP applications.
- The manufacturing cost methodology is well developed for the task at hand.
- The project has a well-organized approach. The cost modeling does not identify Design for Manufacture and Assembly (DFMA); however, DFMA is reported in the presentation.
- Battelle's methodology involves market assessment, system design, and costing supported and guided by stakeholders from most aspects of the technology. It is not clear what entities provide input into market assessment and system design. Market assessment does not seem to involve end users. Battelle uses

established DFMA techniques for cost estimates where appropriate. Parametric analysis by system size and manufacturing volume is informative. Technology Readiness Level 9 for stacks and fuel processors is optimistic.

- The decision to use a venturi approach to the SOFC anode recirculation instead of a blower had the design impact of strongly limiting the turndown ratio. This in turn reinforced the decision that the target market was urban and always on the grid or almost always on the grid. This unnecessarily limited the target market. Non-urban non-grid adopters were excluded, but these customers might be early adopters and willing to pay a premium. Examples include the oil exploration/fracking industry. Not using a recirculation blower also removed an expensive and poorly developed piece of equipment from the cost/development equation. High-temperature SOFC anode recirculation blowers are difficult to come by and have a low mean time between failures. The team repeatedly received input that potential customers would expect SOFC CHP systems to be able to provide backup power should the grid go down. The system that this effort developed therefore may be targeting an unnecessarily small niche—urban users who rarely expect the grid to go down. The customers looking for backup power would have to look elsewhere.

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 following important specific conclusions have been obtained: electronics and power conversion dominate system cost, particularly as system size increases; an attractive value proposition exists under specific utility rate conditions; Manufacturing Readiness Level (MRL) for many BOP components not ready for mass production could be a significant cost driver; and DFMA performed on specific components (fuel processing, stack) assumes technology at greater than MRL 9.
- The primary goal was quantifying cost reduction likelihood as production levels increased. For the system chosen, this was very well done. The analysis is sound.
- The breakdown of costs that have been provided can be very useful for DOE, as it can identify the most important cost contributors. It is evident power electronics for both applications are expected to be the biggest contributor to BOP costs for both technologies, and therefore future DOE efforts should involve projects dealing with this issue. It is strange that, in the forecasts shown, there is no labor cost decrease with the increasing number of units.
- The project appears to have made good progress toward surveying and summarizing the size, application, and chemistry space. It would be useful for understanding the system designs if the authors were to include equivalent systems for comparison and to highlight where assumptions have been made.
- Cost analyses were completed for 100- and 250-kW PEM and SOFC systems for CHP and primary power. Tornado and waterfall charts and identification of high-impact R&D would be informative.
- It is not clear why the fuel processor for the PEM system requires two high-temperature shift reactors. Commercial SOFC (Bloom Energy) systems use stack heat to improve electrical efficiency. It is not clear that this design loses electrical efficiency by having a micro-CHP application or that other proposed commercial SOFCs have a CHP component, e.g., the LG Fuel Cell System. System life is projected to be 50,000 hours. It is not clear whether the PEM or SOFC stacks will last 50,000 hours and, if so, what experimental evidence exists for such long stack life. Overall efficiency of 80% for a PEM system is very high considering the low quality of heat from a PEM fuel cell; it is not clear how this is justified. Electrical efficiency of 40% for an SOFC system is low when compared to the reported Bloom Energy SOFC electrical efficiency (50% or greater). Most SOFC grid-connected systems are for base load with peak power supplemented by other systems. The justification for not identifying the SOFC as a base load system is not clear. The use of PEM systems for off-grid operation as a critical load or backup power is justified. The SOFC system as a backup power source would be difficult to justify based on operating at hot standby waiting for backup power applications. For PEM applications, the gas diffusion layer does not have a microporous layer added to the paper, the feasibility of which seems questionable. Silicone is poison to PEM fuel cells and is normally not used in PEM fuel cell seals. The use of silicone should be discussed with fuel cell original equipment manufacturers (OEMs) and fuel cell seal manufacturers. DFMA methodology is reported on slide 13 but is not identified in the Approach. It would be good to know how the cost per kilowatt-electric for PEM compares with the Ballard or Altergy cost (slide 14). PEM BOP

costs are much greater than PEM stack costs, perhaps implying that the Fuel Cell Technologies Office should redirect efforts to reduce BOP cost. Total system cost per kilowatt with markup is considerably below the Bloom Energy reported cost of $10,000 per kilowatt. How Battelle rationalizes its cost numbers when comparing them to Bloom Energy's is unclear. The reviewer agrees with the overall summary.
- Lack of data on commercial systems in the power range presented as well as limited customer acceptance data left the project lacking real informative data for the last barrier. Updating and revising the same methodology used over the last few years does provide interesting cost and system-level information, but the missing current CHP data and customer usage requirements leaves room for progress next year.
- The deemphasizing of HTPEM systems is regrettable.
- While the cost models were very detailed and the methodology sound, there were few bill-of-material or manufacturing cost reduction suggestions, and team did not suggest R&D areas to improve cost. These would significantly increase the value of the project to DOE.

Question 3: Collaboration and coordination with other institutions

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

- The stakeholder input is done through 19 companies that represent the sector well. They have been consulted for design inputs, cost inputs, and reviewing the results. Dissemination of the results should be encouraged.
- The team had significant participation from industry in the areas of LTPEMs and SOFCs.
- Collaboration is very extensive.
- The list of collaborators is appropriate for the systems being analyzed.
- There are good interactions. A SOFC OEM should be included.
- A large cross-section of stakeholders is listed to provide design and cost input and to assess validity of the results. Involvement of raw materials suppliers and stack/system component providers seems weak.
- The long list of collaborators proves that data are being requested and used to fill in the model, but a lack of mature system data is noticeable. Looking to Europe or Japan for current CHP data and performance metrics is suggested to help increase the fidelity of the project's customer acceptance portion.
- It would have been better if potential customers had played a larger role in the early part of this effort, when the performance parameters of the CHP system were being determined.

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 very high relevance and potential impact by helping to identify the biggest contributors to system cost. Though more data are needed for this project, the cost and system data provided can help DOE focus investment in future years.
- The work helps answer questions about opportunities for cost reduction to penetrate non-automotive applications.
- Cost estimates provide insight into key areas for more R&D aimed at reducing cost.
- The primary goal was cost reduction. That analysis was excellent—a 4.0. The secondary goal was manufacturing capability. That was also well explored. The third goal was customer acceptance. The approach chosen was to win customer acceptance by driving down unit cost. That was flawed. Customer acceptance would have been enhanced by better understanding customer requirements.
- The project has a strategic impact, as it can affect and adjust DOE's plans regarding funding cost-reducing projects and regarding projects that focus on increasing the MRL of the components that are identified here. It would have been good if, through the project's analysis and the feedback received from the stakeholders, suggestions for further cost reductions could be made.
- The project highlights the importance of BOP to the overall cost. Some of the analyses need to be compared to real-world costs. The results reported here should be compared with costs for a state-of-the-art

gas turbine. It is unknown whether a state-of-the-art gas turbine would be less costly and how emissions would compare to a gas turbine.
- While the authors have broken down component cost contributions, it is not clear that the authors have addressed areas in which current manufacturing approaches need improvement. This is the kind of insight that is needed for this work to have its maximum benefit.

Question 5: Proposed future work

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

- Reviewing and updating all of the work done over the duration of the project with the most up-to-date information will be a huge benefit to this project. There has been much progress over the years, and making improvements to past simulations will be very useful.
- Future work involves straightforward updating of previous cost estimates. The project is ending in 2017. Applications and reports will be revisited and revised as appropriate.
- Work appears to be concluding. Battelle will revisit all applications in the previous four budget periods and update all reports.
- The project is all but complete.
- The proposed future work is not detailed and was mentioned only briefly.
- This effort is near its conclusion, so there is neither a great deal of proposed future work nor much room to change. One thing that might possibly be done would be to take the finished system design, predicted performance, and predicted costs, and “shop it around” to potential customers to gauge the product’s effectiveness in attracting market interest. The results of that could have an impact on future cost studies and system designs.
- Future work is not specific, but a review is necessary.
- The proposed future work leaves out significant detail. Because this is the final year of the project, it makes sense to review the analyses of the previous four years. However, it would have been helpful to know what specific weaknesses of the previous analyses the authors expect to address, and maybe some global trends that they would like to explore. At this point, the authors should be in a position to address what the key cost drivers are.

Project strengths:

- Many valuable data have been created through this project. The system design and cost analysis have been very detailed. The project has done a good job on receiving input for the system design and manufacturing costs from a large number of stakeholders.
- The project does a great job of identifying the cost drivers for the CHP market. The project highlights the need to address BOP, which seemed to be a common theme in the review this year.
- The project provides a broad survey of fuel cell applications including primary power, backup power, auxiliary power units, and material handling equipment. Business cases were explored.
- There is a broad team from across the LTPEM and SOFC industry. The cost modeling methodology is sound and detailed.
- The system design is well developed. The project uses a systematic approach.
- The cost analysis performed was excellent and educational.

Project weaknesses:

- The decision to not use an anode recirculation blower for the SOFC may have unnecessarily limited the potential market, which means the analysis is weakened because of its applicability. The effort might have benefited from having representatives from the customer base involved as collaborators.
- The project lacks real CHP data from current market leaders. There is also a lack of an international baseline because CHP is more common overseas.
- The project did not include HTPEM. Information from companies such as Advent or Serenergy could have been used.
- Several sizes and types of systems were analyzed without fully exploring manufacturing issues.

- There is a large discrepancy between the analyses and costs for commercial FCSs.
- There is a lack of solutions proposed that address the cost reduction issues.

Recommendations for additions/deletions to project scope:

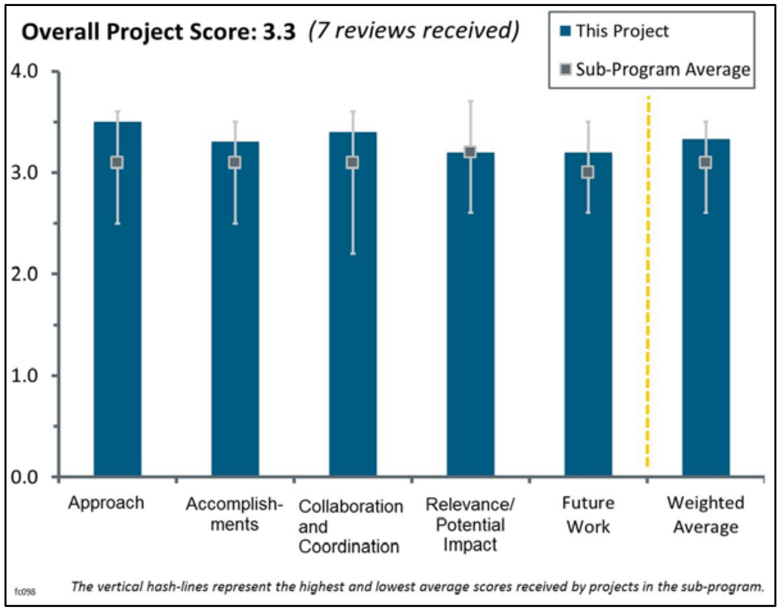
- This is a good project.
- DOE should take note that if we rely on all types of PEM cells and applications to build the manufacturing base and move down the learning curve, the technologies should use “the same” cell materials and processes. It is not clear that such a comparison has been made between the cost programs of Battelle, Lawrence Berkley National Laboratory, and Systems Analysis, Inc.
- If possible, in the months left and with the small amount of funding left, “shopping the design around” to potential customers would help assess how germane the research is to the potential customers who are interested in CHP.
- The project should rationalize the differences between the cost analyses presented here and the cost of commercial FCSs and the cost/properties of a state-of-the-art gas turbine system.
- The project should find commercial data for commercial acceptance and cost savings.

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:

The overall objective of this project is to develop a total cost of ownership (TCO) modeling tool for design and manufacture of fuel cells in stationary and materials handling systems in emerging markets. Project goals include (1) expanding the modeling framework to include life cycle analysis and possible ancillary financial benefits, including carbon credits, health/environmental externalities, end-of-life recycling, and reduced costs for building operation; (2) identifying system designs that meet lowest manufacturing cost and TCO goals as a function of application requirements, power capacity, and production volume; and (3) providing the capability for sensitivity analysis to key assumptions.



Question 1: Approach to performing the work

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

- An independent TCO model of low-temperature polymer electrolyte membranes (LTPEMs), high-temperature polymer electrolyte membranes (HTPEMs), and solid oxide fuel cells (SOFCs) with recommendations to the U.S. Department of Energy on the most effective areas for continued research and development (R&D) is an excellent project.
- Lawrence Berkeley National Laboratory involves stakeholders from most aspects of the technology to provide input to materials, processing, stack and system design, and operations and to review results for validity. Parametric analyses on the basis of production volume and system output is instructive. More information on the Air Pollution Emission Experiments and Policy 2 analysis model (APEEP2) is needed. Focusing on externalities and applying findings to other fuel cell costing projects would be interesting.
- The life-cycle impact assessment is a good addition to the Fuel Cell Technologies Office.
- The approach appeared sound and complete.
- The approach for the project is credible, but inputting data from multiple stack producers and using current density from one vendor and power density from another does not seem realistic. The cost information updates compared to last year's data were appreciated. Presenting combined heat and power (CHP) usage examples in various markets is an excellent approach to identify acceptance criteria.
- The attempt to value environmental externalities is carefully presented and included in the overall life-cycle assessment. However, externalities may be overvalued. HTPEMs would be attractive for CHP; however, industry consensus is that durability is inadequate. There was no mention of HTPEMs in the discussion; the project just needs to justify ignoring them.

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 accomplishments over the last year have been excellent. Highlighting cost benefits, health benefits, and potential future regulations all help paint a more accurate picture of what CHP systems need to do and cost to be competitive.
- Extensive revisions to the SOFC CHP systems life-cycle cost model were completed, including updated financial quantification of externalities (e.g., environmental benefits). Tornado charts provide insight into the areas needing the most cost-reduction R&D. Perhaps tornado charts could be generated for sensitivity of externality inputs and assumptions, if that is possible.
- The effort is near its end, and the results are almost finalized. The accomplishments and work progress are excellent and essentially complete.
- The de-emphasis of HTPEM is regrettable. The team's approach was very detailed, and the TCO model is impressive and appears sound in most areas. As to overcoming barriers, there appeared to be few recommendations for improving the TCO models. Leasing and platinum recycling are two areas that could significantly improve the cost of electricity (COE).
- The focus on SOFC for CHP is good. System temperatures seem a little low for current SOFC technology. The discussion of turndown and other system operating factors that influence effectiveness as CHP were not included in the presentation (it is not clear if operating factors were included in the analysis but just not presented). PEM systems may also be relevant for CHP when heat available from fuel processing is included. Specifics of PEM hotel evaluation are lacking (whether waste heat recovery is counted). It is not clear whether either system is considered capable of operating off-grid in backup power mode. Based on SOFC schematics, off-grid operation seems not to have been considered. Grid-outage operation may be an important consideration for end-user value—saved business and saved product during grid outage translates into real dollars, unlike environmental externalities, which typically do not have real cash value in most locations.
- There was no sulfur clean-up in the 50 kW SOFC CHP system; this should be added. It is not clear where heat comes from in the second heat exchanger that increases the air temperature to 650°C. A 59% average system net electrical efficiency is high compared to Bloom Energy's ~57% electrical efficiency with no CHP component. Stack yield numbers on slide 16 appear to be very high. The scrap rate was not provided. The system cost is very low compared to Bloom Energy's cost. There was no definition of the marginal emission factors (MEF). It was difficult to follow the greenhouse gas (GHG) benefits. Units change between slides 23 and 24.
- It was not clear how the costs determined here compare to a state-of-the-art gas turbine system. The state-of-the-art gas turbine system has GHG emissions better than or equivalent to the Bloom Energy systems; it is not clear how this system compares.

Question 3: Collaboration and coordination with other institutions

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

- Stakeholders from most aspects of the technology provide input and results assessment. Involvement of Strategic Analysis, Inc., is encouraging because of the company's long experience in estimating costs of fuel cell systems.
- The project has great input from multiple collaborators. It is clear the project has reached out and communicated with experts across the United States and international communities.
- The nature of this project does not lend itself well to collaboration in carrying out the work, but it is good to see that industry has been consulted as a reality check on assumptions.
- The diversity of the collaborator group seemed well thought out and comprehensive.
- The list of subcontractors and contacts is extensive.
- Teaming arrangements are very good.

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.

- Insights into high-cost areas (tornado charts) are crucial to identifying R&D needs. Quantification of externality benefits (emissions) helps assess the market potential and initial geographical and application penetration and identify early market opportunities from an environmental standpoint.
- The work is highly relevant. The regional emissions data and potential impacts that fuel cells can offer to those highly affected regions are huge. The coupling of the health impacts with the cost data and TCO keeps this project highly relevant.
- There is one aspect to how this effort was performed that is noted here rather than in Accomplishments and Progress, and that is the unknown confidence interval in the calculations used to determine the health/environmental impact in dollar per kilowatt-hour. The results of the study are clear: even at the highest production levels studied, from a private entity's perspective, adoption of this technology is not cost-effective. Only when the societal impact of NO_X/SO_X/PM is factored in does the technology appear cost-effective. To calculate that environmental externality, it was necessary to depart from standard Design for Manufacture and Assembly (DFMA) analysis and extrapolate from a university study to estimate the health/environmental impacts of NO_X/SO_X/PM reductions in monetary terms. (The analysis showed that health was the driving externality by far over GHG emissions, even when the CO_2 reduction was measured at a high GHG credit rate of $40/ton of CO_2.) The problem is that there is no confidence interval in the calculation of the health/environmental impact. The danger is that the results of this study might be used to argue for mandated use of these systems to displace diesel systems when the calculated value is used without knowing a true confidence interval. Without the confidence interval, making a policy recommendation based on the results of this study implies an analytical underpinning that is stronger than what actually exists.
- The team highlights progress in cost reductions, cost of energy increases, and high spark spread areas and accurately predicts geographies where there is a strong existing COE. Including "escalating social cost" as recommendations for improving the COE of commercial CHP systems will be difficult.
- The relevance/potential is not as high based on differences between the analyses presented here and actual costs of commercial fuel cell systems. There is a disconnect somewhere. It is not clear how the GHG emissions compare to state-of-the-art gas turbine systems or whether the project is making an improvement.
- The focus on environmental externalities is valuable but probably insufficient to assist with market penetration in most localities under most current regulations.

Question 5: Proposed future work

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

- This project is near its end, and there may not be much room to make any changes. But if it were possible, taking a stab at determining the confidence interval of the health/environmental externality impact on notional cash flow would advance the overall project's worth.
- The project is in the final phase. Future work involves final estimation revisions and preparation of the final report. The report should include adequate discussion of externality concepts and benefits.
- The project ends in three months; future work is primarily documentation and reporting.
- This is the end of the project; no future work was addressed.
- The project is near completion.
- No future work was proposed.

Project strengths:

- Studying TCO and health (societal) impacts of using fuel cells in a CHP is definitely a project strength. Adding future impacts of the clean power regulations shows the project is looking into the future to ensure the data provided are accurate for the near future and further out.

- The analysis is detailed and rigorous. The COE model is impressive. There has been extensive vetting with industry and collaborators.
- The project is well envisioned, has good analytical rigor, and is well performed.
- Quantification of externalities is very informative.
- The project attempts to value environmental externalities.
- The project benefits from the inclusion of life cycle impact assessment.

Project weaknesses:

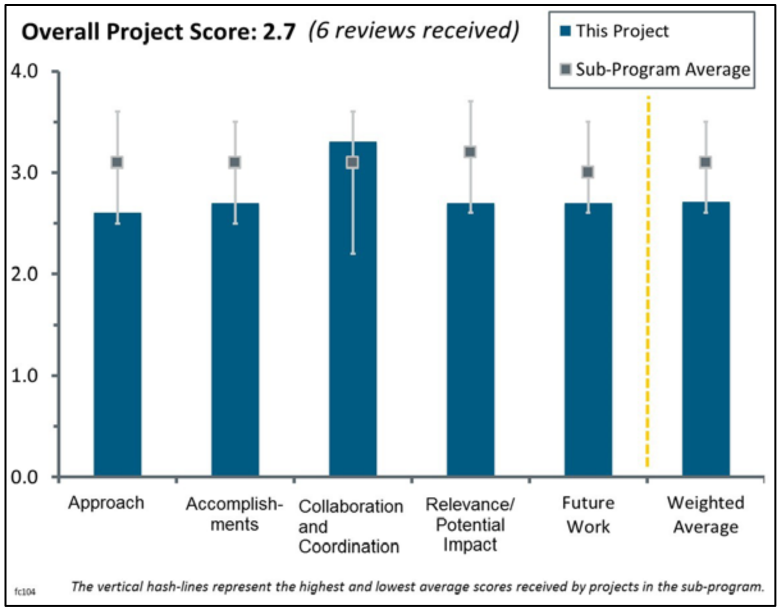
- This is not as much a weakness as a result of the effort: the advisability of using these systems rests upon a societal benefit because the cost analysis, when looking at just the impact for the system owner, indicates that there would not be a cost benefit to adopting it. The societal benefit is determined by the one element of the analysis outside the standard DFMA framework: the health/environmental externality. Without a confidence interval for that calculation, is it difficult to know how to weight any decisions to adopt such systems.
- The data can be very complex when presented; the only weakness would be in the pace of the presentation.
- It is hard to tell if off-grid (grid outage) operation is included—probably not. If not, this is a significant oversight in evaluating the TCO.
- Lack of HTPEM data is a weakness. Detailed information could be gathered from companies such as Advent and Serenergy.
- The project needs to compare its results to state-of-the-art gas turbine systems in terms of cost and GHG.

Recommendations for additions/deletions to project scope:

- The project should include grid-outage evaluation. Operation off-grid will require additional hardware and changes in design compared to what was presented and will therefore increase cost. However, the value of avoided losses in business and/or product (think frozen food at the grocery store) will more than offset the additional cost.
- Slide 34 shows future stack durability as 40,000 hours. A better number would be 60,000–80,000 hours. It would be good to know whether this changes the results and conclusions.
- The project should attempt to calculate the health/environmental externality financial cost confidence interval.
- The SOFC cost analyses need to be rationalized to the commercial SOFC cost data.

Project #FC-104: High-Performance, Durable, Low-Cost Membrane Electrode Assemblies for Transportation Applications

Andrew Steinbach; 3M



Brief Summary of Project:

The overall objective of this project is to develop a durable, low-cost, robust, and high-performance membrane electrode assembly (MEA) for transportation applications able to meet or exceed the U.S. Department of Energy (DOE) 2020 MEA targets. Objectives for fiscal year 2016 are to (1) produce project best-of-class components and catalyst-coated membranes (CCMs) via continuous pilot manufacturing processes; (2) validate performance and operational robustness of MEAs in short stack; and (3) evaluate MEAs for performance/cost modeling and durability under accelerated stress tests (ASTs) and load cycling.

Question 1: Approach to performing the work

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

- The project aims to overcome issues with the nanostructured thin film (NSTF) structure in order to take advantage of its inherent benefits. While the benefits in terms of corrosion resistance and high specific activity are considerable, several years of funding have now been expended in an effort to overcome the limitations. There has been considerable progress, but reaching the project goals seems unlikely at this point. There has been extensive testing and a reasonable level of characterization and diagnosis. Although the presentation lists an approach to identify mechanisms of unanticipated component interactions through advanced diagnostics, it is not entirely clear what was done here aside from operational studies and linkages to membrane degradation. These are good but not entirely sufficient. The performance drops off as the cathode oxygen reduction reaction (ORR) decreases below 10 mA/cm^2. This behavior looks similar to conventional supported catalyst losses at low roughness factors/low activity. Losses were stated as being due to intrinsic specific activity loss (structure, composition); extrinsic (contamination); and coarsening/dissolution. These seem to be all the same factors that affect more conventional designs but with the added challenge of the lower surface area. The NSTF catalyst seems to be converging with conventional catalysts at these low loadings with little inherent benefit and with reduced design space levers. However, there should still be a benefit of no ionomer required, which may alleviate the thin ionomer transport losses but the extent of which is not clear. Since the challenges of NSTF catalysts and more conventional Pt/C catalyst designs at low loadings are converging, understanding gained under one system may be applied to the other system with the differences helping to elucidate effects. More use of models and more fundamental understanding on the limitations with additional diagnostic approaches may have been helpful. In terms of stated mitigation approaches, the approach to decrease 3M perfluorosulfonic acid (PFSA) polymer electrolyte membrane (PEM) decomposition rates should be relatively easy to test through the use of PEM stabilization additives. It is not clear if this has been done. While the stated mitigation approaches address the kinetic activity losses, these losses affect only 30% of the performance loss. It is not clear what approaches will address the over-70% of mass transport loss observed. The external contamination effect remains a real risk for the NSTF catalyst. An assessment of the level of risk for this effect compared to a high-surface-area catalyst should be established. Improved approaches to understand proton transport in the NSTF layer would have been useful to potentially design a better layer or to leverage the understanding to other catalyst layer designs.

- The researchers have provided General Motors (GM) with MEAs for independent testing of the NSTF 3M MEAs. This is a much-needed independent validation test of the materials. The researchers were unable to reconcile the differences in MEA performance.
- The effort let by 3M to address fuel cell cost has been well designed and proven to be feasible. NSTF-based catalyst supports have been very well characterized. The implementation of this technique is extremely effective at addressing the issues with catalyst loading and power density. The issues of durability with this system have been in question for some time; the system may not be able to address the durability targets.
- This project continues incremental improvements to NSTF MEAs relying on dispersed-catalyst interlayers to improve robustness, giving the anticipated decrease in durability against voltage cycling versus NSTF alone. Apparently the growth of longer support whiskers, which could increase the electrode thickness for better operational robustness while maintaining the durability of NSTF alone, has not proven feasible.
- Within the limits of the NSTF MEAs that 3M has been evaluating stubbornly for the last 17 years, the approach of fine-tuning and juggling is fine in the attempt to find some progress that makes the materials competitive with traditional MEAs using supported catalysts. Testing in short stacks is a good way to show the progress or lack thereof very clearly.
- Status of each performance metric is clearly identified against the DOE targets. Identification of mechanisms for unanticipated component interactions through advanced diagnostics is not matured or demonstrated sufficiently to ensure all MEA targets will be achieved by the end of the project.

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.

- The accomplishments and progress on addressing fuel cell cost are very good. This is only addressing a few barriers to fuel cell implementation.
- A good deal of work was accomplished; however, it was not sufficient to fully resolve major issues with the NSTF platform: load-cycle durability at rated power density as well as robustness in transients at ambient temperatures. Work over the past year focused mostly on mitigating solutions via modifying individual components outside the NSTF electrodes, which still did not prove sufficient to meet robustness or durability targets. It is quite unfortunate that the researchers found fundamental root causes of low polarization performance in stack due to elevated overpotential in the anode quite late in the project. Using alloys with transitional metals, especially at the anode side, may change reference potentials and promote leaching of ions. Activation of both electrodes should have been addressed in greater detail early in the project.
- Overall, progress continues on the use of the NSTF catalyst-layer design, but over the past year, progress appears to have slowed with no real new advancements. Accomplishments include the following: 12 MEAs tested at pilot scale with good reproducibility; increased mass activity and specific area achieved at pilot versus laboratory scale; significant improvements in low-temperature performance with the reduced hydrophobic backing treatment (X3); and the interlayer concept's enablement of the ability to achieve load transients—Type B selected with 16 $\mu g/cm^2$. This year saw down-selection of designs and increased characterization and testing; robustness targets have not been met, but improvements have been made. The design with M catalyst has further small improvements and is getting closer to targets, designs with interlayers pass DOE AST targets, single-cell testing load transient data provides similar results between GM and 3M, and improved robustness for best-of-class (BOC) MEA is confirmed. A number of issues remain including the following: durability of less than 800 hours during load/relative humidity (RH) cycle for 10% degradation in performance (30% kinetic losses and 70% mass transport losses); lower performance in single-cell testing at GM (60 mV), even though 3M testing shows expected performance; and disappointing short stack evaluation results with much lower polarization curve performance. The stack transient performance was also much lower than expected, and cells failed at 70°C, 100% RH. However, the NSTF baseline CCMs passed, though there was still some instability observed. Therefore, more optimization and understanding are needed; conditioning ineffectiveness may have been a contributing factor, and the project is actively working on and making progress on improved activation procedures.
- The results were a bit disappointing. GM was not able to validate the best performance results. The dealloying catalysts do not seem to provide benefits over conventional catalysts. It still remains to be seen

whether the researchers can translate their single-cell results into stacks. The activation protocol appears to be difficult to reproduce and may not work in stacks.

- The accomplishments are minimal but acceptable considering very few knobs left to tweak after a decade of work on very gradual improvements to arrive at the state in which the project finds itself today. The negative results in stack studies can be blamed, as usual, on poor break-in/conditioning/thermal cycles in GM's test stands. It is not surprising, and 3M MEAs have never worked in a PEM fuel cell stack under practical PEM fuel cell conditions. If they had, automakers would be using them in their fuel cell electric vehicles (FCEVs). Although this year's project is a freebie, getting GM or anyone else to evaluate 3M MEAs in PEM fuel cells could be a distraction.
- The new BOC MEA has given mixed results, performing poorly at high current density at GM (perhaps owing to problems with proper break-in technique) and not yet meeting DOE 2020 loading, specific power, and load cycle durability tests. The 30,000 load cycle test could probably be passed with a slightly higher loading of interlayer, but MEAs of interlayers will likely never match the durability of base NSTF in ASTs. The new BOC MEA with interlayer also failed short stack transient testing at GM while, to the surprise of all, the NSTF baseline, without interlayer, passed. Based on the project results, it is not clear that continued investigation of NSTF is going to get to the ultimate DOE targets and automotive targets for operational robustness.

Question 3: Collaboration and coordination with other institutions

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

- It is always an excellent idea that 3M gets its NSTF MEAs tested in automotive stack environments by independent parties. It exposes the severe shortcomings of their NSTF catalyst that is well known in the automotive fuel cell industry in a fair and transparent manner. Owing to a variety of folks providing data, the units used within each slide vary from atmosphere absolute and kilopascal absolute, depending on the scientist plotting the data. The SI system was designed to be used as absolute units, and kilopascal is more accurate. Kilopascal gauge and kilopascal absolute are not real, valid units. Atmosphere absolute is also obsolete. Gauge and absolute pressure are used only in the English system units such as pounds per square inch gage and pounds per square inch absolute.
- 3M has strong collaborations with various universities and DOE national laboratories and a good strong partnership with GM. The effort is well coordinated. It is not clear whether cathode catalyst alternatives to Pt-Ni are being evaluated.
- The work shown in this presentation appeared to have been done at only 3M and GM, with modeling done at ANL. Perhaps the other subcontractors completed their work prior to this last (extended) year of the project. Completion of stack testing at GM has required a year's no-cost extension of the project.
- The reported year collaborations, as presented, were focused on the GDL partner and modeling efforts and stack testing at GM.
- GM data was a bit limited compared to what 3M provided.
- The project has a strong team.

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 **2.7** for its relevance/potential impact.

- The research effort was needed, as the NSTF MEA performance was difficult to reproduce between investigators. It remains to be seen if this type of MEA will meet 2020 performance targets.
- Although early on the technology showed exceptional promise to meet the low-loading and durability targets, the continued problems with operational robustness and the durability results under load/RH cycling are reducing the probability of success with this design. At this point, it is considered unlikely that the NSTF will be the design of choice for future automotive stacks. However, the value of the work could still be reasonable if increased modeling and diagnostics were incorporated to learn from the design.

- The base NSTF system has sufficient durability virtues that attempts to patch its shortcomings in operational robustness have been justifiable. The addition of an interlayer to improve transient and cold-start conditions raises sufficient durability issues that extended testing was appropriate. The modest (if any) net gains over the past year suggest that further work along these lines might not have much impact on the industry.
- If DOE asks all the FCEV companies for their input on whether these 3M NSTF MEAs are suitable for automotive stacks, DOE will receive a close-to-unanimous vote. 3M stacks have been evaluated in a number of automotive companies with no success; the MEAs used today in stacks of cars that are on the road or about to be on the road are typically PtCo/C, as openly reported.
- While the relevance and potential impact of the project remains high, the speed of the progress toward achieving the goals and, therefore, being accepted for commercial distribution is slowing down.
- The project aligns with the goals and objectives of the DOE Hydrogen and Fuel Cells Program (the Program). Insufficient information was presented to assess whether the approach to address durability in the NSTF-based catalysts will have the impact to address the Program's goals.

Question 5: Proposed future work

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

- The proposed future work focuses on stack operation and a down-selection of MEA. This project will meet the MEA cost goals.
- The project has essentially ended with only a few months left. The stated future work is appropriate.
- This project is almost complete. Future DOE-sponsored work would be on catalyst development under a new project reviewed elsewhere.
- It is really difficult to assess the effect of the project's future work on progress, but all indications are negative unless they drastically change the NSTF catalyst layer to a hybrid of some sort. Only modest modification of the NSTF structure was allowed, according to 3M (on slide 18). The likelihood of success with this constraint is low.
- The mitigation strategy to develop ionomers to minimize contaminant generation proposed for future work would again shift the focus away from the NSTF electrodes into different component development and should be avoided. Focusing on maturing activation procedures for both electrodes as well as transferring this technology and its early diagnostics to the stack project partner is essential to further narrow discrepancies between 3M and GM testing.
- There is much future work needed to replicate the previously reported data.

Project strengths:

- The project has a strong team with excellent industrial partners and testing under relevant conditions. The design provides an opportunity for additional understanding around issues with low catalyst loading at high current densities.
- The project develops significant insights on the NSTF-based electrodes for application in the MEAs in PEM fuel cells. The status of all project metrics is clearly identified against DOE targets, and if successful, this project has a high potential impact on the automotive fuel cell industry. Findings in the project have high and synergistic values narrating issues with thin film electrodes for performance at mass-transport-limited power densities and transient behavior.
- It is a strength to have a project where NSTF catalysts are evaluated by an independent laboratory. The candidness of the current 3M researchers in reporting less-than-spectacular results is appreciated.
- The project continues diligent and well-thought-out work on incremental improvements to NSTF MEAs towards meeting DOE's ultimate targets and automotive requirements on operational robustness.

Project weaknesses:

- NSTF-based MEAs remain highly sensitive to practical operational aspects of PEM fuel cells in load-following applications specific to thermal and load transients, start-up, and ability to demonstrate required power density, which limits the industry appetite to test these MEAs in stacks. Several aspects addressed by

3M for robustness to ambient temperature and operating power density at the beginning of life were quite derated in durability testing, showing insufficient mitigation adopted to resolve the fundamental issues.
- The project approach relies heavily on addressing each challenge incrementally and does not appear to do sufficient analysis of underlying issues. Inclusion of modeling with increased predictive and mechanistic exploration would have been useful.
- The gains of the project over the past year have been marginal, at best, suggesting that NSTF may be reaching a limit of diminishing returns without a major reworking of the system.
- The inability to translate single-cell break-in protocols to stacks was a major weakness.
- The focus only on the Pt-Ni cathode catalyst was a weakness.
- Only modest modification of the NSTF structure was allowed, according to 3M on slide 18. The likelihood of success with this constraint is low to negligible.
- Collaboration with an FCEV auto company shows clear evidence that this project, which has lasted a long time, has run its course.

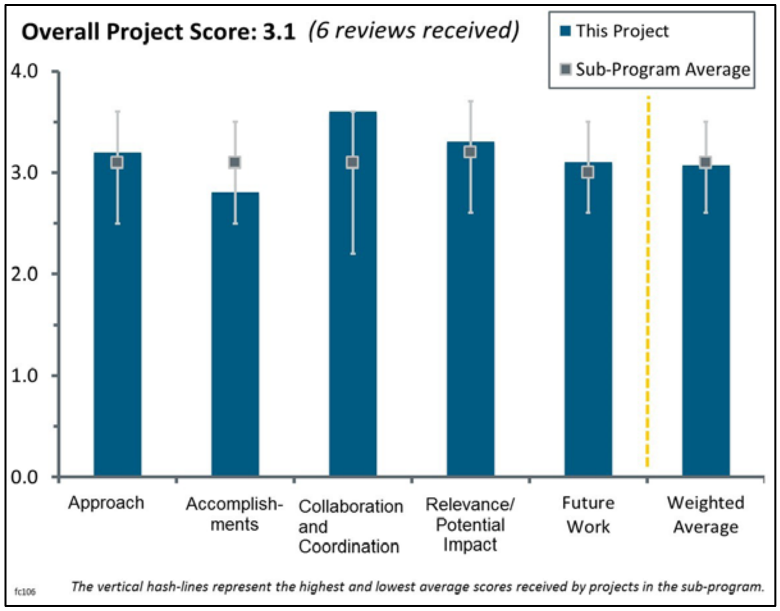
Recommendations for additions/deletions to project scope:

- The team should be refocused on polishing activation procedure and commissioning diagnostics for both electrodes as well as technology transfer to the project partner. Synchronization of the short stack size and flow fields between 3M and GM testing is desirable to ensure similar test results. The project should avoid shifting focus on the additional components, such as development of new ionomers for the MEAs, but rather investigate the model systems for sensitivity of electrodes to contamination.
- The project should continue studies on why the break-in procedures do not work in stacks.
- The team should complete the project and move on to changes to the basic support structure of NSTF or to non-NSTF MEAs.
- Small modifications to the scope will not be of any help after 17 years of development.
- The project is essentially complete.

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 platinum-based cathode electrocatalysts in membrane electrode assemblies (MEAs) and stacks operating at high current densities and on air and at low-platinum-group-metal loading. Specific goals are to (1) determine the electrode/catalyst property that limits the high current density/air performance of electrodes based on advanced platinum-based cathode catalysts; (2) use information from characterization efforts to determine the performance-limiting property of the current d-PtNi electrode; and (3) design the catalyst layer composition and structure and support functionality to mitigate the performance limitations, guided by computational modeling.

Question 1: Approach to performing the work

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

- The approach used in this (by now complete) project has been very thorough, from catalyst pre-treatment to MEA processing. Several key factors for the cathode/MEA performance were investigated, with the focus on improvements to the high current response of dealloyed PtNi catalysts. The project generated considerable output, which best attests to the value of the approach taken.
- The approach is perfectly adjusted for improvement of MEA performance with utilization of a complex dealloyed PtNi/KB catalyst. The characterization methods were selected based on determination of main parameters of MEA fabrication that will affect the overall performance.
- The approach is a good combination of careful analyses of a relevant materials set.
- Taking on the issues of catalyst cost, performance, and durability is highly relevant for fuel cell systems. The specific approach of this project has three components: determine electrodes/catalyst properties that limit high current density performance (not particularly compelling as conveyed), use characterization to determine performance-limiting properties (good science but unclear how it relates back to improved performance and durability), and design catalyst layers to mitigate performance limitations (good science in some areas but, like the inks, less compelling in the area of catalyst supports). These are all highly relevant pursuits, but they are difficult to accomplish, and it is unclear how effective any of the proposed approaches have been at advancing the state of understanding or performance. The approaches applied are fairly empirical and have limited impact potential, but by starting with Johnson–Matthey Fuel Cells Inc. (JMFC) state-of-the-art materials, the approaches have good performance as a starting point. Much of the work seems to be focused on lower loading of materials developed/demonstrated on earlier projects.
- The project focuses on important factors that limit the performance of dealloyed PtNi catalyst, namely the ionomer distribution, carbon/ionomer agglomerate structure, and leached Ni effect. The uses of in-cell diagnostics and advanced ex situ techniques such as ultra-small-angle X-ray scattering (USAXS), X-ray absorption spectroscopy (XAS), and transmission electron microscopy (TEM) were interesting. This is mainly a characterization project with limited material development effort. Electrode development appears

to be a shotgun approach with little depth into each path. The electrode/ink optimization study in this project has little value to experienced MEA integrators. Target/milestone settings are somewhat arbitrary and did not align with the DOE targets or heat rejection criteria. Overall, the targets were quite modest.

- The approach is reasonable; however, dealloying is a bit questionable for improving the performance of Pt. The baseline annealed Pt is a questionable experimental control, as it may not be the optimal 5 nm Pt catalyst. It would have been preferable for the researchers to use an as-prepared 5 nm Pt catalyst with a narrow particle size distribution. Annealed samples tend to display log normal particle size distributions that may grow faster than monotonic dispersions.

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.

- During the last year of the project, final experiments on integration of PtNi/KB catalysts into MEA structures were performed via optimization of ink composition, modification of support, etc. These experiments allowed the project to complete the matrix of proposed tasks and obtain the crucial information on MEA performance with a new generation of catalysts.
- The project was needed to validate the value of the dealloyed catalysts. The performance gains observed in rotating ring-disk electrode (RRDE) measurements have not translated to improved fuel cell performance. The development of advanced characterization and modeling techniques within this project will benefit future Fuel Cell Technologies Office (FCTO) investigations.
- Good progress was made, with all reviewer comments addressed.
- This is the last year of this three-year project, the objective of which was to "to realize the ORR mass activity benefits of advanced Pt-based catalysts." The team has met both fiscal year 2016 performance milestones, except for the durability using trapezoid cycling up to 0.95 V (a 22% performance loss in peak power, greater than the 10% target). The benefits of dealloyed PtNi catalysts are unclear; at the beginning of life, the catalysts already suffer from significant high-current-density losses (catastrophic at 30% relative humidity [RH]) and trail An-Pt/C in cycling durability testing, not meeting the targets for electrochemically-active surface area and mass activity (unless a less-demanding General Motors [GM] cycling protocol is used). There are no conclusions regarding the viability of cathodes based on the dealloyed PtNi catalysts, which one would expect at this point. There are apparent differences in the durability testing at the United Technologies Research Center (UTRC) and JMFC, with the results of the latter attesting to a noticeably better d-PtNi/C performance than the UTRC test data, even after cycling up to 1.0 V. This is confusing. Sharp improvements in mass activity and "negative" cell voltage loss in UTRC durability testing of the An-Pt/C catalyst are puzzling. They ultimately result in better mass activity and higher cell voltage at end of life than at beginning of life, which needs explanation. Comparison with similar data for dealloyed catalysts strongly favors the Ni-free system. The question of the PtNi catalyst viability thus remains unanswered.
- In comparing the summary of results from the 2015 presentation to the 2016 presentation, it seems that the new additions for this review period are the use of lower equivalent weight ionomer inks and low-loaded performance status. (Slide 19, which is a summary of results, is essentially unchanged from the 2015 presentation, and this overlap is reflected in some of the content presented for this review period.) There are certainly new data in the presentation that go beyond this, including areas of microstructural analysis and limiting current measurements. It would have been preferable to have more conclusions from this work with more information on mechanisms and what can be applied in future systems from what was investigated here. In general, there was too much focus on data instead of increasing knowledge of the systems.
- Broadly speaking, it is not obvious what was learned from this project that was not already known prior to the project. The project further illustrated the effect of leached Ni in many ways but failed to quantify the effect, propose a solution/criteria, or develop a solution. USAXS on the inks and TEM on the electrodes were quite interesting. However, these samples were not prepared from a process that an experienced MEA integrator would carry out. It is dubious how useful these learnings are. MEA testing appears to be the bottleneck of this project. Only a handful of tests were carried out throughout the project. Most data have

only one data point, making interpretation of data quality very challenging. The project largely meets the milestones, but the targets seem modest.

Question 3: Collaboration and coordination with other institutions

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

- The project is a fine example of coordinating multiple laboratories toward a common goal. Characterization work is excellent.
- The team has excellent participants well known in precious metal catalyst design, MEA fabrication, and characterization.
- This project has involved many partner organizations with complementary skills and well-defined roles. It appears to have been very well coordinated, too.
- The team arrangement is good, with JMFC providing the catalyst and MEA, UTRC testing the MEA, and the University of Texas at Austin doing TEM. However, with the involvement of JMFC falling short of expectation and with the loss of UTC Power, securing resources and know-how must have been quite challenging. It is good to see some continuation with GM as a consultant.
- The integration of work is excellent and seems to be due to good collaboration. The modeling could be better integrated.
- The team is strong. Including GM as an in-kind contributor is a strong addition, although GM’s role is not completely clear.

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 presented research on catalyst layers is highly relevant to FCTO goals and objectives. It focuses on the issues of utmost importance to fuel cell systems for automotive applications, directly targeting improvements in performance and cost reductions.
- The major value of the project was showing that the dealloyed Pt-Ni did not show major performance improvement over Pt.
- The project addresses an important challenge in achieving high power on highly active catalysts.
- The project allowed for a very promising material to be examined in more detail and at lower loading. Some progress was demonstrated toward achieving a number of the DOE targets.
- The project aims to benchmark and understand relevant issues in electrode construction using technical catalysts.
- The project was ended successfully, meeting of all the major milestones and go/no-go.

Question 5: Proposed future work

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

- As the project has essentially ended, this is less of a concern than other areas. Work focused on d-PtNi/C is highly desirable (in particular, Ni-ion issues, and mass-transport and low-RH issues). Next steps in characterization/understanding are of some interest but are less compelling (X-ray tomography, USAXS, electrochemical impedance spectroscopy [EIS], and cell performance data).
- Proposed future work is relevant to initially proposed targets (and will be done during a no-cost extension).
- The project has ended but proposed some continuation to the Fuel Cell Consortium for Performance and Durability (FC-PAD) and a new GM project. It would be ideal to use the newly developed techniques on other relevant materials under the new projects.
- The project has ended; remaining work is aligned with FC-PAD.
- Proposed “next steps” (rather than “future work,” as the project effectively ended in March 2016) are logical, stemming from the work performed to date. There is some doubt whether simple measures, such as

development of catalysts with “more uniform morphology,” are going to be effective in rectifying severe issues facing dealloyed PtNi/C catalysts and possibly other dealloyed catalysts as well. There seems to be little understanding of the causes for poor performance of such catalysts, especially at low RH. Future efforts in the field should concentrate more on understanding, even if less routine testing were to be performed.

Project strengths:

- The project has generated an impressive number of data and helped identify several challenges confronting dealloyed Pt-based ORR catalysts. This should help to better focus future efforts in the field and ultimately result in improved stability of such binary catalysts.
- This is a systematic study with good cross-functional collaboration. The uses of advanced characterization (USAXS, XAS, and TEM) have added visibility to this important challenge.
- The project has good teaming with a technical catalyst manufacturer. Characterization and analysis are excellent. The emphasis on MEA-based testing is a strength.
- The team is strong. Materials are highly developed and highly performing.
- This is an excellent collaboration for performance testing and materials characterization.

Project weaknesses:

- The MEA testing constraint is a weakness. It is unclear whether the studied materials set is relevant to the state-of-the-art process/materials. Details on the inks and electrodes are not disclosed, making the findings mostly meaningless to anyone else. Material development is limited; this is mostly a characterization project. The project is unlikely to provide any solution.
- Lack of error estimates on some of the summary data makes it hard to validate conclusions, for example, in slide 10. There is no automotive partner for testing and cutting-edge MEA, but this seems to be corrected in the rolling into FC-PAD. Acid washing of electrodes is likely not a viable approach, and in addition, result interpretation is highly questionable because of convoluting effects.
- The main project weakness is relatively little insight into the reasons for observed phenomena, such as losses in the d-PtNi/C cathode upon cycling and poor performance at low RH. At this stage, one would also expect a more definite statement about the feasibility of the d-PtNi catalyst, given its performance limitations identified in the project.
- There are limitations in the knowledge gained from this project. It is more a data-mining activity than one that provides insight into mechanisms or alternative approaches for improved performance. Little was presented on the “rational design” of electrodes.
- The fundamental hypothesis that dealloyed Pt-Ni would be a better catalyst than Pt was probably incorrect.

Recommendations for additions/deletions to project scope:

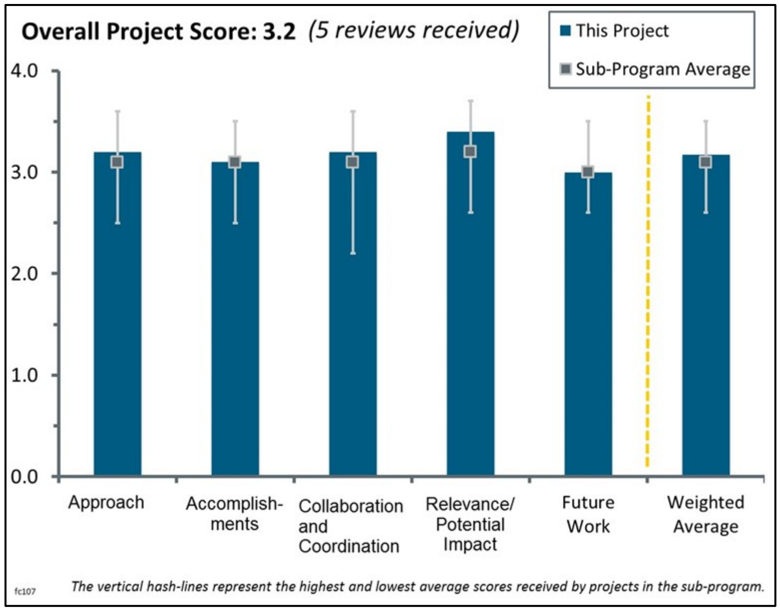
- The cells used were of triple serpentine flow channels, which are known to have back pressure and other issues. Other designs, such as the Ballard system developed under DOE funding, should be used.
- Research of dealloyed catalysts is worth continuing with greater focus on understanding, knowledge-based interpretation of the test data, and direct feedback into the design of next-generation catalysts.

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 overall objective of this project is to advance platinum-group-metal-free (PGM-free) cathode technology through the development of new materials and implementation of novel electrode concepts, together resulting in (1) high oxygen reduction reaction (ORR) activity, viable for practical automotive systems; (2) practical catalyst durability; (3) high ionic/electronic conductivity within the cathode; and (4) efficient oxygen transport and effective removal of the product water.



Question 1: Approach to performing the work

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

- The approach is relevant to U.S. Department of Energy (DOE) targets and objectives and directly addresses the barrier of high fuel cell cost by pursuing potentially low-cost catalysts to replace PGM catalysts. The approach addresses the barrier of durability by addressing PGM-free catalyst durability. Los Alamos National Laboratory (LANL) combines a practical approach to improve activity and performance with computational effort and some experimental studies to try to help determine what species are responsible for the ORR activity. The recent shift to increase the level of work on non-Fe PGM-free catalysts is in line with automotive original equipment manufacturer recommendations and concerns about Fe leaching's leading to membrane degradation and short lifetimes. The approach is multifaceted, with effort directed at trying to identify and characterize the active sites, efforts at improving mass transport, and efforts to improve activity through synthesis efforts at what appear to be three different organizations. It is not clear how much the characterization and modeling are influencing the synthetic approaches. Last year's modeling effort seemed to imply a bimetallic site (e.g., Fe_2N_5 or Mn_2N_5 or $CoMnN_5$) would be more active, but the synthetic approaches this past year did not seem to be directed at trying to obtain a bimetallic site.
- The multipronged approach provides significant value, including the focus on the following:
  - Catalyst activity – new synthesis routes with promising improvements in activity, porosity tuning, and good analysis on active site characterization and understanding
  - Durability – reasonable effort on using alternate transition metals to Fe, and evaluation of corrosion and fluoride release effects
  - Membrane electrode assembly (MEA) performance analysis – imaging with a computerized tomography (CT) scanner, which is an excellent approach, along with the linkages to models to optimize structure

  The stated approach to improve water management is important, but although there was good model-based investigation, there was not much evidence of experimental results or model validation in this area.
- The principal investigator's approach is quite effective in addressing all critical barriers in PGM-free catalysts, as evidenced by the publication and presentation record as well as by listed accomplishments in the Annual Merit Review presentation.
- The approach to improving kinetic activity is good. The quality of durability testing has improved, with more attention to higher voltages (e.g., 0.7 V RHE) than were used in earlier work out of Los Alamos National Laboratory. More attention needs to be paid to the engineering of thick (~100 µm) catalyst layers

with good transport properties while operating on air. Intermediate targets may be achievable with thinner layers, but to meet ultimate practical requirements, thick catalyst layers will be needed for non-Pt catalysts in acid. If it proves impossible to get good transport properties in such thick layers, work on this class of non-Pt cathode catalysts should stop. The 2018 target of 0.88 V at 0.044 A/cm^2 (in oxygen and iR-free) sets the bar way too low for practical applications and distorts the research effort away from what is really needed. More attention should be paid to higher current densities in air. Zn evaporation is an ingenious way of introducing porosity. Further data should be shown on whether this porosity improved the high-current-density performance in air.

- Modeling describes why Co- and Mn-based catalysts have somewhat lower activity than Fe-based catalysts. However, it would be hoped that LANL could also add ideas about how to enhance activity of Fe-free catalysts. Some of the catalyst layer modeling points toward a need to achieve at least five times greater active site density to reach 0.5 W/cm^2 at 0.6 V. The modeling is good, but what needs to happen at some point is for this to be translated into a high-current-density target, even if it is defined only within the project. A power density of 0.5 W/cm^2 is still not high enough. A better approach to this project than what has been taken would be to start with high current density in mind, not low. Although high open circuit voltages are important to gauge if the catalyst has any chance at all for application, the next step should be to figure out how to get the active site density that is sufficient for 1 W/cm^2 at 0.6 V. This would likely filter out a number of ideas and focus the project on the best ideas. Furthermore, it would create urgency to generate electrodes with low thickness. Data taken with thick catalyst layers (>70 μm) may not be representative of ohmic losses. It appears that acid leaching the catalyst has helped create greater durability, but this is not clear.

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.

- Good progress has been made, and high-value work has been completed. The catalyst performance activities have met project goals and are on track for the 2018 targets. Good work has been done on the non-Fe catalysts and model exploration to determine the relative activity of Fe vs. Mn, Co, and Ni. Preliminary tests on the effect of the Fe on the membrane degradation mechanism indicate that this may not be an issue. Further elucidation of this result is required to determine the importance of eliminating the Fe from the catalyst. Specific highlights and comments on the project's accomplishments include:
    - The Zn-induced microporosity work showed an impressive three-times increase in Brunauer–Emmett–Teller surface area.
    - The work to understand Fe atomic dispersion is good.
    - The improvement in durability was good.
    - The work to understand the carbon corrosion rate and fluoride evolution is good, but some of the conditions chosen to be reported are of low relevance. Although the corrosion rate at 0.6 V is similar to Pt/C, the rates are relatively low anyway and appear to be of baseline values, so it is not clear why presentation space was taken up with the bar charts on slide 16. However, the rate increases significantly in the region of concern at 0.9 V and is significantly higher than that of Pt/C.
    - Regarding the fluoride loss, a similar comment can be made. Membrane degradation is typically accelerated under open-circuit voltage conditions, whereas the plot on slide 16 shows not much difference in degradation between this condition and 0.6 V. Further explanation or work is required to understand this result and confirm the relative effects of the catalyst system on degradation. No information was supplied on repeatability of the results or measurement error.
    - The high-angle annular dark-field imaging scanning transmission electron microscopy and electron energy loss spectroscopy work provide the insight into atomic-level FeN_x sites, which is excellent work, and should yield good information for further understanding and development.
    - There has been some reasonable progress in the area of the CT scanner characterization, including analysis of the ionomer density and actual layer morphology. This technique appears to be very powerful.
    - The parametric model studies are useful, and it will be interesting to see whether experimental parametric studies will match model trends. Based on the tornado plot, the hydrophobicity effect

  - seems to be most critical to address for performance. However, this result does not appear to have any validation, and the probability of achieving this performance gain would be low.
  - The work on catalyst-to-solvent ratios provides some nice studies and analysis with useful results that should help to optimize the catalyst layer structure. There is still quite a mix of conditions shown, as well as a variety of membrane thicknesses. There have been improvements in this area, but it has not been completely addressed.
- LANL has made significant progress, increasing the performance of PGM-free catalysts, reaching activity of 0.044 A/cm^2 at 0.87 V, and making good progress toward the DOE 2020 target of 0.9 V at 0.044A/cm^2. LANL has made significant progress increasing performance in oxygen (increasing current density at 0.6 V from ~0.85 A/cm^2 to over 1.2 A/cm^2 in 1 bar O_2) and performance in air, where measurements were rarely made prior to the start of this project. Development of a magnetic separation method to remove Fe particles has been beneficial. The electrode voltage loss study and mass transport studies, including the ionomer loading modeling, have been beneficial
- This project is one of the most successful DOE-funded efforts in terms of novelty, achievements, and accomplishments.
- There has been good progress using side chains and Zn to enhance activity. However, the site density targets are still far from being reached. There is no line-of-sight described to reach active site density targets, and it is still very difficult to see how this will be anywhere close to being part of a commercial vehicle in the next 20 years. Durability of PGM-free catalysts is shown to be similar to Pt/C catalysts, but the reasons are unclear. Durability was shown on the basis of CO_2 and F^- emission rates, but it is not clear what this might mean for surface area, catalyst layer thickness, and the resulting performance. The slides do not make clear which catalyst sample was used for many of the data shown. Modeling work needs to be validated—in particular, the contention that increased hydrophobicity would increase power density needs to be validated through experiments. The same can be said for decreased ionomer tortuosity. Catalyst layer thickness should be reported to understand to what extent mass transfer limitations are playing a role in the data reported.
- The project appears to have achieved a modest increase in kinetic activity toward the self-stated 2018 target. LANL has been far too slow in proceeding to testing in decent-sized (5–50 cm^2) MEAs, even given the delays in implementation of subcontractor contracts. LANL appears to have made some progress in estimating active site densities through comparisons of experiment and model calculations, but the presentation did not explicitly emphasize this point. One of the major problems in PGM-free catalyst research has been the lack of methods to quantify the number of catalytically active sites. LANL has developed new ways to image, if not necessarily to quantify, the apparent FeN_4 active site. Significant improvements in durability at meaningfully high potential (0.7 V RHE) have been achieved by removing iron that is not properly coordinated with nitrogen. The presentation should have paid more attention to whether peroxide was produced during ORR, particularly for non-Fe catalysts.

Question 3: Collaboration and coordination with other institutions

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

- Collaboration between the partners appears to be good. The characterization efforts and modeling efforts appear to be working well together to help inform the electrode preparation and help define loading for MEAs. Some collaborations outside the team are evident, with publications including some authors in the field outside of the current team.
- This project can be used as an example of how collaboration should be organized, coordinated, and executed.
- Los Alamos National Laboratory collaborations with the University of New Mexico, Argonne National Laboratory (ANL), and Oak Ridge National Laboratory (ORNL) appear to have been good in the past year. The presentation did not make it clear whether interactions with other subcontractors have been effective.
- The General Motors (GM) aspect of the collaboration was not clearly shown this year. The same can be said for the University of Waterloo and the University of Rochester. Most recent collaborations appear focused on ORNL and Carnegie Mellon University for characterization of powders and layers. The FeN_4 site found by ORNL is an interesting contribution. Some collaboration between Carnegie Mellon and IRD Fuel Cells (IRD) appeared to help thin out the ionomer and improve performance. It would be interesting to

know more about the University of Buffalo catalyst. It is not clear what the structure of the "Fe-MOF" is, how durability could be improved, or how higher power density (higher active site density) could be generated.

- This is a large project team with what appears to be good coordination between groups.

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 has high relevance and potential to significantly reduce the cost of polymer electrolyte membrane fuel cells (PEMFCs). Replacing PGM catalysts with PGM-free catalysts could be a potential game changer, as the catalyst costs are currently estimated to account for approximately 50% of the stack costs at high volume because of the high cost of the PGM. PGM-free catalysts are a key strategy to meet DOE's ultimate cost targets. The project also addresses the durability barrier, addressing durability issues with PGM-free catalysts.
- It is unclear whether PGM-free catalysts will have a role in automotive fuel cells, but advances achieved in the last few years are improving the possibility. The durability of the catalysts remains a major concern. The work is relevant, as the goal to minimize Pt from the PEMFC is a worthwhile one.
- From the very beginning, this effort can be considered as cutting-edge research and development of PGM-free materials. Progress made and achievements are striking, and DOE funding is perfectly justified by achieved targets.
- A project on PGM-free catalysts is relevant because precious metals have been shown to contribute a high percentage of cost to conventional PEMFC systems at high production volume. Technical targets that focus on low current density in an equivalent manner to PGM catalysts are appropriate. Throughout a polarization curve, similar power densities should be expected because of cost and vehicle packaging constraints. As with any PGM-free catalyst, durability targets are appropriate. One target is noticeably missing from the relevance slide is performance at high current density. Owing to the need to restrain the expense of membranes, gas diffusion media, plates, and other repeating parts, there should not be an expectation to increase stack active area to accommodate a PGM-free catalyst. Therefore, a high current density performance target is needed.
- This project has been tightly focused on the holy grail of fuel cell research: an effective ORR catalyst that is free of precious metals and is effective in acid. The impact of the project would have been greater had LANL paid adequate attention to the engineering of thick catalyst layers with effective mass transport. The use of Fe in such catalysts has been challenged on the basis of Fenton's degradation of membranes. Because LANL appeared to have generated catalysts with Fe only in the active FeN_4 sites, it should have addressed the question of whether Fenton degradation of membranes is avoided when Fe is restricted to only those active sites.

Question 5: Proposed future work

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

- The team has laid out the important future work to be addressed, with some promising approaches for activity and durability. Increased focus on understanding the durability mechanism is important.
- This is the last year of the project, which will continue in differently formatted funding. For that reason, the proposed future work, which will rely on full utilization of research capabilities across national laboratories, is a logical continuation in strategy to further advance PGM-free systems.
- The project is complete, except for a few subcontracts. Future work to complete the subcontracts is logical and should accomplish the remaining project milestones and goals.
- The project is almost complete, and therefore, the presentation did not concentrate on details of future work. The listing of remaining challenges and barriers is comprehensive but differs little from a similar list that would have been given at the start of the project. The listing of proposed future work gives little in the

way of specific innovations besides trying to induce strain in bimetallic catalysts as a means of increasing the activity of Fe-free catalysts.

- The project ended in March, with the exception of some tasks for IRD and GM. IRD is delivering MEAs to be tested at GM. Based on the results of the project, it is unlikely the MEA test will reveal a catalyst capable of meeting high-power-density performance required to displace precious metals. LANL would have done well to spend some remaining time attempting to validate the modeling. In particular, any validation of the options that might improve power density would have been worthwhile.

Project strengths:

- This is a large, multi-talented team addressing the work from various angles: modeling and characterization to elucidate the activity and durability mechanisms, use of novel characterization methods to link models to actual structures, and an industrial partner to provide relevant MEA preparation and testing.
- The project demonstrated progress on durability, apparently through avoidance of Fe in forms other than the FeN_4 active site. The project showed some progress on estimation of active site densities through comparison of experiment-based calculations with model calculations.
- LANL is aware of many of the fundamentals of fuel cell testing and in situ diagnostic breakdowns (e.g., high-frequency resistance and limiting currents). LANL has access to many universities, suppliers, and developers for collaboration. LANL has made progress compared to where the technology was five to ten years ago. This does not mean LANL is on a trajectory toward meeting application targets, but it does have some ability to make improved materials. LANL has access to advanced characterization equipment through both universities and national laboratories.
- All aspects of this project have been executed in the most effective manner, as evidenced by the publication list and project goals achieved.
- Over its lifetime, LANL made significant improvements in PGM-free catalyst activity and durability.

Project weaknesses:

- LANL has focused too much on low current density rather than high current density. LANL has focused too much on short-term goals and objectives rather than attempting to think seriously about what a catalyst would need to be to be suitable for an automotive fuel cell stack. There is still too much reliance on Fe for activity. LANL should have attempted to expand upon the activity found with Co and Mn. There is not enough explanation behind why durability has been improved. From the perspective of the presentation, the work may have been empirical in its understanding of durability. Models need to be validated to understand what the future paths are toward improving power density.
- The project paid inadequate attention to the engineering of thick catalyst layers with adequate transport properties. The project demonstrated modest, if any, net kinetic activity gains. The project set too-low intermediate targets that could be achieved without the transport-challenged thick catalyst layers that would be needed for practical applications.
- There is some inconsistency in data and conditions. The project has broad scope, with promising results in a number of areas. Further in-depth studies in each of these areas could have been useful, indicating the resources may be spread too broadly.

Recommendations for additions/deletions to project scope:

- For any succeeding projects, it will be critically important to look at the power density that can be generated with a precious-metal-free catalyst. This project looked perhaps too hard at low current density and not enough at current densities that exist around 0.6 V. Catalyst layer thicknesses should be reported, and they should ideally be minimized. The project was probably not aggressive enough in attempting to reduce catalyst layer thicknesses. High-voltage cycling experiments should be done in addition to steady state holds. Durability of the materials is still largely unproven. Analysis to understand whether hydrogen peroxide is being produced is recommended.
- Any future projects on PGM-free catalysts should keep the nose to the grindstone explicitly in engineering 100-micron-thick electrodes with good transport properties. Because of the low density of active sites in

non-Pt catalysts, high activities per active site would be of little practical utility unless a large thickness of electrode can be effectively utilized.

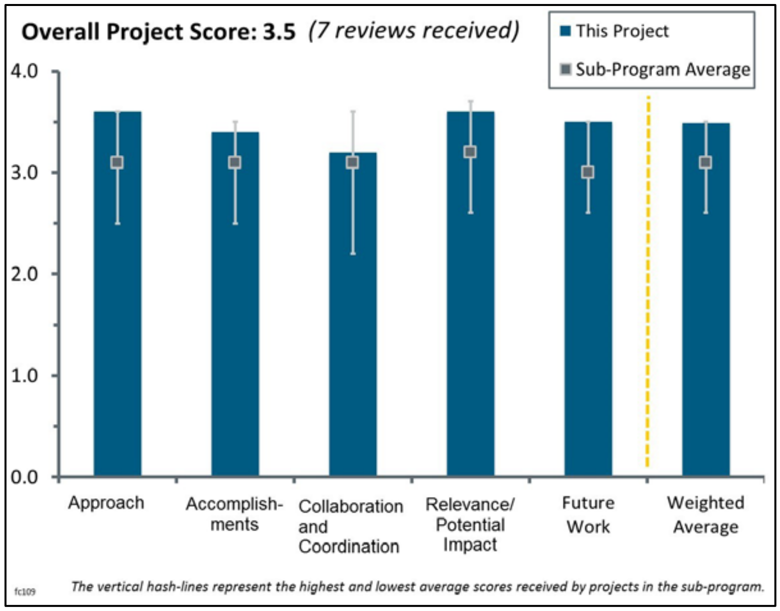
- The team followed last year's recommendations to run parametric analysis with the models to understand opportunities for catalyst layer optimization. Further validation of some of these effects is required. Much more can be done to understand and achieve catalyst layer optimization.
- The computational efforts should be continued into the Electrocatalysis Consortium (ElectroCat) and should be used more to help drive the synthetic approach and help determine ways to increase active site density.
- Recommendations include a well-justified approach that would tackle both fundamental principles that are behind the mechanism of operation and applied aspects of implementation of PGM-free systems.

Project #FC-109: New Fuel Cell Membranes with Improved Durability and Performance

Michael Yandrasits; 3M

Brief Summary of Project:

The overall objective of this project is to meet all of the U.S. Department of Energy (DOE) Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan membrane performance, durability, and cost targets simultaneously with a single membrane. Tasks include ionomer development, nanofiber development, ionomer and membrane testing, membrane electrode assembly (MEA) fabrication and fuel cell testing, dual fiber electrospinning, and stack testing.



Question 1: Approach to performing the work

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

- The project is very well conceived and well executed, focused on creating improved fluoroionomers for polymer electrolyte membrane fuel cell (PEMFC) applications. The approach of using monomer units with two or more protonic groups is now well established as a means of reducing ionomer ion exchange capacity while not sacrificing other materials properties. The approach is sound and has been logically and ably pursued.
- The approach to performing the work is excellent. 3M has laid out a clear and cogent plan that aims to simultaneously achieve all DOE membrane targets. By building on the multi-acid side chain approach developed in a previous 3M project, along with incorporation of robust supports and radical scavenging additives, 3M has met most targets.
- The project is aligned with DOE targets and goals. The project approach utilizing multi-acid side chains allows one to decrease equivalent weight (EW) while maintaining more mechanical strength than standard perfluorosulfonic acid (PFSA) materials with the same EW. 3M is focused on meeting all the targets at the same time.
- The approach is good. The project is investigating new lower EW PFSA materials. Performance appears to show good improvements.
- This project has an excellent approach that is making polymer electrolyte membrane (PEM) materials that have not been synthesized elsewhere.
- The approach is excellent for developing a low-relative-humidity (RH), high-temperature membrane. However, the ionomer in the catalyst layer should be addressed in parallel.
- The approach might well be better than the score, but these types of presentations are the hardest to judge; so much of the data is “Ionomer A, Support B, Additive C,” etc., and the reviewer really has no method of judging. The approach is incrementally improving the best PFSAs, which is definitely worth pursuing. It would be good to see more fundamental work on whether the nitrogen linkage in the perfluoroimide acid (PFIA) is viable. The project has some data that show it may not be and may poison the catalysts. This should be demonstrated as soon as possible; if it is not, further incremental improvements are a waste of time.

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 so far are excellent and show increased progress over the previous year.
- The project continues to make excellent progress. The principle achievement in the last year was the successful completion of milestone 8, which yielded a membrane that meets most of the DOE targets. The milestone 8 membrane could not quite meet the most aggressive area-specific resistance (ASR) target at 120°C, but 3M has done an excellent job of mapping out a path forward to meet this target, with projections showing that further reduction in ionomer EW could yield membranes of 10- to 14-micron thickness that meet all ASR targets. Notably, while 3M has done an excellent job of attacking this target, automotive original equipment manufacturers (OEMs) have indicated that high performance at 120°C is not strictly necessary because most OEMs would be reasonably satisfied with high performance at 95°C. A bigger concern is the possible durability impacts of the 3M membrane. While the membrane itself has successfully achieved DOE durability targets, membrane degradation products could negatively affect catalyst and electrode durability. These degradation products appear to play a role in the low observed open circuit voltage, which is probably due to adsorption of degradation products on the catalyst surface. As discussed in the presentation, the existence of multiple acid groups per side chain may make adsorption of degradation products from the 3M membrane a bigger issue than with conventional PFSAs. Going forward, this is an issue that 3M will need to address more thoroughly, but based on limited results reported so far, it appears that 3M has some ideas of how to attack the problem. The stack testing component of the project is not necessary at this stage. From a membrane development standpoint, it is not clear what stack testing would reveal that cannot be revealed in single-cell testing. Given the known issues with membrane degradation products, it would have made more sense to skip the stack testing and devote more resources to ionomer and membrane development. Furthermore, stack testing results will be strongly dependent on MEA performance, and little work has been done on MEA integration to date.
- The team has consistently met its marks in terms of project milestones and has generated useful insights into the advantages and limitations of its approach. The systematic approach to simultaneously increasing ion exchange capacity and reducing film thickness has led to incremental but impressive improvements in membrane properties. The project came up a little short on oxygen permeability and on ASR under the most aggressive conditions of 120°C and 40 kPa water vapor pressure (20% RH), but other than this, the team has provided excellent advances.
- 3M has made significant progress toward meeting the DOE membrane targets simultaneously. Most of the targets have been met, except for conductivity at 120°C and low RH. Recent work on supported membranes has allowed reduction in thickness to 10 microns, which improves water management and reduces ASR. The fiber distribution work should have a durability component (at least RH cycling) to show the effect of fiber distribution on mechanical durability. The fiber diameter study is interesting, but the larger-diameter fiber sample also had a higher fiber content (40% higher), which could account for the differences. This study needs to be repeated with more samples and closer control of fiber content.
- There has been very strong incremental improvement on the best PFSAs. There is a very systematic, well-thought-out approach on what it will take to meet targets using these systems. Some results, notably water uptake, were clearly just wrong, and the presenter seemed to be aware of this and should have looked more deeply.
- This project has promising results on a new membrane material. There are some questions about the relative stability of the membrane and the effect that the material has on catalyst activity that should be addressed quickly.
- It appears much of the progress toward the targets is a result of thinning the membrane from 14 microns to 10 microns. Thus it is not clear how much of the improvement in performance is due to a fundamentally improved material or simply the same-performing material at a thinner thickness.

Question 3: Collaboration and coordination with other institutions

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

- Project coordination with a key stakeholder OEM (General Motors [GM]) and a university providing novel nanofiber support structures is excellent and gives good guidance to the project.
- Collaboration with the university and GM are good. It appears GM is being used simply to test 3M materials and check whether the materials meet targets.
- The collaborations with Vanderbilt University and GM appear to be going well. GM durability testing is ongoing, and stack testing with the 3M membrane has been initiated.
- 3M really does not need to collaborate; the team is fully capable of characterizing, developing, and taking these products to market. It is really important to know what the different supports are in order to judge them against each other.
- The collaborations with GM and Vanderbilt add value to the project. GM's role in performing advanced durability studies, including peroxide vapor degradation and blister strength tests, is particularly valuable. The Vanderbilt nanofiber support studies are valuable in terms of improving understanding of support properties, but despite some interesting and informative results from Vanderbilt, it is not clear to what extent these results are actually feeding into 3M's nanofiber development.
- The collaboration between 3M and Vanderbilt appears to be valuable. However, the primary development rests solely on 3M, with GM conducting primarily validation and Vanderbilt working on nanofiber development. It seems like they have not been widely incorporated into the project.
- The work with Vanderbilt seems like an add-on; it was not clear what the work added or whether it was necessary. The role of fibers in controlling swelling and providing mechanical strength was clear, but it was not clear which fiber approach was best, or whether it mattered what fibers were used or how the fibers were used. Mention was made of polyvinylidene fluoride in fibers, which could create problems with peroxide stability—problems that were not 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 **3.6** for its relevance/potential impact.

- The project is highly relevant to the Hydrogen and Fuel Cells Program (the Program) goals and has potential to have a high impact. The 3M membranes have the best chance of providing suitable high-temperature (95°C–120°C) performance of any membrane currently under development. The 3M project also provides DOE with the most likely path to meet the aggressive ASR targets without sacrificing durability and mechanical properties. The biggest unknown in terms of the potential impact at this stage is how the membrane will impact MEA durability. 3M will need to demonstrate that the membrane can be incorporated into a MEA that meets MEA durability targets, but this work is beyond the scope of the current project.
- The project is highly relevant to DOE and to fuel cell development. Membranes with better performance at low RH can reduce system costs by eliminating the need for humidifiers and increasing power density. Membrane costs are a significant portion of stack cost at low production volumes.
- This project has consistently provided new materials that meet or exceed DOE/FCTO milestones. The team has been generous in providing materials to other DOE-supported workers. The project has had high impact.
- Membrane developments in this project are showing steady progress toward the DOE targets.
- This is an important and relevant project.
- An improved membrane and ionomer that works at low RH and high temperature would be enabling for the commercialization of automotive PEMFCs. However, the proprietary nature of the materials and additives is of great concern, especially as the project is using taxpayer funds.
- These are incremental tweaks and improvements. Having said that, they are improvements on the best PFSAs we have, so the work is definitely worthwhile. The project team members do not present a convincing case as to why their supports are needed/better compared to expanded polytetrafluoroethylene

(ePTFE). Ultimately, the team needs to address costs, especially if the team members think that is an advantage.

Question 5: Proposed future work

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

- Stack testing plans seem well planned and likely to succeed.
- Ionomer and nanofiber development are excellent next steps in addition to stack testing.
- The proposed future work represents a logical path forward. Some areas that should have been included on the future work slide are the further development of lower-EW ionomers with three or more acid groups per side chain, and studies of the ionomer degradation products. Both these issues were mentioned during the presentation, but it is not clear how much they will be addressed in the remainder of the project.
- The future work seems relatively straightforward; however, 3M could get more from its partners (especially GM) and also leverage others to help with understanding the material properties better.
- The work is very difficult to judge because so many of the materials are a "black box." The project is nearly done with project goals, however, and progress has been very good.
- The future fiber distribution work should have a durability component (at least RH cycling) to see the effect of fiber distribution on mechanical durability. The peroxide vapor chemical stability study should look into the membrane degradation route to determine the level of chemical suppression needed. PFIA side chain fragments are likely catalyst poisons. The degradation mechanism also has implications for the applicability of perfluoroionene chain extended 3 (PFICE 3) or PFICE 4 derivatives.
- When going to membranes as thin as 10 microns, more focus on membrane lifetime should be undertaken. It is not clear whether a "rainbow" stack is the best platform to study the lifetime of different membranes.

Project strengths:

- This is the best membrane project that the Program has funded to date. The project was thoughtfully designed and has an excellent chance of meeting most DOE membrane targets. While the most aggressive ASR targets probably will not be met, there is a clear path forward to meet them in future work.
- The approach is valid and novel in the area, and results seem to indicate that there is promise for the material. The strengths of this project seem to be in the synthesis of the materials.
- The path, metrics, and methods are very clear. All the right tests are being done with the proper controls and targets.
- 3M has strong polymer background, GM has testing capability, and Vanderbilt has expertise in electrospun fibers.
- The project has excellent materials, has a good team, and has made excellent progress.
- The project has an excellent team, which, with good coordination, leads to quantifiable progress toward DOE goals.

Project weaknesses:

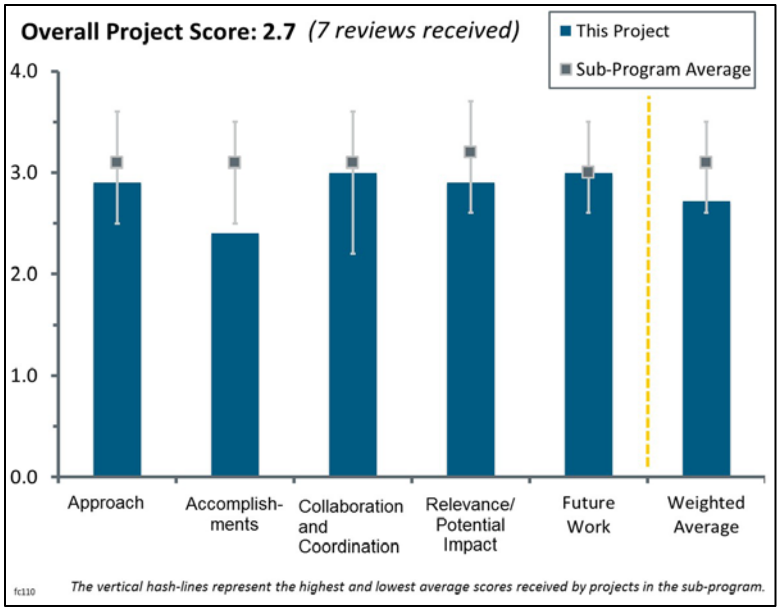
- The PFIA membrane degradation story is as yet incomplete. It is difficult to understand how valuable this membrane/ionomer is or could be without a better understanding of the effect that it seems to be having on the oxygen reduction reaction (ORR). It is not clear whether it is three times that of Nafion (because it has three times the SO_x groups) or whether something else is going on. Advanced characterization of the membranes—such as determining membrane crystallinity through small-angle x-ray scattering, wide-angle x-ray scattering, or other techniques—is missing to date and could potentially provide valuable information.
- Simply relying on reducing membrane thickness to reach the DOE goals is a risky approach.
- The proprietary nature of materials and additives renders the work not so useful to the U.S. taxpayer.
- The project needs a stronger argument on the support side. The advantages in performance or ultimate cost should be made clear.
- The stack testing task adds little value to the project at this stage.

Recommendations for additions/deletions to project scope:

- This year, 3M has addressed the question about the effect on catalyst activity, which is good; however, questions remain. There are multiple paths that 3M could pursue to help understand whether this quick open circuit voltage decay is a problem. Suggestions include providing some of the PFIA as an ionomer to conduct rotating disk electrode evaluation and comparing this to Nafion's effect on reduction in ORR kinetics (e.g., the National Renewable Energy Laboratory). PFIA could be provided to an organization that regularly conducts membrane accelerated stress tests, including degradation fragment analysis such as F and SO_x with water analysis and H_2 crossover with time (e.g., Los Alamos National Laboratory).
- The project should do more work on membrane lifetime in the 10-micron thickness range. It is not clear whether this is a feasible membrane thickness.
- The concentration has to be on the viability of the PFIA side chain. This needs to be cleared up before more work is done; it is the most important thing to demonstrate for acceptance of these materials.
- The project is scheduled to end in December, so changes to scope should not be made at this point.

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 superacidic inorganic functionalized monomers with (1) area-specific resistance (ASR) <0.02 Ω cm^2 at operating temperature of an automotive fuel cell stack (95°C–120°C) at low inlet relative humidity (RH) (<50%) and (2) 50 cm^2 membrane electrode assembly (MEA) with desired mechanical properties and durability. The current-year objective is to incorporate the best hybrid polymer system into an MEA and deliver a 50 cm^2 MEA with all desired properties for third-party testing.

Question 1: Approach to performing the work

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

- The principal investigator (PI) is testing multiple approaches to incorporating heteropoly acid (HPA) protogenic groups into membranes for polymer electrolyte membrane fuel cell (PEMFC) applications. This is a long-standing line of investigation from this PI. Most work from the past year focused on adding aryl phosphonate groups onto fluoroelastomer polymers via an unusual coupling reaction between phenol groups and a specific carbon in the vinylidene difluoride-hexafluoropropylene (VDF–HFP) linkage in the fluoroelastomer. This reaction has previously been used to provide crosslinking in fluorelastomers. HPA groups have multiple attractive properties, including high conductivity at low water activity and an ability to mitigate chemical damage from peroxide. The approach of attaching HPA groups to fluoroelastomers seems sensible, though the fluoroelastomer stability may need to be studied to understand how it compares to other materials in response to fuel-cell-like challenges.
- The multi-directional approaches taken by the team for the completion of all tasks are adequate. All the analytical techniques have been thought through appropriately. The study on material synthesis based on functionalized superacidic inorganic moieties is the personable approach. Stabilization of HPA in polymer matrix may be a great challenge. However, learning from first-generation membrane work seems to have helped the team in tackling the stability issue.
- Barriers are clearly addressed, with new performing polymers that show progress over state-of-the-art perfluorosulfonic acid (PFSA) ionomers.
- The approach of using anchored HPAs in a polymeric matrix has the potential for high conductivity under low RH conditions and could overcome barriers to high conductivity under high-temperature, low-humidity conditions. High-conductivity membranes can help overcome cost barriers by enabling simplification of balance of plant. Leaching tests still show loss of HPA (reduction from 70 wt.% to 60 wt.%). More needs to be done to determine whether additional leaching occurs, whether leaching is controlled by the equilibrium concentration in the leachate, or whether it is controlled by the fraction of doubly attached or singly attached HPA. ASR measurements should be measured directly through plane rather than calculated from in-plane conductivity. It is not clear why ASR is decreasing and then increasing with increasing temperature at 95% RH for some samples.

- Using polymers functionalized with HPAs is a promising approach to achieve high conductivity at low RH. There does not seem to be a systematic approach to meeting the requirements especially to reduce swelling and improve mechanical stability.
- The focus of this work has been on improving synthetic techniques to achieve DOE ASR targets with HPA-based membranes. However, given how much work the PI has already performed on HPA-based membranes without demonstration of a membrane that can actually be incorporated into an MEA, this approach does not seem promising.
- This group has been working on this approach for a very long time, and the members' thoughts and direction are not yet mature; they appear to still be stumbling through the dark to find something that will work. Each year, there are unsubstantiated, last-minute results with incomplete thoughts behind them. At this point, the polymer and HPA to be used should be clear and the team members should be perfecting and iterating. The fact that they are considering a polymer that splits on its own when thin demonstrates lack of background data; if the polymer can drive itself apart while drying, the source of hope to survive RH cycling is unclear. Beyond mechanical stability, the team does not give a very good justification for why the base, hydrocarbon-based polymer might survive chemically, other than that the HPA acts as a radical scavenger. If the team members have shown this, they did not show it here. Again, this is an extremely easy thing to demonstrate with traditional membranes.

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

This project was rated **2.4** for its accomplishments and progress.

- Recent progress with the HPA/FC-2178 system is excellent. The synthesis is refined and is yielding large amounts of high-quality material in good yield. HPA-loaded materials lose some HPA upon exposure to boiling water but retain much more and show high conductivity and low ASR even at 50% RH. The discovery that thin films have low ASR due to cracks is significant insofar as the crack problem may be relatively easy to fix by switching to a supported membrane, e.g., an expanded polytetrafluoroethylene (ePTFE), and/or by blending. An area that needs more attention is the film durability; e.g., the stability of FC-2178 upon challenge by peroxide before and after phenol addition and before and after HPA addition is not established and needs to be rigorously tested. Mechanical testing will also be important at some point.
- There has been good recent progress on polymer synthesis/membrane preparation, but overall progress has been delayed. Recent experiments have shown high conductivity and achieved the intermediate target.
- This project makes and characterizes new membranes that show improvements.
- It is understandable that the team could not achieve much this year because of the mishap that occurred in the laboratory. Despite this, the team seems to be on target to accomplish the project's 2016 goals. The HPA seems not to be very stable in the polymer matrix. In slide 12, the author mentioned that with improved processing (cross-linking), the loss of HPA is decreasing and is presently at about 60%. This means the HPA is not completely stabilized in the polymer matrix. The swelling property of the membrane is also not very good. From slide 14, it seems that there is still room for improvement in the membrane's swelling property by further cross-linking. The team needs to focus on the membrane's optimization to the extent that it gives desirable conductivity. It is not clear why the team chose to compare the project's conductivity data with N117, which is much thicker and does not respond to humidity change quickly. The team should use a standard 2-mil Nafion® membrane for conductivity comparison.
- Progress to date has been limited. Some promising results were reported in terms of possible low ASR from 70°C–110°C at 50% RH, but the questionable approach to ASR testing undermines these results. Colorado School of Mines (CSM) needs to test ASR in a through-plane setup. Calculation of ASR using in-plane conductivity is inappropriate, which was clearly demonstrated in the project's own results in which the existence of cracks resulted in an unrealistically high ASR estimate at 50% RH. Comparisons with N117 were made on several slides, but such a thick membrane is not relevant to the Hydrogen and Fuel Cells Program (the Program). A more relevant membrane with a thickness of 25 microns or less should be used. Properties such as chemical and mechanical durability and gas crossover have not been robustly addressed to date (aside from the Fenton testing), and they cannot really be addressed until CSM produces a membrane that can be incorporated into an MEA. Most of the HPA appears to be water-stable at 60°C, but the membrane HPA's ability to be retained in an operating MEA will need to be demonstrated. CSM

indicates that a viable membrane will be produced soon, but the project does not have a good history of living up to claims of future performance.

- Some progress has been made toward meeting the conductivity target. The conductivity data are inconsistent. After much effort, the project has been able to make films without cracks, but it does not leave much encouragement that the team will be able to make mechanically stable films with this chemistry. There are no ASR data; reported ASRs were calculated from in-plane conductivity measurements. There are no conductivity data at the targeted temperature of 120°C. There are no chemical or mechanical durability data.
- Despite the project being in its third year in the Program and ~10 years on these systems, the project team members are not even close to MEA testing let alone RH cycling, chemical stability testing, or open-circuit voltage testing. All they have done is some conductivity testing, and the presenter admitted that they do this poorly (bad ovens, not reaching temperature, etc.), and it is done only in-plane. The thought process on HPA containment is either not mature or was not well presented.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** 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) will be beneficial to the team.
- So far, this project is an excellent collaboration between CSM, 3M, and Steve Hamrock, formerly of 3M. Other partners are less involved at this stage.
- 3M seems to be providing valuable support with polymer synthesis and film formation. Neither NREL nor Nissan appears to have contributed to the project as of yet.
- There is good collaboration with 3M and a good approach to base work on available polymers.
- Collaboration has been limited to date because acceptable membranes were not being made. Now that a method for preparing acceptable membranes has been found, testing at the partner organizations can be initiated.
- Participation by 3M has been valuable in providing materials and in consulting on synthetic technique. NREL and Nissan involvement to date has been limited by the lack of a membrane with which they can work. Plans for their involvement going forward appear reasonable, though.
- Work with Dr. Hamrock is very good and should steer things for the better; however, it is uncertain how this technology will be eventually transferred or brought to market. Other partners are appropriate, but it was not made clear, for instance, what Nissan's role is.

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.

- This project provides a unique approach to solving the problem of poor membrane ionic conductivity at low water activity. The behavior of HPA proton donors is unique and quite different from that of organic acids, which are the usual protogenic groups in polymers. The quite high conductivities and low ASRs from these materials at low RH are difficult to achieve from other materials, and as such, the materials from this project offer much to forward the goals of DOE and the Fuel Cell Technologies Office (FCTO) in hydrogen energy conversion. There is also a high potential for producing materials at low cost.
- The project is relevant to the objectives of the FCTO Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The activities are aligned with DOE's goal to address the commercial barriers such as performance, cost, and durability. The focus of the project is to demonstrate a low-cost hybrid inorganic/polymer from superacidic inorganic functionalized monomers. This is an alternative low-cost approach to develop a low-RH (inlet <50%), high-temperature membrane (95°C–120ºC).
- The project supports the MYRDDP and advances progress toward DOE goals and objectives. Membranes that can operate under low-RH conditions and at high temperature can reduce system costs and improve system performance.

- The project has the ability to improve the state-of-the-art membrane performance; the project is well aligned with DOE program goals.
- Developing durable, high-performing membranes is highly relevant to the DOE program goals and objectives. The project did not receive a 4.0 because cost considerations are not included.
- The relevance of this work to the Program is questionable. Reasonably robust and durable membrane technologies are already currently available, and other DOE projects have further advanced the state of commercial or near-commercial technology. The technical maturity of the ionomers being developed in this project is so low that it is difficult to predict whether they will ever be commercially relevant at this stage, but the amount of work already performed in this project and previous projects, with little progress to show for it, suggests that the HPA-based approach to providing membrane conductivity is not likely to make it into real fuel cells in the foreseeable future.
- It is extremely hard to see how this project can be turned around in the last year with over half of all funding spent. The team does not yet have a viable membrane. Worse is that at this point there is still no evidence that HPA is a good and viable approach. Dr. Herring did an exceptional job years ago educating the community about the possibility of these materials, but we still do not know how to utilize these materials or whether their conductivity enhances the ionomer in which they are imbedded.

Question 5: Proposed future work

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

- Studies on material and membrane durability are needed. Future work with supported membranes is planned and is needed. The prior observations of membrane cracks suggest that even with supported membranes, mechanical durability could be an issue (e.g., in response to an RH cycling test). Such tests should be pursued.
- The proposed future research is aligned with the goals of the project; however, there is a significant amount of work to be done. The team should get some extra time because of the time lost this year due to the mishap in the laboratory.
- The future work is directed at characterizing the other relevant membrane properties. Work has begun on incorporating support materials to improve mechanical properties. Future conductivity measurements should extend to 120°C and to lower water content (water pressure [pH_2O] down to 40 kPa).
- The project is still near the very beginning of a polymer program, having just decided on the base materials. The team recognizes the shortcomings and has shown the two most important things, HPA retention and conductivity. There is a good deal of work just to get to mechanical viability—the project will look at supports and blends—but this is just an idea at this time. Demonstrating through-plane conductivity, chemical durability, and mechanical durability needs to be the focus.
- While CSM has done a good job of reporting on its synthetic method development thus far, future work in this area was not adequately described. The future work for the current year includes extensive testing at NREL, 3M, and Nissan, but it is still not clear that CSM will actually be able to provide a membrane for them to test.
- Scale-up and electrode optimization should not be done until there is some proof of durability using the DOE accelerated stress tests. The primary focus should be mechanical stability.
- More work is needed on demonstrating key improvements of the new polymer over the incumbent PFSA polymer—in particular on cell lifetime and cost.

Project strengths:

- The team is composed of respectable research organizations with adequate expertise. Overall, the team is equipped with the necessary knowledge base, resources, and industry/academia/national laboratory mix that is required for the success of this project.
- The HPA membranes made to date have had very high conductivity. HPA membranes should have benefits for chemical stability, as HPAs have been added as a chemical stabilizing agent for PFSA membranes.
- HPAs bound to polymers is a promising approach to meet membrane performance and durability targets.
- The strength is the unique properties of HPA protogenic groups.

- There has been good progress on developing new materials that are converted to membranes that can be characterized.
- Chasing the potential of HPA in ionomers is still an interesting endeavor.

Project weaknesses:

- More focus is needed to prepare a polymer HPA composite in which the HPA is permanently bound to the polymer groups. The team should make thinner membranes with the desired conductivity. The conductivity of the HPA/polymer membrane seems to vary with membrane thickness.
- CSM has been pursuing a similar approach for years now with little success. There is little reason to think the project will be successful at this point and even if a membrane for testing is produced within this project, it seems unlikely that the membrane will make it into commercial fuel cell products in the foreseeable future.
- There is a lack of focus and direction. Progress to date is very poor. The project is still searching for its polymer system. The project team needs to better understand the nature of the conductivity of these systems through a clear design of experiments in which investigators vary the HPA loading and break down conductivity to the ionomer and the HPA.
- There is no proof the membranes can be made mechanically or hydrolytically stable. There is very limited data—primarily just some fixed RH conductivity data. It was nice to see repeat measurements, but the data is highly variable.
- There is no fuel cell data and no indication of near-term or long-term cost benefits over PFSA.
- Membrane mechanical properties and potentially membrane durability are weaknesses.
- Project progress has been slow to date. The properties of this new class of membranes are not known.

Recommendations for additions/deletions to project scope:

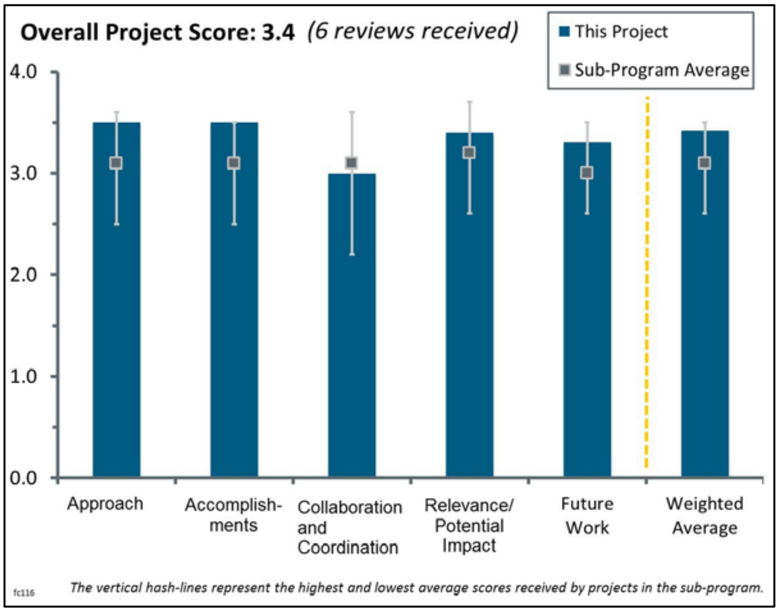
- ASR measurements should be conducted. The project should dig into the root cause of the weight loss after washing; it is unclear whether the weight loss was due to non-bonded HPA or whether bonds were broken. The project should confirm whether weight loss is due to HPA. The temperature and time dependence of the mass loss are unknown as is the impact of annealing on conductivity. The project should measure mechanical properties (e.g., through tensile tests).
- The focus should be on showing viability of these systems and doing a fundamental study so the HPA approach can be judged. A systematic loading study should be done with believable RH conductivity testing in a robust system. The project needs to be able to measure conductivity well, including through-plane. A systematic study of HPA loading with water uptake and conductivity as a function of temperature and RH needs to be done.
- Less work should be done on new polymer investigation. More work should be done on converting existing project polymers into membranes that can be demonstrated in cells. There should be more demonstration of costs and a go/no-go analysis. New materials offer no benefit to Program goals if manufacturing costs are a non-starter.
- The project should conduct more studies on membrane durability (e.g., in response to challenges from peroxide and to RH cycling).
- The electrode development portion of the future work should be deemphasized or removed. Given the low maturity of the membrane technology, any MEA testing performed should be done with standard electrodes or even platinum black electrodes so that the focus remains on the membrane.

Project #FC-116: Smart Matrix Development for Direct Carbonate Fuel Cell

Chao-yi Yuh; FuelCell Energy, Inc.

Brief Summary of Project:

The overall objective of this project is to develop an innovative, durable, molten carbonate fuel cell (MCFC) electrolyte matrix (Smart Matrix) to enable >420 kW rated stack power and 10-year (80,000-hour) stack service life. Compared to current-generation MCFC commercial technology, these correlate to a >20% increase in cell power density and ~100% increase in stack service life. The objectives for the current project year are to scale up manufacturing of the Smart Matrix and prepare for stack evaluation.



Question 1: Approach to performing the work

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

- The FuelCell Energy (FCE) team has done an excellent job in planning a promising route to achieve U.S. Department of Energy (DOE) durability targets for stationary fuel cell durability. The approach addresses all aspects of molten carbonate fuel cell (MCFC) matrix performance and durability and, given the central role of the matrix in this technology, the project appears likely to provide significant improvements in overall MCFC performance and durability.
- The project's approach is very clear and perfectly addresses barriers A and B. The project is very focused and well-structured. The timeline set out is very logical and well-thought-out.
- The approach is well-structured, -organized, and -conceived. It has milestones and go/no-go decision points.
- The approach based on accelerated testing to evaluate matrix stability is excellent.
- FCE methodically develops an understanding of degradation mechanisms and moves on to defining and verifying mitigation approaches. Accelerated testing is part of the approach to defining and mitigating degradation processes, as is out-of-cell analyses and testing. Acceleration parameters need more discussion in terms of justification and in their not creating different degradation mechanisms. Coarsening studies without electrolytes may underestimate degradation severity. More electrochemical screening could be informative.
- The approach is generally sound. Accelerated aging tests are necessary for improving durability of MCFCs. The accelerated aging studies strongly support the hypothesis that the CO_2 partial pressure has a major effect on the aging of the $LiALO_2$. It would be very useful to determine that the presence of oxide ions accelerates the decomposition of the lithium aluminate. This could be determined by electrochemical measurements of the oxide ion activity within the melt as a function of the CO_2 partial pressure. An yttria-stabilized zirconia (YSZ) probe may be used for these determinations. Overall, the research approach has contributed to improving the lifetime and durability of MCFCs.

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 FCE project has made excellent progress in the last year. The principle achievement this year was the demonstration of 80,000-hour durability (projected) through accelerated testing procedures. By reporting the accelerating conditions (i.e., higher temperature, humidity, and fuel utilization), and by showing the correlation between degradation metrics for single-cell accelerated testing and real-world stack operation, FCE has provided ample evidence for the validity of their accelerated testing and the accuracy of their durability projections. Other significant progress this year includes the demonstration of more than a 40% increase in matrix snap strength and improvement in Ohmic resistance relative to the baseline matrix.
- The accomplishments shown as a result of the accelerated testing are impressive. Understanding of the degradation mechanisms has been improved. Material stability of the new Smart Matrix has been confirmed. What remains is to show that the Smart Matrix can, indeed, be the main life-extending factor, and that can only be done by long-term stack testing.
- FCE has made substantial progress and is preparing a stack test to verify durability improvements; technical milestones have been met so far.
- The project is on track in achieving its milestones. The status is well-identified.
- The project team has achieved all the milestones as planned.
- The research team demonstrated substantial improvement in the durability of the electrolyte separator and met the 5,000-hour test milestone. The team predicts an 80,000-hour lifetime; however, the slope of the degradation curve is so flat that the errors in this estimate have to be very large. The average pore size was reduced, as previously presented, and the particle size distribution was significantly narrowed. The narrowing of the particle size distribution from the mean value is known to decrease the ripening of the ceramic particles.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- There are very few MCFC developers/experts worldwide. FCE collaborates with key experienced players such as the Illinois Institute of Technology (IIT) and the University of Connecticut (UConn). FCE works closely with raw materials suppliers.
- FCE collaborates well with subcontractors UConn and IIT to develop fundamental understandings of matrix coarsening and wettability and to help design mitigation approaches.
- Project strengths include the collaborations with UConn and IIT.
- Collaboration between partners is good, and there is good complementarity between them. However, there is no evident interaction or collaborations outside the sphere of the project in terms of knowledge exchange and dissemination activities.
- The collaboration with UConn is satisfactory; however, better engagement of the university resources would foster a better scientific understanding of the degradation mechanisms. Perhaps involvement with other universities with experience in high-temperature molten salt electrochemistry would be beneficial.
- Most of the work is being performed by FCE, but mechanistic studies of coarsening mechanisms at UConn and matrix wetting at IIT are contributing to the matrix design. Since much of the work is of a proprietary nature, this task distribution makes sense.

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 highly relevant to the Hydrogen and Fuel Cells Program's efforts on stationary fuel cells representing the most promising route to achieve stationary fuel cell performance and durability targets.

FCE is a leader in fuel cell commercialization and has produced and deployed more fuel cells (on a megawatt basis) than any other U.S. company, but further technical improvements in FCE's MCFC technology could broaden the market and accelerate fuel cell commercialization, so the impact of the proposed work could be significant.

- The relevance is good. MCFC technology is relatively mature; however, increased lifetime would substantially reduce cost of ownership. The potential impact is also good in that it facilitates the entry of fuel cell power sources for stationary generation.
- Success by FCE would enable meeting the durability/life target for stationary combined heat and power (CHP) and distributed generation (DG) fuel cell systems. Ownership costs (e.g., cost of electricity [COE] payback period) would be greatly reduced.
- The impact the project can have with the increase of lifetime is substantial. The fact that FCE is leading the project and is one of the industry leaders ensures that the impact will have an immediate effect on the fuel cell industry.
- The project aims at improving life and cost of MCFCs, thus fully supporting FCTO's research, development, and demonstration objectives.
- Doubling stack life to 10 years can result in a substantial COE reduction.

Question 5: Proposed future work

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

- Proposed future work is good. The new electrolyte separator technologies need to be scaled up and incorporated into full-sized power stacks.
- Future work logically follows progress to date. Parameters and processing conditions will be optimized and scaled up. A stack will be built from the new matrix and tested for 5,000 hours.
- Appropriate future work has been proposed.
- The future work is appropriate, but more detail on the future tasks would be helpful. Also, all future tasks appear to be FCE tasks, and it is not clear whether the university partners will still have a role going forward.
- Controlled release of Smart Matrix in direct fuel cell (DFC) products is planned to enhance DFC market penetration and clean energy job creation, enable a cost-effective distributed hydrogen-production DFC-H_2 system, and enable DFC-CO_2 capture for reducing CO_2 emissions.
- The proposed future is absolutely necessary for the validation of the results and to make sure that the barriers are addressed. The additional 30 kW technology stack endurance testing that will extend beyond this project is very valuable. It is very good to see that, outside this project, FCE is focusing on other life-limiting factors apart from the Smart Matrix. More details could have been provided on the plans to improve the manufacturing process and yield and on the cost-reduction approach.

Project strengths:

- The focus is very clear, excellently combining the expertise of the industrial and academic partners. If successful, the project will have an immediate impact on the fuel cell industry, as the results will be integrated in FCE products.
- The milestones for this project year were clearly met. The 5,000-hour test was completed with excellent results. The research should result in a substantial improvement in the durability of MCFCs.
- Approaches to evaluate the proposed Smart Matrix are excellent. Future work should focus on obtaining a better understanding of degradation mechanisms and proposed solutions.
- The project has met all milestones and succeeded in producing a new matrix that will enable improved performance and durability relative to the baseline.
- The project has a strong industrial participant.
- The project has a methodical approach.

Project weaknesses:

- While FCE has clearly achieved major improvements in the matrix design, the technical details of how these improvements have been achieved are mostly unknown. This may be unavoidable given the proprietary nature of the research, but FCE should give more detail about the materials and processes used, where possible.
- Some details on key technical work were not described, e.g., how to estimate life based on accelerated data. There were no details given on partner work, especially the work at UConn.
- Input from customers/end users would lend credibility to the project.
- The degradation model may be insufficient for the prediction of the cell lifetime. The x-ray photoelectron spectroscopy data show a decrease in sodium concentration within the matrix after testing. The impact of this loss was not discussed during the presentation. The project would benefit from high-temperature thermochemical and electrochemical data collection and analysis. This would provide a better understanding of the degradation mechanisms at play.
- In terms of publications and presentations, there are only four outside the sphere of DOE; more would have been expected, given the positive results.

Recommendations for additions/deletions to project scope:

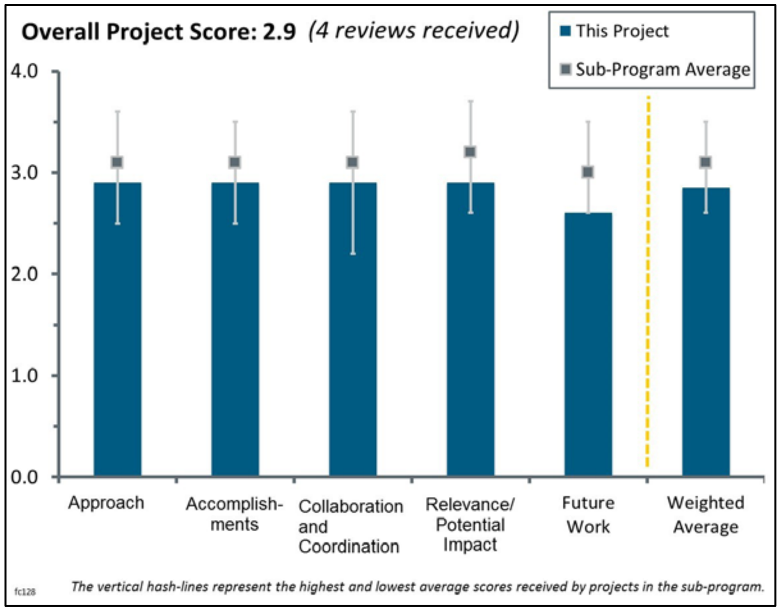
- More information should be provided in terms of the planned cost reductions. Manufacturing improvements could be described in more detail (e.g., present yield and how will it be improved).
- The project should add high-temperature electrochemical measurements of oxide ion activity as a function of CO_2 and water vapor pressure.

Project #FC-128: Facilitated Direct Liquid Fuel Cells with High-Temperature Membrane Electrode Assemblies

Emory DeCastro; Advent Technologies, Inc.

Brief Summary of Project:

Direct dimethyl ether (DME) is a carbon-neutral hydrogen carrier that can be used both for internal combustion and as cost-effective fuel for auxiliary fuel cell power systems in automotive transportation. This project will demonstrate direct DME oxidation with high-temperature membrane electrode assemblies (MEAs) and a Los Alamos National Laboratory (LANL) catalyst. DME is expected to significantly outperform state-of-the-art direct methanol fuel cells (DMFCs). The project will incorporate the new ternary anode catalyst in gas diffusion electrodes designed for high-temperature MEAs, evaluate performance with two different high-temperature membranes (PBI and TPS), and optimize structures and reaction conditions.



Question 1: Approach to performing the work

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

- The project started with very strong preliminary data, and the proposed approach to use high temperature membranes/MEAs is a natural one.
- The project approach addresses the project targets to increase maximum power of a direct liquid fuel cell and decrease Pt loading from that of a current DMFC by switching to a more energy-dense fuel and utilizing a high temperature polymer electrolyte membrane (PEM). The approach utilizing the higher temperature of a high temperature-PEM should prove beneficial for a DME-based fuel cell. It is not clear whether there is any steam reforming or hydrolysis of the DME prior to the fuel cell at the temperatures of operation. The project should perform some calculations to look at the stability of DME in steam at 160–200°C. It is not clear that the targets for the project would provide high enough power density for applications. Power density targets are fairly low for the applications mentioned on slide 3.
- Other than using DME as fuel, no new catalyst or membrane is to be developed by this project. It is essentially a system integration project. The principal investigator (PI) repeatedly emphasizes the importance of electrode structure, yet no scientific rationale or hypothesis was given on what structural improvement will be pursued.
- While the DME-based fuel cells are intriguing from a scientific standpoint, their performance (and loadings) to date seems so far away from commercial relevance that targeting small incremental advances in the technology, as has been proposed in this project, seems insufficient for an Office of Energy Efficiency and Renewable Energy (EERE)-funded project. From a science standpoint, developing improved membranes and catalysts for DME fuel cells can be done, and the proposed approach does this (although there is no real synergy in trying to do both; ideally, it would have been a catalyst-only project, as the current PBI membrane is not really the critical limiting element—rather, it is the poor cell performance and high catalytic overpotentials). The rationale for PtRuPd is reasonable based on the question-and-answer session (Pd is used to cleave ether linkage) but was not clear from the presentation. There is not currently a commercially viable high temperature DMFC, and just being as good as DMFCs is not compelling for this technology. Finally, DMFCs operate effectively by recycling the water in the system, and this is

accomplished by the natural phase separation of CO_2 gas from aqueous methanol solutions. It is unclear that such separation could ever be done effectively in the proposed DME system.

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 project has made good progress to date with substantial improvements over performance with Pt/C.
- The PI perhaps should have delineated clearly whether each/every single milestone in the project has been met. The data on slide 8 seem to suggest that the milestone is well met.
- The project is only six months old. Only limited experimental results are reported, mostly in test condition definition and benchmarking.
- From a scientific standpoint, the results are interesting, but too often a relevant baseline comparison is not included. It is not clear how the performance of DME with the Pd catalysts compares to PtRu (Pd-free catalysts) with either DME or methanol. More relevant comparisons of these families would improve the ability to judge performance improvements. Still, the performances reported are low (in terms of both current density and voltage). These represent modest improvement over previous performance but remain far from what is required for anything resembling commercial relevance. Some techno-economic analysis and market analysis could help define what is required for commercial viability. The project goals seem to be incremental improvements in today's performance without concern over what would be required to make commercially viable systems. The catalyst work is the most interesting. The membrane may offer incremental improvements, but it is not the critically limiting factor.

Question 3: Collaboration and coordination with other institutions

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

- Collaborations have been limited to LANL to date, as the project is fairly new. Reasonable agreement has been achieved between tests run at LANL and Advent Technologies, Inc. (Advent). Plans to collaborate with the University of South Carolina are in the future.
- The PI has identified possible relevant partners for incorporating non-precious metal catalysts.
- The team relies on the catalyst expertise from a national laboratory and membranes from the supplier/collaborator, making it difficult for the PI to control the project development pace.
- This really seems like it is just Advent with LANL playing a supporting role in catalyst development. It is not clear where the value proposition is for this with Advent.

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.

- Auxiliary power for transportation and for backup power is an important application for high temperature fuel cells.
- The project is relevant to portable power, backup power, and distributed generation fuel cell markets. The technology is different enough from low-temperature PEMs that market development with high temperature -PEM direct DME fuel cells will not be beneficial to the PEM supply chain.
- The project is relevant to DOE Fuel Cell Technologies Office goals.
- Performance is so far away from any targets in the DOE Multi-Year Research, Development, and Demonstration Plan that it is hard to imagine this project having an impact on any of them.

Question 5: Proposed future work

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

- Future work focused on reducing platinum-group-metal (PGM) loading and optimizing the electrode layer is appropriate and will lower costs and improve power density.
- Research targets are clearly identified. However, the paths are generic and lack specifics.
- More details are needed.
- The proposed future work is primarily associated with targets that are not compelling. They focus on incremental improvements in performance from poor baseline performance or in scaling up and reducing loading to a very high loading level: 4.5 mg PGM/cm^2. The remaining challenges and barriers are also not compelling. For optimizing DME, especially when performance is so poor at high loadings, efforts to improve water ratio seem like a poor area of focus.

Project strengths:

- The novel approach to utilizing a direct liquid fuel cell for the backup/distributed power market is a project strength.
- The team has a good collaboration between industry, a laboratory, and academia.
- The strong performance of the DME MEA is a project strength.
- Catalyst work is interesting from a fundamental science standpoint.

Project weaknesses:

- More details are needed for the annual report.
- There is a significant lack of innovation in this project.
- Performance of these systems and possibilities of commercial relevance are project weaknesses.
- The project does not align with the transportation focus of the EERE Fuel Cell program, and learnings from this will not really cross over to or have an impact on PEM fuel cells for transportation.

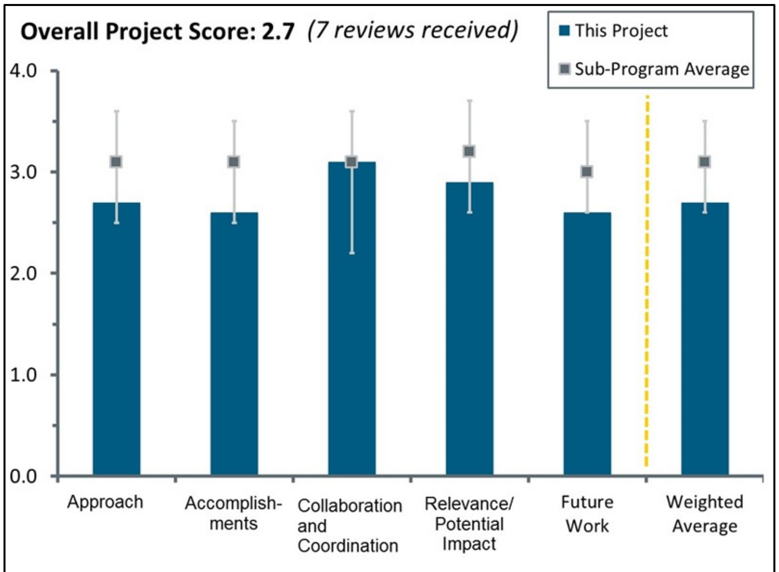
Recommendations for additions/deletions to project scope:

- Techno-economic analysis should be added to make a case for what performance and costs would enable the technology to be competitive in specific markets. Without this, the project targets are relatively meaningless.
- The PI should provide a clear description if a milestone has been met and what exactly will be done next year.

Project #FC-129: Advanced Catalysts and Membrane Electrode Assemblies for Reversible Alkaline Membrane Fuel Cells

Hui Xuastro; Giner, Inc.

Brief Summary of Project:



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

The project aims to combine an anion-exchange membrane (AEM) water electrolyzer with a fuel cell in a single stack to develop a reversible AEM fuel cell for energy storage and conversion. A water electrolyzer is an ideal device for storing energy as hydrogen from wind turbines and solar farms. The stored hydrogen can later be used in fuel cells to generate low-cost electricity during peak times. Use of catalysts based on non-platinum-group metals (non-PGMs) drives down capital costs. The project also contributes to maturing AEM technology and developing new concepts for oxide catalyst design. Tasks include (1) designing and developing oxygen reduction reaction and oxygen evolution reaction (ORR/OER) bi-functional oxide PGM-free catalysts and (2) integrating ORR/OER bi-functional oxide catalysts and alkaline membranes to develop highly efficient, reversible alkaline membrane fuel cells for stationary energy storage.

Question 1: Approach to performing the work

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

- The selection of the catalyst materials is sensible. Carbon nanotubes (CNTs) represent a good direction for more robust catalysts. The important issue is whether these catalysts can ultimately overcome the oxidation deactivation issue. There is no clear role for the National Renewable Energy Laboratory (NREL) in this project. NREL is leveraging their contribution from another U.S. Department of Energy (DOE)-funded project, although they have not demonstrated a clear path to reduce Pt usage.
- The use of a unitized electrolyzer/fuel cell in the alkaline domain has been the goal of electrochemists for decades. Use of perovskites as bifunctional oxygen electrodes is also known, albeit never showing stellar performance in a practical system. The approach, therefore, though sound, is not tremendously innovative. In addition, the project's approach using rotating disk electrodes (RDEs) as the sole indicator of effectiveness in an operating cell is very ambiguous. However, as per project metrics, this effort is on target.
- Despite the fact that several classes of metal oxides, spinels, and perovskites have reasonably high activity in both ORR and OER, the approach selected in this project based on usage of carbon supports cannot be scientifically justified. All types of carbons with no dependence on the level of graphitization will be oxidized to CO_2 at potentials higher than 1.23 V versus RHE. Slide 7 shows the stability of different types of carbons during the cycling between 0 V and 1.9 V. Taking scan rate into account, the whole experiment duration should be around 30 minutes, and dramatic degradation of material is seen. Electrodes fabricated with PGM-free oxides will operate in AEM electrolyzers at realistic potentials of 2–2.1 V (in deionized water), and the expected life-time should be 50,000+ hours. Thermodynamically, there is no carbon material that can withstand these conditions.
- The project addresses multiple reversible fuel cell barriers associated with the catalysts for reversible alkaline membrane fuel cells. However, the challenges of achieving sufficient activity and durability with a

single bifunctional catalyst are high enough that, despite significant progress, the project has a slim chance of leading to a commercially viable technology.
- The proposed approach has limited potential in addressing aggressive barriers in reversible fuel cells. Besides the proposed methods, investigators should be focused on examination of catalysts' structures after electrochemical cycling.
- While the original plan assumed a comprehensive approach, targeting development of cathode and anode catalysts, as well as the membrane electrode assembly (MEA), so far, this project has been dominated by the development of non-precious metal ORR/OER catalysts. Much less attention has been devoted to the hydrogen oxidation and evolution reaction (HOR/HER) catalysts and virtually none to MEA integration. This evident imbalance needs to be corrected to enhance the odds for success of this project. The use of carbon supports for OER catalysts is highly questionable.
- There are serious weaknesses in evaluating results, both internally and externally. The two goals for the project are to develop and then test bifunctional catalysts. For this period, only the development of materials took place, and this development seems to ignore most background work that has been done to date.

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 met the first go/no-go milestone. Compared to other incubator projects, this one is more on track and on performance. The current perovskite-/spinel-based catalysts' performance remains significantly inferior to Pt/C for ORR in an alkaline medium. This should be addressed as the project continues.
- The overall project metrics and goals, including the go/no-go decision points, have been met using the RDE technique. All other tasks are well under way.
- The activity of two types of catalysts—Co_3O_4/CNTs and graphene tubes in OER and ORR—is very confusing. On slide 10, ORR performance at 0.9 V was 1.24 mA/mg and the limiting current was 90 mA/mg, while on slide 16, the second catalyst had a limiting current of just 2.5 mA/mg. The same confusing results were obtained for OER catalysts. The electrochemical performance was reported partially in mA/mg and partially in mA/cm^2 with no information on catalyst loading, which makes correct interpretation intrinsic activity and stability extremely complicated.
- Giner, Inc., and Giner's coworkers have made significant progress toward the project objectives. The main achievement this year was demonstration of 1 mA/mg ORR performance at 0.9 V and 15 mA/mg OER performance at 1.6 V in RDE testing, meeting go/no-go criteria. This milestone was satisfied with two different types of catalysts synthesized within the project. However, both ORR and OER activity are still rather low compared to the activity of dedicated monofunctional catalysts, and the round-trip efficiency of a reversible fuel cell based on the catalysts developed in this project would be rather low. Some progress has been made on development of hydrogen catalysts as well, but the catalysts developed to date are PGM-based. The possibility of eliminating PGMs is the main reason for interest in alkaline membrane fuel cell and electrolyzer technology, so the inclusion of PGMs on the hydrogen electrode is undesirable. Catalyst results to date come from RDE testing; MEA performance is not reported yet. DOE goals for this technology are not well defined, so it is not clear that this project is contributing to meeting the overall objectives of the Fuel Cell Technologies Office (FCTO).
- The oxygen catalyst development has followed two approaches. The results are interesting, and progress is evident. Performance targets, inexplicably defined in terms of mass activity (not justified for non-PGM catalysts), have been met, though they were not very challenging. In reality, the ORR activities are generally low—below the state of the art—and also indicate possibly high peroxide generation (which was not determined). There is no convincing evidence in the presented results that OER currents are carbon-corrosion-free (a realistic possibility, based on some presented results). Relative to the effort invested in the development of the oxygen catalyst, the team has paid little attention to the development of the hydrogen catalyst. No polarization plots for PtNi nanowires have been shown, which makes true activity evaluation impossible. There is some evidence of a relatively poor stability of the nanowires, not surprising for a PtNi alloy. No catalyst developed in this project has been MEA-tested, which is disappointing.

- While some progress has been reported, the status of perovskite-based materials remains unclear. There is an obvious discrepancy between what was proposed in the technical milestone table, what is in slide 23, and what has been reported.
- Reported progress is not supported by the data presented. All the perovskite goals—phase purity, crystallite size, and performance—are not demonstrated in data, and the data that are presented contradict the stated success. Further, no identification or quantification of the oxygen vacancies is presented, and while the selected materials set (Co-based nanoparticles on graphene oxide tubes) does appear to demonstrate performance, there are issues with this approach as well. It is unclear how these are graphene nanotubes. There are no characterization data for these. Further, there is no explanation or description of the path toward improved performance in this poorly defined and described materials set.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration with State University of New York–Buffalo (SUNY-Buffalo) on development of CNT-supported oxides appears to have contributed significantly to the project. The NREL collaboration is critical since NREL is supplying the hydrogen catalysts and the membranes.
- This project features excellent partnerships between various entities, including a university (SUNY-Buffalo), a national laboratory (NREL), and a commercial partner (Giner, Inc.), with clear goals and objectives.
- The team has excellent participants that are well known in PGM-free materials design, electrodes fabrication, and characterization.
- The team includes a university and a national laboratory and is well balanced.
- The three partner organizations in the project appear to collaborate with one another. NREL's contribution has been less than that of the two other partners, but the laboratory's role may increase in the future when the focus shifts more toward the MEA and hydrogen catalyst development. No external collaborations were listed. An addition of potential future customers in the second year of this project could be helpful.
- Interactions between participants can be improved by engaging methods to investigate catalyst structural properties during and after electrochemical cycling. The PI should also consider stability screening of carbon-based catalyst supports through additional in situ techniques.

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 focus of this research is on the development of better materials for "reversible" alkaline fuel cells/ electrolyzers, including catalysts for both electrodes and MEAs capable of competing with the state of the art in the field, e.g., systems based on Tokuyama membranes. While viability of the alkaline system of this type is not certain and the effort lies on the peripheries of FCTO, the project is interesting and overall relevant to the FCTO objectives.
- The project is relevant to the FCTO and DOE research, development, and demonstration objectives.
- The project is relevant to FCTO's goals.
- While reversible fuel cells are not likely to be vehicle-deployable, they promise additional future hydrogen generation and storage options.
- A unitized fuel cell electrolyzer is a great objective toward which to strive. Being in the alkaline domain, the principle advantage is the freedom from noble metal catalysts. However, challenges include severe overpotential losses for both HOR/HER. No clear strategy is mentioned for overcoming these losses.
- The project's main achievements at the moment of presentation are made with oxides supported on carbon or carbon-based electrocatalysts, which intrinsically cannot be stable at electrolyzer conditions.
- The project is not very relevant to FCTO goals. The substantial technical challenges faced by unitized reversible fuel cells make them unlikely to be commercialized in the foreseeable future. Furthermore,

technology for reversible alkaline membrane fuel cells is sufficiently different from conventional fuel cell technology that improvements made by this project will not be relevant to other fuel cell projects.

Question 5: Proposed future work

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

- The research plan is rational. The technology barrier remains significantly high.
- Future work follows in line with the milestones and metrics for this effort.
- If the proposed future work is based on Co_3O_4/CNTs and graphene catalysts, it may result in achievement of initial performance in unitized regenerative fuel cells; however, the cell will degrade substantially in the first hundred hours of operation.
- The future work is largely dedicated to MEA integration of the novel catalysts and testing in MEAs, which is appropriate at this stage, but further technical detail on the integration and testing plans is needed.
- In addition to MEA fabrication, future work should be more focused on the link between measured electrochemical properties and structural features of the catalyst. That would be helpful in overcoming barriers and would guide the synthesis of robust catalysts.
- There was very little substance in the future plans slide in this presentation. MEA fabrication and optimization of test conditions have little to do with the development of materials, on which the team should continue to be focusing. The same is true of modifications to “fuel cell configuration” and the test station for intermittent operation.
- Future work was unclear, nor was it clarified when the presenter was asked.

Project strengths:

- Strengths of the project include its focus to achieve performance that would meet targets and overcome barriers, implementation of both electrochemical methods RDE and MEA, and reliance on non-precious-metal-based catalysts for ORR/OER.
- The project strength can be in design of unsupported oxides and perovskites with high electrical conductivity.
- The project is doing a good job of leveraging previous work and outside work to produce novel catalyst structures and is on track in terms of meeting milestones.
- The oxygen catalyst development is by far the strongest part of the effort.
- The project is well on track.
- The concept is interesting. Giner has demonstrated systems experience.
- The project aims at enabling a unitary fuel cell operating with an AEM. Partners in this effort are well placed to effectively meet the goals and objectives of this effort. ORR/OER catalysts have met the project go/no-go decision point. However, this has been obtained using the RDE technique, which is significantly distant from obtaining the same result in an MEA half or single cell.

Project weaknesses:

- Hydrogen catalyst development has trailed the oxygen catalyst effort with no polarization data presented. There seems to be lack of understanding of the causes of performance differences between materials; for example, various carbon supports and carbon tubes have been derived from different transition metals. CNTs have been selected as a stable nanocarbon support for the OER in spite of exhibiting what appears to be persistent corrosion at potentials higher than 1.7 V.
- Weaknesses include apparent discrepancies between the technical milestones and systems in this report; need for careful investigation of stability for carbon-based supports; lack of detailed insight into catalyst structure before, during, and after electrochemical cycling; and lack of in situ methods for structural characterizations of catalysts. Quantitative analysis of the metal oxide catalysts before and after electrochemical cycling is needed, as well as strong proof that observed currents are not associated with dissolution of employed materials. Improved coordination between participants is also needed. Existence of iron in the most active catalyst could induce damage of the membrane. Evaluation for perovskite- and spinel-based materials is needed.

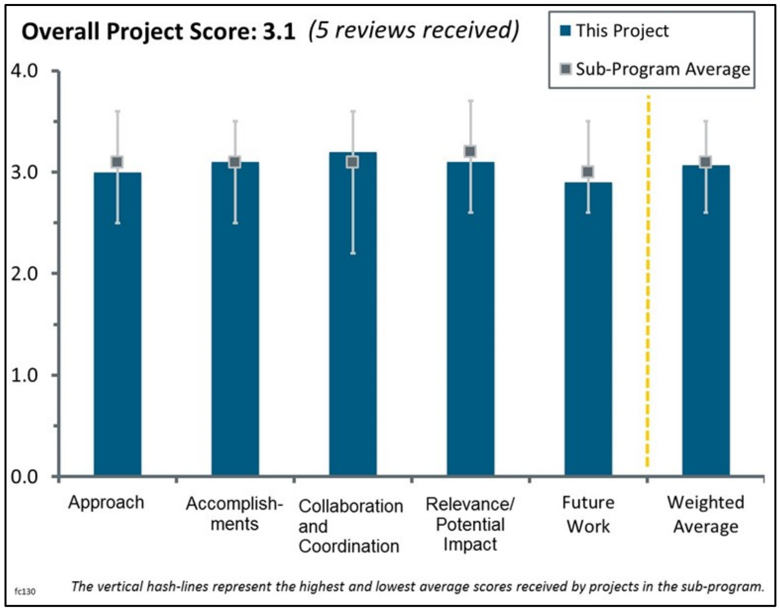
- The project is not very relevant to FCTO. Even if the project meets its goals, it is unlikely to substantially increase the odds of unitized reversible fuel cell technology becoming commercially viable.
- The project does not examine any innovative class of materials for ORR/OER; perovskites are well known. There is very little attempt at performing detailed structure property relationship studies in concert with catalyst activity. Important details of the kinds of metal oxides and their conductivity should be included. Taking into consideration that the AEM ionomer has a significant effect on catalyst activity, the ionomer's role should have been a part of the RDE studies. No details were provided about the nature of graphene oxide tubes, including surface areas and corrosion analysis.
- A project weakness is the completely wrong selection of materials (carbon-based supports or catalysts) for ORR and OER.
- The materials developed are poorly developed, and a path to improvement is not presented.

Recommendations for additions/deletions to project scope:

- More details on the ORR/OER metal oxide catalysts, such as electronic conductivity and/or structure, should be provided. It is not clear what the active site is for ORR and OER or whether there is a correlation with defects. It is not known whether the cycling tests were conducted under inert gas flow or under a blanket of oxygen—or, in the case of the former, how it would be reconciled with actual cell operation. Economic analysis should include the comparison of operating a system with AEM membranes but with the electrolyzer and fuel cell separate.
- This project would benefit from a better understanding of the reasons behind observed differences in catalyst performance and from early discarding of materials that do not perform/show promise. For example, the durability testing of the Co_3O_4-oCNT catalyst should not have been carried out, given the low ORR activity of that catalyst. The levels of peroxide generation rates need to be evaluated and used as one of the down-selection criteria for ORR/OER catalysts. Fuel cell testing is necessary. Non-PGM catalyst performance targets should be given in terms of surface-specific activity, not mass activity.
- Better proof of the materials development claims and performance is needed to demonstrate the viability of the approach.
- The project would benefit from additional screening of the catalyst's structure using in situ methods (carbon-based supports and metals) and compositional analysis of the catalyst and quantitative comparison of the metal content before and after electrochemical cycling.
- The reviewer recommends a no-go decision regarding carbon-supported materials, and additional concentration on passing a go/no-go design point with unsupported conductive oxides.

Project #FC-130: Development of Platinum-Group-Metal-Free Catalysts for Hydrogen Oxidation Reaction in Alkaline Media

Alexey Serov; University of New Mexico



Brief Summary of Project:

This project will enable integration of platinum-group-metal (PGM)-free anode materials into an optimized membrane electrode assembly (MEA) structure. The resulting PGM-free-based anion-exchange membrane fuel cell (AEMFC) is expected to demonstrate significantly improved peak power density (up to 250 mW/cm^2). Objectives include developing PGM-free electrocatalysts for hydrogen oxidation reactions in alkaline media, scaling up the catalysts to 50 g batches, synthesizing a new type of ionomer for the AEMFC, and fully integrating the PGM-free catalyst with the ionomer into the MEA.

Question 1: Approach to performing the work

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

- The approach generally appears sound, with a multi-faceted approach that addresses the major aspects of developing a PGM-free hydrogen-oxidation-reaction (HOR) catalyst for alkaline media. Recommendations include the following: provide more information on the cost and target analysis; benchmark data against other systems; discuss stability issues and whether any evaluation will be done; justify the scale-up activities; establish MEA evaluation criteria; increase ionomer integration work; and develop plans to establish the structure-to-properties relationships.
- The approach taken by the project team is generally effective; however, it could be improved. For example, catalyst performance and material preparation processes could be evaluated/optimized with parametric studies/experiments.
- The approach is good, with two exceptions that are related to potential durability issues. First, the MEA testing does not appear to include testing on air. It was unclear how the team will determine whether there are issues with CO_2. This is an anode catalyst, but CO_2 will cross over from the cathode and may cause other issues that may have an impact on the performance (e.g., carbonates in the catalyst layers). Second, the approach does not appear to assess stability over the full electrochemical potential range to which an anode catalyst will be exposed.
- Using a nickel-based catalyst for HOR in alkaline medium is a relatively well-known approach. The team uses other metals as additives with some encouraging development. The progress appears rather slow, given that the synthesis and characterizing methods used are relatively straightforward. The result is not particularly encouraging.
- The authors are attempting to fabricate, characterize, and scale up a new catalyst for the HOR in alkaline solution, and much of the work being done in this project is new. The approach is to characterize electrocatalysts with a rotating disk electrode (RDE). The catalyst loading is very high, and very thick electrodes are being used versus the standard thin-film RDE methodology. As a result, it is very hard to evaluate the data. It would have been helpful if the principal investigator (PI) had provided references to the proven methods employed because they may not be as well-known as the PI assumes. On slide 7, it appears that Los Alamos National Laboratory (LANL) is doing the baseline voltammetry for ionomer evaluation in 0.1 M $HClO_4$. The purpose of these studies is not clear because it is well known that RDE is

inadequate for HOR measurements of Pt/C in acid because of mass transport limitations (see Julien Durst, Christoph Simon, Frédéric Hasché, and Hubert A. Gasteiger, "Hydrogen Oxidation and Evolution Reaction Kinetics on Carbon Supported Pt, Ir, Rh, and Pd Electrocatalysts in Acidic Media," *Journal of The Electrochemical Society* 162 no. 1 (2015):F190-F203, doi:10.1149/2.0981501jes, and references therein). Also, no details are provided on the catalyst coated membrane (CCM) manufacture and assembly. A simple error in over-compression of a gas diffusion layer can cause the results seen between the two different ionomers. More information on the CCM manufacture (provided in the back-up slides) would have been helpful.

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 Ni-Mo-Cu sample has a current density of 0.055 mA/cm^2 at a voltage of 0.01 V, which is approaching the target of 0.085 mA/cm^2, which is the first go/no-go design point. It is not clear what the timeline to achieve this target should be. Regarding ionomer development, work has been done to select the ionomer, with density functional theory (DFT) modeling to support. The experimental data and DFT modeling are in agreement, and the team was able to rank the ionomers and downselect. Although the work on the ionomer testing on slide 16 shows various systems tested, no conclusions are presented, and it is not clear what the team has learned from the ionomer testing. The scale-up process work has been initiated, and the MEA design and testing has been initiated, but no results have been shown for the target catalyst systems. Baseline Pt/C MEA testing data are shown.
- The project team has made significant progress toward achieving the go/no-go decision point.
- The team appears to be on track midway through the project. The MEA performance is disappointing, but other results are good so far.
- According to its own metrics, the project has made significant progress. However, good benchmarks for HOR catalyst performance in alkaline solution by RDE or in CCMs do not exist. It is also not clear that RDE voltammetry has been validated as a useful tool for predicting electrocatalyst performance in a functional cell. It is difficult to evaluate whether accurate kinetics can be measured in such a way. It would have been helpful if the PI had provided the reasoning behind the "benchmark" of 0.085 mA/cm^2 at 0.01 V. It is not clear if this target is iR-corrected, nor is it clear what is expected for Pt. Overall, not enough information was provided to evaluate the project's accomplishments. In the project's defense, this is probably a ten-year project, so the one-year accomplishments are reasonable, especially with the focus on transition imposed by the DOE Office of Energy Efficiency and Renewable Energy.
- The project is clearly behind schedule.

Question 3: Collaboration and coordination with other institutions

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

- Collaboration and coordination with other institutions is excellent; the team seems to be working well together, and it is a nice mix of industry, national laboratory, and universities.
- Good collaboration exists between university, national laboratory, and industry.
- A good, effective team has been assembled, each with clear expertise and contributions to the project. However, it is difficult to tell how much interaction the team has on a regular basis, and given the nature of the work, it is recommended that regular meetings are held to improve the effectiveness of the integration activities.
- The collaborations are good, although in hindsight, the project partners should have pursued collaborations that would yield more fundamental information about their electrocatalysts. Given the applied nature of the project and the two-year timeline, the collaborations are acceptable.
- Although the project involves multiple partners, the contribution from other members seems very minor at this stage. For example, there seems to be a great deal of needed MEA development, which is supposed to be done by IRD Fuel Cells (IRD). It is unclear whether IRD has adequate knowledge and expertise in handling the metallic-material-based ink.

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.

- Slow HOR kinetics in alkaline solution is a known problem, and seeking a non-precious-metal replacement to Pt/C would be beneficial. The development of an inexpensive but effective electrocatalyst for the HOR in alkaline solution could be very important.
- The alkaline fuel cell anode is an important area for cost reduction.
- The project aims to develop PGM-free catalysts for HOR, thus fully supporting DOE research, development, and demonstration objectives.
- The work is relevant to developing the AEMFC, which is an important and potentially disruptive technology that may achieve Fuel Cell Technologies Office objectives at a lower cost. However, the project team has not provided analysis to clearly show the potential positive impacts.
- This project is good to include as part of an alternative to conventional proton exchange fuel cells (PEFCs), but it does not address all of the major issues with alkaline fuel cells. Since it is well accepted that PGM-free catalysts can be used in alkaline fuel cells (AFCs), it may be more useful for DOE to focus on the more serious AFC barriers, such as membrane stability and carbonates, which may not precipitate in AEMFCs but still have a major impact on ionic conductivity of membrane and catalyst layers.

Question 5: Proposed future work

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

- The activities outlined in the path forward appear suitable: decrease particle size of nickel-molybdenum-metal (Ni-Mo-M) catalysts; optimize RDE working electrode preparation to establish kinetics parameters; integrate a PGM-free anode into an MEA structure; design an MEA fabrication and testing protocol. Further work is required in the areas of catalyst system development and ionomer integration work to understand optimization opportunities. Increased information should be provided on what work will be conducted on establishing the structure-to-properties relationships. This will be important work.
- The proposed future work is good, but the team should assess stability in the presence of air.
- The project partners should focus less on DFT and powder scale-up and more on electrochemical methods (RDE and CCMs). Although the project partners probably do not have time, in situ spectroscopy measurements would have seemed more important than DFT work.
- Although future challenges are identified, no clear solutions or approaches are identified.
- It is not clear what impact the proposed near-term activities will have on overcoming the barriers.

Project strengths:

- The concept of the project is very good, and it is positive that there are commercial partners and interactions with numerous collaborators.
- There is a strong team addressing project objectives with a multi-faceted, integrated approach.
- Project strengths include the focus on key activities to support the project objectives and sound technical approaches, the ability to make 50 g batches of catalysts, full MEA testing, and no C supports.
- The team achieved some promising initial results.

Project weaknesses:

- This is a very complicated project. It is not clear that the project has an active electrocatalyst because of limitations and challenges with the test methods. The authors are using “boilerplate” methods for advanced catalysts—scale-up, DFT, etc. More thought is needed on the fundamental electrocatalysis and methodology.
- The RDE testing is a primary evaluation tool, and it is not clear whether the kinetic data being reported will translate to MEA data, or how they compare against any benchmark data.

- Potential stability issues are a weakness with the project. The team should assess stability in the presence of air (1.23 V RHE) since anodes in a real-world fuel cell system will be periodically exposed to air (it is practically impossible to keep hydrogen on at all times). The lack of MEA testing in air is a weakness.
- The lack of identification of key impact/optimization factors, for example, for catalyst preparation processes, is a weakness.
- The progress is significantly behind schedule, particularly given the high level of funding for the project.

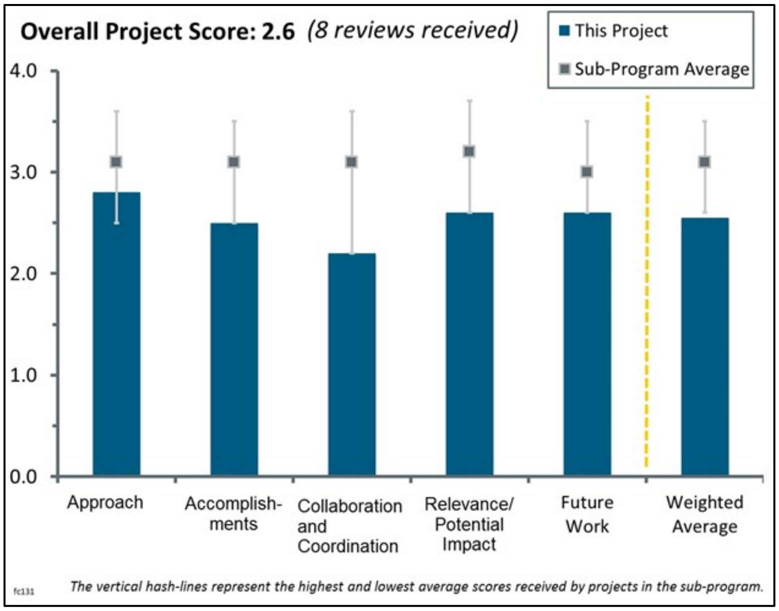
Recommendations for additions/deletions to project scope:

- More information regarding relevance of the work and the potential impact should be included. The University of New Mexico (UNM) should identify whether cost and scenario analysis has been done to help set targets, and if not, what information would be required to get to the point when the analysis can be done. Stability of PGM-free HOR catalyst is a primary concern, and the manner in which this will be addressed should be identified. Benchmarking data of catalysts for relevant systems should be provided. It is not clear whether the scale-up work is required to support the other project objectives.
- LANL work should focus on systems more directly related to the UNM work. Perhaps the project could deemphasize scale-up and emphasize more work on method development.
- Specifications for materials preparation processes should be developed. Electrode/material performance stability studies should be performed.
- The project should add cyclic voltammetry testing that includes cycles up to 1.23 V RHE to assess whether there are potential stability issues. Hydrogen/air testing with MEAs should be added.

Project #FC-131: Highly Stable Anion-Exchange Membranes for High-Voltage Redox-Flow Batteries

Yushan Yan; University of Delaware

Brief Summary of Project:



This project aims to develop a class of anion-exchange membranes (AEMs) with very high oxidation resistance for high-voltage cerium redox-flow batteries (RFBs) and other alkaline-membrane-based electrochemical devices, such as fuel cells and electrolyzers. Cerium RFBs show potential to offer high-performance and low-cost electricity storage solutions for renewable energy, and stable AEMs are the key missing element in making cerium RFBs a viable technology. Stable AEMs can also be used for hydroxide exchange membrane fuel cells, for improving cell durability and performance, and for highly durable AEM electrolyzers, lowering hydrogen production costs. This project will contribute to knowledge of polymer chemistry and membrane technology that will help advance the design and development of polymer electrolytes for electrochemical devices.

Question 1: Approach to performing the work

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

- This project seeks to extend the principal investigator's (PI's) prior work on alkali-stable aryl phosphonium ionomer AEMs for use in double-membrane RFBs having Ce(4+) as an active component. The approach is to identify new phosphonium organocations that are stable to extended exposure to Ce(4+), and incorporate them into ionomers from which oxidatively stable AEMs may be fabricated and tested. This is a generally good approach.
- The project has a nice systematic approach to developing stable phosphonium cation-based AEMs. It is unclear why phosphonium is preferred over ammonium cations. The cost projections based on the high-voltage redox chemistry show the value in pursuing this approach. The project highlighted a specific redox chemistry using a bipolar membrane or a pair of polymer electrolyte membranes (PEMs)/AEMs, but the bulk of the presentation outlined the synthesis of the AEM ionomer. It is unclear whether the project is to develop the RFB chemistry and membrane, or just the membrane.
- The project seeks to develop a stable alkaline membrane for use in RFBs, enabling the development of a double-membrane flow battery as demonstrated in a separate Advanced Research Projects Agency–Energy (ARPA-E) project. The approach of combining a stable cation with a stable backbone to achieve high membrane durability for RFBs is reasonable.
- The synthetic approach appears solid. It would be beneficial to see at least some focus on conductivity/resistance measurements in addition to stability. This project would greatly benefit from some analysis as to what is required for specific applications. It is not clear why milestones are limited to 40°C. It is not clear how the ex situ stability tests correlate with in situ degradation. Methods for backbone and functional group down-selection are unclear.
- The approach to make stable AEMs using non-conventional cationic groups is good. The stability study for the candidate cationic groups before putting in the polymer structure is reasonable. However, the rationale for using phosphonium versus ammonium is not fully justified. The cation exchange membrane and AEM

approach for redox flow cells to increase the potential window is innovative. The approach to develop such systems should include other requirements such as compatibility with redox active species, conductivity, and crossover. The synthetic approach to make AEMs is satisfactory but not innovative.

- The project appears to be systematically addressing oxidative stability issues. While durability is promising, conductivity of the materials was not reported, suggesting it is not being characterized. Both durability and conductivity must be co-optimized.
- One element that is lacking is a consideration of cation crossover, including Ce(4+) and, as the PI mentioned in his comments, H+. This seems like something that should be given attention for an RFB because very high concentrations of redox agents are going to be desired.
- The stability test methodologies (looking for weight change and color change) are not the most accurate methods to determine membrane degradation. An analysis of the leach solution for residual organic components would be more sensitive and provide some information about the degradation site. The 9MeTTP+ cation should be quite stable if it can be attached to a polymer backbone; however, the steric factors, which help the stability, will make it difficult to link this cation to the ionomer backbone.
- The Program's technical targets are not mentioned in the presentation. It is unclear how this work is advancing toward meeting any membrane performance goals. It is not clear whether this is a battery project or a fuel cell project.

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.

- There is nice work improving the stability of the linking group between the cationic group and backbone.
- The PI has identified a phosphonium cation and screened several polymer backbones that are stable to extended exposure to Ce(4+). T
- Timely progress was made against project milestones. Durability and synthesis is improving. Conductivity of materials is unclear.
- The project is working toward meeting durability targets on ionomers, but it is having difficulty in the synthesis in the transition from small-molecule salts to ionomers. The PI's initial coupling strategy to make ionomers did not give high enough coupling yields, so he switched to another coupling strategy, which worked better but was not durable on exposure to Ce(4+). He has identified a third approach that he expects will succeed, but he has not yet tested it. Conductivity measurements have not yet been pursued because adequate ionomers have not been made. He is doing the right things to get the synthetic chemistry to work; he just has to work through the difficulties that inevitably come up in chemical synthesis.
- Some progress was made on making stable polymers; however, it is not clear how the weight loss measurements related to fuel cell stability. For example, in perfluorosulfonic acid materials, a weight loss measurement is not typically used to forecast membrane lifetime; only in-cell accelerated tests accurately replicate operational stability. The technical readiness of this project is far from this point.
- The project is narrowly focused on ionomer development for alkaline membranes, and it appears that good progress is being made in developing the proposed synthetic pathway, but a clear justification of how these results represent progress toward the overall project goals and DOE goals is lacking.
- Stable polymer backbones have been identified. 9MeTTP+ has been connected to a commercial polymer backbone with high yield. There is minimal stability data on functionalized polymers. Quality membrane films have not been prepared. There are no conductivity or performance data.
- The project has run into difficulties preparing a 9MeTTP+ derivative of a polysulfone backbone, has not been able to produce a membrane to begin tests, and is behind schedule. An alternate approach utilizing one of the methyl groups of the 9MeTTP+ to link to the backbone through an amine linking group has been proposed. There appear to be problems with this approach as well, as bromination at multiple methyl groups is possible. Bi- or tribrominated cations would lead to crosslinking of the membranes and poor membrane properties.
- Justification of the claims that the project has met the milestones is insufficient. Significant technical detail on the synthetic pathways being pursued was reported, but no data were presented to address specific milestones.

- Polymer backbone stability of unfunctionalized PSF, PEEK, and 6F-PBI for 1,000 hours at 40°C (slide 8) should have been done in more rigorous conditions for the sake of time. Also, measuring only weight after the stability test may not be good enough for ensuring stability. Mechanical properties and/or gas permeation chromatography (GPC) measurements should have been performed before synthesizing AEMs. There is relatively good progress on synthesizing polymer membranes. However, more membrane characterizations such as conductivity, titration, and stability measurements need to be done or have been planned, so overall progress on this project looks to be slow considering that this is two-year project. Much of the polymer degradation can happen with the combination of polymer backbone and cationic group. As the stability test for the target polymer is incomplete, it is doubtful that the PI can complete the membrane development and testing within the remaining project time.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.2** for its collaboration and coordination.

- It appears that no work has been done outside the University of Delaware yet. The National Renewable Energy Laboratory's (NREL's) role is to test membrane durability and make and test MEAs. Because no membranes have been prepared yet, there has been no work for NREL to do.
- It appears that there has been no collaboration/coordination because synthesis difficulties are causing the PI not to have materials to give to NREL for testing.
- Collaboration with NREL is stated; however, the project is far from the ability to make testable membranes.
- There is no evidence of collaboration with NREL, the only partner on this project.
- It is unclear whether the single project partner, NREL, is engaged.
- NREL's role is unclear.
- So far, the project does not seem to have meaningful collaboration. The reported results all appear to have come from the University of Delaware. More collaboration may come in the future, with planned membrane and MEA testing at NREL, but this work was not discussed in a meaningful way.
- This is a project sharing resources between University of Delaware and NREL. No NREL work has been identified. NREL's work plan is not well defined. It may not be realistic for NREL to perform all membrane durability, fuel cell testing, and flow cell testing with $100,000 budgets. No interactions are specified besides NREL, and there is no clear pathway to get stable polymers from Rensselaer Polytechnic Institute (RPI). The project is about developing new AEMs, and it is not clear how the PI wants to transfer the major task to RPI.

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.

- Developing stable AEMs for flow/fuel cells is relevant to the Program. Adopting phosphonium into polymer structures may be valuable to study and give potential impact on flow/fuel cell developments. Developing redox-flow cells is also relevant to developing advanced fuel cells because both systems share some similar requirements, even though the current project does not have much system study.
- Development of stable AEMs is a key need for AEM fuel cells. It is unclear whether AEM fuel cells have the potential to achieve commercial relevance against PEM fuel cells because of their poor hydrogen oxidation reaction kinetics and lower intrinsic conductivity.
- The project goals are aligned toward making a membrane stable in a high-voltage flow cell. The flow cell battery environment is different from that for a fuel cell, but it is thought that a membrane stable in that environment should be stable in an alkaline fuel cell.
- Making anion-exchange ionomers that are stable to Ce(4+) is a good goal, but that goal is not so closely tied to the Program; it is more closely tied to energy storage, e.g., with a flow battery. The project is fine for what it is, but the focus seemed a bit mismatched with Fuel Cell Technologies Office (FCTO) goals from the start.

- The basic concept of high-voltage flow batteries is appealing. It is unclear whether all the materials are stable to the potentials and will have the required durability.
- The project seeks to advance RFB technology, which is mentioned in the FCTO Multi-Year Research, Development, and Demonstration Plan but does not represent a major part of the Program. The Program does not appear to be working toward any specific goals or milestones with respect to RFBs. Therefore, the relevance of this project is questionable.
- This project only partially supports Program goals. It is more a fundamental material research project.
- It is not clear how reversible flow batteries address U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program) goals. While the approach is directed at a membrane for high-voltage flow cells, the membrane should be applicable to AEM fuel cells and address stability issues of alkaline membranes.
- It is unclear to what application the double-membrane RFBs would be applied and what the technical requirements for such membranes should be. Even if the project reaches all of its milestones (high oxidative stability in an alkaline media), without any performance criteria (or even measurements) there is no expectation that the project will advance progress toward the Program goals and objectives. Perhaps it will demonstrate something about the fundamental stability of AEMs.

Question 5: Proposed future work

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

- The PI correctly notes that his problems have to do with synthesis, and he is working as best as he can to correct the problems.
- Future work does put emphasis on preparing membranes that could be placed in fuel cell or flow battery test fixtures. More emphasis on this type of work would be appropriate for the Program.
- The proposed future work addresses the failure in attempting to directly link the 9MeTTP+ to the polymer backbone and has some potential. Attachment to the backbone through one of the methyl groups should avoid the problems caused by steric crowding at the P atom and should prove feasible. It may be difficult to limit substitution/bromination to one of the methyl groups. Methylation of the P and then attachment through a phenyl methyl group could lead to decreased stability of the phosphonium cation, as the steric crowding at the P is less.
- Specified future work includes improvements to several synthetic steps, and a brief mention of testing in flow batteries or fuel cells, but more detailed discussion of the future work is needed.
- Future work to improve the ionomer stability seems well focused. More emphasis needs to be placed on ionomer characterization such as ion-exchange capacity or conductivity measurements. Clearer plans on testing in flow battery or fuel cell devices would be helpful.
- Conductivity evaluations are necessary.
- The project should put some emphasis on conductivity and performance. The project should also focus on mechanical and higher-temperature stability.
- Proposed work is not specific, and there are no clear plans for device testing. No milestones have been established based on the future plans listed on slide 17.

Project strengths:

- The project strengths are the new and novel material research work; the project does have the potential to develop game-changing materials for fuel cells and flow batteries.
- Using AEMs for innovative RFBs is of great interest. Model studies using different phosphonium cations give valuable insight for advanced AEM development.
- The aryl phosphonium organocations with which this PI works are very stable and could be the basis of a new generation of oxidatively stable AEMs.
- Development of a stable posphonium-based AEM may have applications beyond flow batteries. High-voltage flow batteries may have potential in high-power-density devices. Another strength is the systematic approach to identify stable polymer backbones and cation attachment chemistry.
- The project builds off earlier work performed by the PI in an ARPA-E project and successfully leverages that earlier work.

- Solid polymer chemistry approach and polymer characterization (infrared and nuclear magnetic resonance) are project strengths.
- The phosphonium cations chosen have high stability.

Project weaknesses:

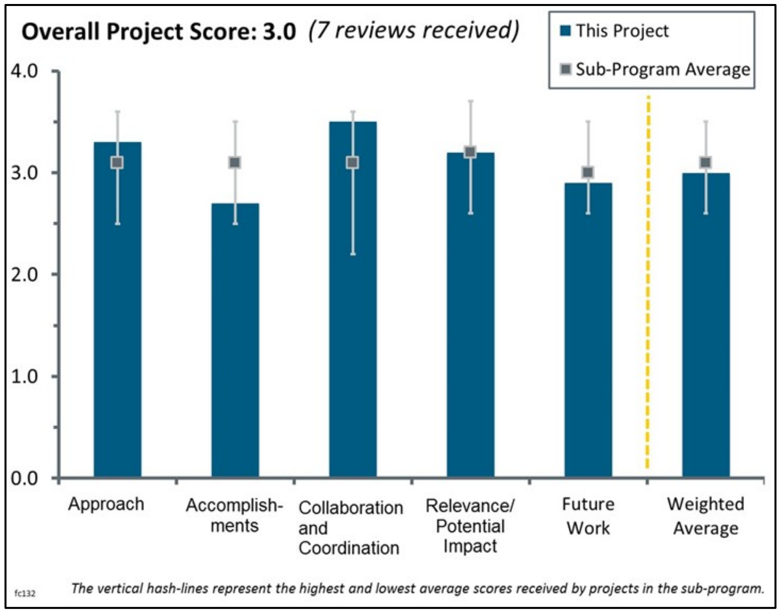
- The synthetic strategies have been difficult to implement.
- It is unclear whether the focus of the project is to simultaneously develop an ionomer and the Zn-Ce RFB chemistry. The roll of PEM and the related requirements are not discussed. More details on International Electrotechnical Committee standards testing or conductivity would be helpful. A good explanation of why ammonium cation AEMs are not suitable would also be helpful.
- So far, the synthetic routes from small molecules to ionomers have been difficult. Also, the connection to hydrogen technologies is not clear.
- The project is narrowly focused on alkaline membrane development for flow batteries and other devices, and relevance to the broader Program is limited. While the synthetic strategy was well presented, a lack of clear metrics or goals makes it difficult to assess progress.
- AEM property characterization was planned with only a stability perspective. Current polymer structure is somewhat deviated from the original proposed structure because of synthetic difficulties. This is acceptable; however, it is unprovable that the current structure can satisfy all requirements for redox battery AEM requirements. If the AEM is proposed in the use of RFBs, the project should have device performance targets rather than just ex situ stability targets.
- It is not clear how this project fits in the Program. No technical targets for performance can be measured.
- The project is not clearly tied to the Program objectives. The scope is limited to oxidative stability at low temperature. There is a lack of collaboration and no clear approach for concept down-selection.

Recommendations for additions/deletions to project scope:

- This project probably needs a go/no-go decision point to decide whether the synthetic approaches are giving ionomers that can be made into durable and ionically conductive membranes. It appears that the PI does not yet have that. His initial synthesis proposals are not giving what he needs, and the proposed new routes might give suitable materials, but they are yet untested. This is okay, but at some point, he has to meet a milestone for the ionomer, not just the parts.
- Stability tests should entail some more sophisticated diagnostics, such as testing leachate for organic residue or for P. Color changes can be misleading, and weight changes can be difficult with substituted materials because water adsorption can change and drying to the same state of hydration is not always easy.
- The project should add focus on conductivity, mechanical stability, and higher-temperature stability. The project should collaborate with a systems modeler to provide the technical targets required to enable successful commercial application.
- The project should focus more efforts on making membranes of any kind that can be built into fuel cells, even if very small. Also, this will allow other membrane characterization tests, such as conductivity. This will be a way to judge the technical readiness of this project for the Program.
- An anion conductivity (ClO_4 or sulfate) target should be added to the milestone. A mechanical milestone (or target) should also be set. Fuel cell testing is irrelevant and may be deleted. All stability assessments should be based on spectroscopic data and mechanical data in addition to internal combustion engine change. This may require third-party evaluation.
- The project should increase focus on characterization.

Project #FC-132: Innovative Non-Platinum-Group-Metal Catalysts for High-Temperature Polymer Electrolyte Membrane Fuel Cells

Sanjeev Mukerjee; Northeastern University



Brief Summary of Project:

This project is investigating the use and development of non-platinum-group-metal (non-PGM) electrocatalysts that would allow for high performance in high-temperature polymer electrolyte membrane (HT-PEM) fuel cells. A successful outcome will enable HT-PEM technology to be less dependent on Pt resource availability and lower membrane electrode assembly (MEA) costs by at least 50%. Benefits include increased energy efficiency, reduced carbon footprint, and improved U.S. energy security.

Question 1: Approach to performing the work

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

- Northeastern University (NEU) project is pursuing a promising approach to develop PGM-free catalysts for HT-PEM fuel cells. The use of PGM-free catalysts enables elimination of a high-cost component while also potentially avoiding the phosphate anion poisoning issue that has limited performance of previous phosphoric acid-based fuel cells. By further developing several related PGM-free catalysts based on carbon- and nitrogen-coordinated iron centers developed for low-temperature PEM fuel cells in a previous project, this project is leveraging previous work toward a new application.
- This work directly addresses the cost barriers for fuel cell technology implementation. The approach to catalyst development is effective. MEAs developed with these catalysts may have a very different implementation from those developed on traditional carbon or nanostructured thin film supports. Issues with MEA development have already been seen with IV characterization of the novel catalysts in the early phases of the Hydrogen and Fuel Cells Program (the Program).
- The approach to developing a variety of non-PGM catalysts for HT- PEM fuel cells is great and includes NEU's metal–organic framework (MOF) catalyst and the University of New Mexico's (UNM's) catalysts. These catalysts have great potential to mitigate anion (phosphate) adsorption. This is very significant. In addition, their synthesis a simple and facile.
- The project addresses the barrier of cost by attempting to reduce Pt loading in HT-PEM fuel cells by utilizing PGM-free catalysts. The project specifically addresses cost of stationary systems as HT-PEM is not applicable to automotive transportation systems because of the lower power density. The approach addresses phosphate poisoning, which decreases performance in HT-PEM fuel cells, and is addressing mass transport losses through the use of the sacrificial support method to create porous structures. The recent breakdown of potential losses indicates that transportation losses are the main issue. Work should focus more on electrode structure and reducing mass transport losses.
- The ball-milling approach and the use of sacrificial support (to generate porosity) and MOF materials are good for generating PGM-free cathode catalysts.
- Using non-PGM materials to replace Pt is critically important in reducing fuel cell cost. The non-PGM catalysts reported in this project represent the extension of materials developed from a project previously funded by the Fuel Cell Technologies Office. The stability of carbon-based non-PGM catalysts proposed by the project investigators is a major concern during high-temperature operation. These catalysts could be

oxidized rapidly in the presence of oxygen. The investigators need to develop a strategy to improve the oxidation resistance of their catalysts. The non-PGM catalysts of this project have different active site distribution from that of Pt-based catalysts. The humidification plays an important role in the proton transfer to the highly distributed active sites. Therefore, high-temperature, low-humidity operation seems to work against the usage of such materials, which may be a major cause of high overpotential.

- The replacement of Pt-based catalysts with non-PGM catalysts represents an enormous challenge. However, if successful, the approach could provide a solution to phosphate contamination of the cathode. Claimed improvement to mass transport and especially corrosion resistance characteristics of catalysts, thanks to the use of the sacrificial support approach, is not obvious. The team spared no effort in studying the active site in catalysts that showed poor activity in the fuel cell cathode. The benefit to the project objectives is not clear.

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.

- (1) The progress of this project seems to be a little bit behind schedule. The original targeted performance was for hydrogen/air but the demonstrated performance from hydrogen/oxygen is good and the hydrogen/air performance is poor. The principal investigator (PI) claims that further MEA optimization, with Advent's help, could help to improve the performance and it is hoped that better hydrogen/air performance can be achieved in the second year. (2) It is unknown whether UNM's two catalysts, the Fe-AApyr catalyst and Fe-Nicarbazin catalyst, were tested for fuel cell performance. (3) In situ x-ray absorption spectroscopy (XAS) is unique, helping to identify the active site of the catalysts. (4) RDE data comparison was very systematic, disclosing a good deal of valuable information.
- The project has made progress scaling up PGM-free catalysts. Current studies indicate phosphate poisoning is reduced in these PGM-free catalysts. Rotating disk electrode (RDE) experiments indicate PGM-free catalysts have higher activity than Pt in 10 mM phosphoric acid; MEA experiments indicate iR-corrected losses and transport losses are higher for PGM-free catalysts. It is not clear what differences there are between the catalyst preparation and scale-up here versus that already developed under the DOE-funded project studying non-PGM catalysts for low-temperature PEMs. The same techniques are used with the same descriptions.
- The team has made good progress toward meeting the milestones set in the project. As of now, MEA performance is not meeting the targets as set but ways to improve have been identified.
- The first milestone, which specifies good reproducibility of catalyst activity in RDE testing, was apparently met but was not adequately described in the presentation. The project has yet to meet the Year 1 fuel cell performance milestones for operation on oxygen and on air. Improvements in electrode structure have been proposed to meet these milestones but the justification for these improvements is rather weak. For both oxygen and air testing, the reported voltage loss breakdowns indicate that mass transfer losses are relatively small though the presenter indicated that the reported breakdowns may not be accurate. Electrode flooding was proposed as a significant loss mechanism but given the high temperatures and low humidities used, liquid water should not be present. More detailed reporting on the electrode structures used would be required to clarify the relevant loss mechanisms. Given the early stage of the project, there is still time to address these issues and a good chance for significant performance improvements.
- Progress is steady; however, one target (go/no-go 1) may not have been hit at the time of the presentation. It was not clear from the presentation or the supporting material whether the work presented included data to be evaluated 12 months into the project. There may have been a performance loss from what had been previously reported (2015 package) and what was presented at the Annual Merit Review. This may have been due to MEA fabrication; this was not discussed.
- Performance targets on oxygen and air were not met in this project. A pressure 1.7 times higher than the target value was required for ultimately meeting the milestone performance on oxygen at 0.7 V. The results point to an even larger gap between the demonstrated and target performances when the fuel cell is operated on air, with the demonstrated current density of ca. 40 mA/cm^2 five times lower than the milestone current density of 200 mA/cm^2. Switching to new MEA formulation has not helped so far. Much better tolerance of non-PGM catalysts than Pt catalysts to anions, including phosphates, has been known for

many years. Reassessment of that property in this project tied up the resources unnecessarily. High CO tolerance is promising.
- The project progress report is inadequate. The PI provided a milestone table but failed to include the most important column, i.e., the status update. The project is nearly at its mid-point. The status updates for milestone 1.1a, 1.1b, 2.1, and 1.2 should be reported and compared to the goal. The project is behind schedule.

Question 3: Collaboration and coordination with other institutions

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

- The team includes two universities and three industrial partners. The role of each team member appears well defined.
- The collaboration is great as the assembled team includes two industrial partners and two universities, all of which have good experience in catalyst development and fuel cell component design, respectively.
- The team members all provide useful know-how and have been collaborating well.
- The partners appear to be collaborating well. Scale-up is progressing, indicating collaboration between Pajarito Powder and the synthetic efforts of UNM and NEU is going well. Most of the partners have a history of collaborating and have worked well together in previous projects. It is not clear how much FuelCell Energy (FCE) has been involved in the project to date.
- The project features an excellent collaboration between NEU and UNM on catalyst development. Advent is a critical partner in supplying materials and helping with MEA integration. Pajarito Powder will participate in scaling up the catalyst but does not seem to have been significantly involved as of yet. The role of FCE is presumably in fuel cell testing, which has not yet commenced but this role should have been described more explicitly.
- NEU has excellent interaction with its technical collaborators. Integrating the technology developed in this effort with that of others in the fuel cell program may be difficult. The compatibility of the catalysts being developed with Nafion-type electrolytes seems uncertain (it is noted that this is not a focus of the present work).
- The project involves several organizations with complementary skills. The role of FCE is unclear, though.

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 highly relevant to the Program as it is following a promising pathway to improved performance and decreased cost of HT-PEM fuel cells. Furthermore, advancements in PGM-free catalysts developed through this project may prove helpful in developing PGM-free catalysts for low-temperature PEM fuel cells as well. If successful, the project could have a large impact in accelerating commercialization of stationary fuel cell technology.
- The project could have a substantial impact on the cost of HT-PEM fuel cells and the cost of stationary fuel cells. The project impact on low-temperature PEM fuel cells or fuel cell vehicles is expected to be minimal. The efforts focused on phosphate poisoning are specific to phosphoric-acid-based fuel cells. The MEA issues and low oxygen and proton transport are also related to phosphoric acid and are unlikely to transfer to low-temperature PEM fuel cells.
- High-temperature fuel cells will have significant impact on the combined heat and power (CHP) application, as proposed.
- By addressing the HT-PEM cathode catalyst challenge, this project is well aligned with the Program goals for CHP systems.
- If successful, the project results would be very meaningful for the Program mission because of the following advantages: 1) the application of non-PGM would enable cost reduction of fuel cell components and 2) HT-PEM fuel cells would alleviate the CO positioning and heat management.
- If successful, the project will align well with the goals and objectives of the Program.

- The project is relevant to the Fuel Cell Technologies Office's goal.

Question 5: Proposed future work

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

- The proposed future work includes optimizing the MEA fabrication. This is very important and will address some issues.
- The proposed future plan is great as the PI prioritized the MEA design to further improve the catalyst performance in a fuel cell. The approaches to improving the MEA performance are very detailed. The other plan the PI should also discuss is the modification of the project's original targets to DOE targets.
- Future work is focused on improving MEA and electrode design, attempting to address the mass transport and iR losses and address flooding issues. The durability of these PGM-free catalysts in HT-PEM systems is a question. PGM-free catalysts have not had the durability required for low-temperature PEMs. It is not clear that the higher temperature involved in HT-PEMs will not accelerate corrosion of these systems. The project should address durability of these catalysts.
- The chief problem seems to be mass-transport-related and the team has identified ways to address this. It is also important to improve the activity of the catalysts.
- The future work presented included a good discussion of the planned electrode development work but discussion of durability studies was surprisingly absent. The milestones table indicates significant work that will be performed on characterizing and validating durability so it is not clear why this was not described in the future work. Further improvements to the catalyst and scale-up efforts were also not discussed though it appears that these are intended to be part of the Year 2 effort.
- This project needs radical solutions to the challenges identified to date. The performance is much below the interim targets. Proposed MEA optimization (Teflon content, MEA annealing conditions, tweaks to hot-pressing) is not likely to ensure significant progress. There seems to be no Plan B.
- The PI identified no clear research path to overcome the major gap between the current catalyst performance and the project goal.

Project strengths:

- The project brings together an excellent team with significant experience and expertise in PGM-free catalysts and HT-PEM fuel cell development. The approach and the planned work represent a promising path to achieving project goals and accelerating the commercialization of stationary fuel cells.
- This is an innovative approach to eliminating PGM from fuel cells. The use of HT-PEM fuel cells is also beneficial toward addressing the implementation of batteries into fuel cell technology, especially the transportation sector.
- This is a great team with excellent experience in individual areas. Other strengths include the diversified non-PGM catalyst development and the in situ XAS characterization and systematic RDE design.
- The project partners have a strong collaboration. The project team has unique expertise in PGM-free catalysts and catalyst characterization techniques.
- The team members are all well-established researchers and bring to the project complementary skills that are essential to the project's success.
- A strong team and skillful catalyst characterization have been this project's biggest strengths.
- This is a good characterization effort providing interesting insight on the active site.

Project weaknesses:

- Catalyst compatibility with traditional PEM fuel cell electrolytes is a weakness (but is not a focus of this project).
- The project is relatively new but it has already missed two milestones and the proposed path for delayed completion of these milestones is not convincing. While characterization of transport losses is supposed to be a significant part of the project, the effort in this area seems weak so far, and the reported mass transport overpotentials and loss mechanisms do not seem accurate or realistic.

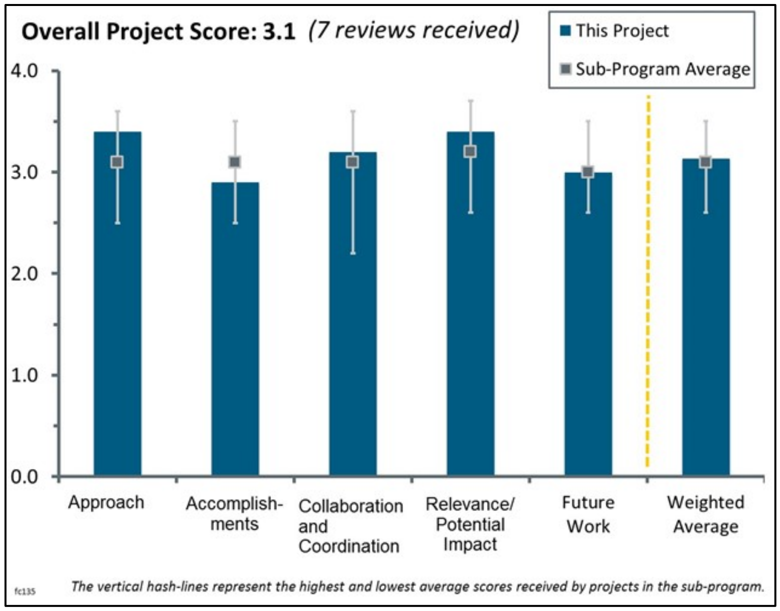
- Given the high level of funding for universities, project achievement has fallen far behind schedule. This is particularly true since the project is based on previous DOE-funded work. The reported catalytic activity is well below the expectation.
- The project should focus on the major deliverables: MEA performance, especially when these targets are not yet met. In this context, the basic characterizations, such as Mossbauer and x-ray absorption near edge structure (XANES)/x-ray photoelectron spectroscopy (XPS) should be less emphasized.
- The fuel cell performance, particularly for hydrogen/air, was not satisfactory. It is hoped that the performance can be improved in the second year following the “future plan.”
- There is little flexibility in the approach, especially in confrontation with lower-than-expected MEA performance, calling for sweeping solutions.

Recommendations for additions/deletions to project scope:

- The team needs to assess the origins of poor cathode performance. It is not clear whether flooding or simply insufficient activity of non-PGM catalysts is the cause. There is growing evidence in the field of non-PGM electrocatalysis that encapsulated metal salts and other metal-rich phases in non-PGM catalysts play no role in the oxygen reduction reaction. Such catalysts may actually be less active than formulations with highly dispersed iron or another transition metal. The team should identify the most promising formulation and focus on it in the second year of the project.
- The technical status, as of now, is significantly below the project targets. At this point, the PI should focus on addressing catalyst performance improvement.
- The report should clearly name the different types of catalysts and use consistent labels so that it is easier to follow the characterization data and MEA performance of each catalyst. The non-MEA (synthesis/scale-up) milestones were not specifically mentioned as being met.
- The PI should demonstrate the performance of alternative catalysts from UNM.

Project #FC-135: Fuel Cell Consortium for Performance and Durability – Consortium Overview

Rod Borup; Los Alamos National Laboratory



Brief Summary of Project:

The Fuel Cell Consortium for Performance and Durability (FC-PAD) coordinates activities related to the denoted development areas and supports industrial and academic developers. This effort aims to advance performance and durability of polymer electrolyte membrane fuel cells (PEMFCs). Researchers will develop the knowledge base and optimize structures for more durable and high-performance PEMFC components; improve high-current-density performance at low Pt loadings; improve component durability; and develop new diagnostics, characterization tools, and models.

Question 1: Approach to performing the work

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

- The approach of bringing in the resources and talents of five national laboratories and many different suppliers and academia in a consortium to harmonize the fuel cell activities and share/understand the merits/limitations of available state-of-the-art (SOA) materials/technologies is a well-thought-out approach. This approach will certainly help with synergy in research and development (R&D) activities in different organizations and with developing a common understanding of all SOA materials/technologies, which may help the fuel cell community to have a consensus in the status of the SOA materials/technologies available from different suppliers. The overall structure of the consortium seems to be logical and includes furthering the scope by integrating new members into the consortium.
- The FC-PAD approach with six thrust areas is excellent and designated to national laboratories with the appropriate core capabilities. Coordination between these thrust areas (as shown in the presentation as an example) is well structured and thoughtful. Barriers are very relevant and clearly focused on catalyst layers—the current industry need—as a key component.
- The overall approach of the project consists of modeling and characterization of fuel cell components to improve their durability and performance. The fact that the objectives are split between the different thrust areas (characterization, performance testing, and modeling of different cell components) is a very efficient way to fully understand the related loss of performance and to address the durability issues.
- The proposed approach is excellent for advancing the performance and durability of PEMFCs, developing the knowledge base, and for optimizing structures for more durable and high-performance PEMFC components. The approach is excellent because it aims to benefit the fuel cell community by providing a better understanding of materials evolution in the membrane electrode assembly (MEA) components.
- The FC-PAD approach appears to be a good method to get even more collaboration and interaction between various fuel cell researchers at the national laboratories (although it is impressive that most of the laboratory researchers have been reasonably collaborative lately). What is missing is even more engagement with industry and universities (although new awards should help), but the amount of outside participation is already good.
- FC-PAD is a strong effort to coordinate/bring focus to a wide array of researchers to address the technical barriers to fuel cell development.

- Of the techniques shown on the capabilities highlights slide, the following may duplicate efforts/capabilities of some component developers: catalyst activity measurement, advanced MEA fabrication (especially with a bench-scale ultrasonic spray system), electrode simulations, multiphysics multiscale models, advanced MEA diagnostics, and transport property measurements. Of the techniques shown on the capabilities highlights slide, the following likely complement efforts/capabilities of component developers: analytical electron microscopy (beyond conventional SEM scanning and transmission electron microscopy [TEM]), advanced x-ray techniques (beyond x-ray diffraction [XRD], x-ray fluorescence [XRF], x-ray photoelectron spectroscopy [XPS]), x-ray tomography, and bulk and thin film morphology and properties. A high fraction of the effort should be devoted to techniques that may not be immediately available within to a stack developer. Certainly x-ray and neutron beams are part of this, but there could also be some electrochemical or fuel cell techniques that veer considerably off the beaten path. The combinatorial cell at Argonne National Laboratory serves as one example. Different types of segmented cells also come to mind, especially if the segmentation includes a diagnostic technique analyzed by segment (e.g., gas crossover, cell resistance, limiting current, etc.).

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 first- and second-quarter deliverables of all five national laboratories in FC-PAD have been completed, and some of the publications have been submitted or are in the process of submission. This shows good technical progress and accomplishments, and the team is on track to meet the deliverables for fiscal year 2016. It is good to see that the team successfully launched the website in time, which is a great accomplishment as this helps external customers (would-be members) to get information about the consortium and its activities.
- This is the beginning period of FC-PAD, and overall, all the national laboratories have made very good progress in the given time. The focus is clearly aligned with research needs. Most of the quarterly milestones have been met.
- Since the project start, the team has been very productive between the various thrust areas. A huge amount of data has been collected, and the project team seems to be highly integrated and collaborative.
- Overall, the FC-PAD team has already done an impressive amount of work. However, it is recommended that the FC-PAD overview presentation should *not* attempt to present any of these results in any detail (especially because most of the different thrust areas are presented individually). Instead, this overview should focus on (1) what the team has decided to focus on over the past year (or appropriate period), (2) why the team focused on these topics (vs. other options), (3) how the work was assigned to different thrust areas and how the different groups are interacting, (4) high-level key learnings and how they affect what will be focused on next, and (5) the future focus of FC-PAD.
- The project clearly addresses the DOE targets in terms of durability, performance, and cost (at a lower level). The durability studies concern the components (gas diffusion layer [GDL], cathode catalyst layer [CCL], MEA, membrane) and not (or perhaps not yet) the stack/system. Therefore, to assess the durability target set by DOE, validation at system level should be undertaken. Several consortium milestones have been completed on time.
- FC-PAD has a great start. The progress of vetting new consortium collaborators is an issue; this may have taken too long to implement (about six months).
- Results with regard to FC-PAD thrusts apply to other projects and should be evaluated in those project reviews. It appears the progress in this project accounts for the establishment of a website as well as numerous meetings and presentations. There are some operational points of progress that should be noted, however. No mention is made as to whether the non-disclosure agreement for working with all five laboratories has been established. It is not clear how data are to be managed other than by using the website for reporting data. It is unknown whether FC-PAD will have proper data security when needed. File transfer websites are not trivial and need to be set up. No progress is shown in this respect. The model that derives from FC-APOLLO (or other past modeling efforts) should be incorporated. It would be good to see what the strategy might be with regard to model inventory.

Question 3: Collaboration and coordination with other institutions

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

- Because the consortium brings in all the important research organizations that are involved in fuel cell R&D, there will be ample opportunities for the consortium members to conduct collaborative work and technology/knowledge transfer for mutual benefit of the member organizations and hence toward the advancement of fuel cell technology. The list of institutions involved in the consortium (slides 19 and 20) is impressive and given the fact they are all connected to one another, the consortium is expected to foster many new collaborative activities that otherwise would not have been possible.
- The main purpose of FC-PAD seems to be to increase national laboratories' interaction with industry and academia and use national laboratory core competencies to support industry and academic technology development. Current FC-PAD members (national laboratories) are working with many collaborators and will work with DOE-funded project teams.
- The project seems well organized with strong coordination (a director and deputy director) of the overall consortium and a coordinator for each thrust area. The consortium seems to have good collaboration with external partners, including laboratories, universities, and international institutions with complementary expertise—and there is a future plan to integrate new organizations.
- The project has collaborated with many partners. This project is highly collaborative with the overall aim of servicing the fuel cell community.
- FC-PAD is a vehicle for collaboration; it will have great interaction.
- Collaboration among the FC-PAD members appears to be excellent. However, there are a couple of minor areas in which more coordination between different thrust areas could be improved, e.g., ionomer studies and electrodes. Interactions with outside groups are very good at this phase without any more formal engagements in place.
- Obviously, collaboration is everything to FC-PAD, but for this particular part (FC-135), the evaluation should be done based on the collaborations relevant to the mechanics of setting up FC-PAD. What is relevant to consider is how well the national laboratories are collaborating with each other to establish legal frameworks, data management, and objectives. There appear to be some difficulties in setting up data management and non-disclosure agreements, which points to a lack of collaboration between laboratories. It would also be good to see the FC-PAD laboratories able to collaborate with DOE user facilities that may be administered by the Office of Science, NNSA, and other agencies.

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 creation of the FC-PAD consortium is a great Fuel Cell Technologies Office initiative with a goal to provide technical expertise and harmonize activities of national laboratories with industrial developers. This initiative is relevant to the objectives of the Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The activities are aligned with DOE's goal to address the commercial barriers, such as performance, cost, and durability.
- The key technical barriers are performance and durability, especially (1) beginning of life performance with SOA catalyst loadings at high current densities and (2) high-activity alloy catalyst durability, which is not meeting targets. The FC-PAD team is certainly focused on addressing these key issues and more.
- The FC-PAD consortium and project is critical to the Hydrogen and Fuel Cells Program and has the potential to significantly advance progress toward DOE research, development, and demonstration goals and objectives with respect to improving the performance and durability of PEMFC systems.
- The main objectives are improving fuel cell durability and performance while lowering cost. These are the most important targets set by DOE in the MYRDDP. The main mechanisms of component degradation are addressed in the different thrust areas for a better understanding leading to a better mitigation solution.
- If successful, this project will make important strides toward addressing issues of cost and durability for fuel cell implementation.

- The objectives of FC-PAD align well with the DOE R&D objectives.
- There are some serious difficulties with understanding the relevance of FC-PAD. The key question is who the customer is, i.e., who is benefiting from this effort. Of the partners that are listed so far (outside the FC-PAD laboratories themselves), there are nine catalyst or MEA suppliers, five universities, four international research institutes, one U.S. government laboratory, and General Motors (GM). The suppliers are providing materials, but to prevent public disclosures of confidential technology developments, the materials are not likely to be the suppliers' latest and most cutting-edge. Except for GM, none of these partners will be designing unit cells and stacks. It is the designers of unit cells and stacks that would be thought to have the most to gain from the knowledge acquired by the project, but that does not appear to be developing. GM has had a long relationship with DOE fuel cell efforts, so its partnership could be expected, but automakers that have recently introduced vehicles are not part of this effort. Membrane stabilization is often obtained through additive packages that are highly confidential. It is unclear how a public project will be able to build upon supplier efforts that have already contributed to SOA membranes. Optimizing structures apart from the use of a fairly high-volume catalyst-coated membrane, GDL, or bipolar plate production line would appear to be, at best, an academic exercise. Developing a knowledge base about materials and structures, performing fuel cell tests, and modeling performance and durability appear to be what happens inside an automotive stack developer. This project must avoid being a duplication of effort with automakers.

Question 5: Proposed future work

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

- The overall proposed research for the consortium and for individual thrust areas are well-thought-out, thorough, and aligned with DOE goals. The project covers individual goals and necessary activities required to address the challenges related to components (catalyst, electrode, ionomer, GDL) and methods (characterization, modeling, operational conditions).
- The proposed future work to define mechanisms for collaboration is appropriate, as is identifying supporting roles for the FC-PAD core national laboratories.
- The proposed future work for project organization corresponds to the immediate needs of the project.
- The project has planned its future in a logical manner. There are issues with the timeframe of adding collaborators and the duration for incorporating new collaborators; incorporation should be streamlined.
- FC-PAD is only few months old, so more time may be needed to make the future work planning concrete.
- The future work for the Consortium should not be very brief summaries of the future focus of the various thrust areas (because this is a repeat). Instead, future work should be a high-level explanation of the overall strategy of FC-PAD, specifically what the key remaining technical barriers are, how the Consortium is going to prioritize these barriers, and what new capabilities FC-PAD may require to better address these barriers.
- The Future Work slide addresses the future work of the thrusts but does not adequately address the future work associated with setting up FC-PAD. There are obvious mechanics of collaboration that are still missing at this stage. The future work should focus on what the individual laboratories are going to do to make sure they work better with each other and with all the partners. The focus should be on streamlined processes for working with national laboratories as well as data sharing and security. There should also be some consideration of how to involve more stack developers in the work.

Project strengths:

- The project has access to a considerable amount of equipment and resources. The project has managed to generate interest from at least nine MEA or MEA component suppliers. The investigators have combined years of experience in fuel cells that can probably be measured in centuries.
- The main strength of the project is the team, which constitutes the ensemble of all subject matter experts required for such broad activities. The team has all necessary technical expertise and equipment resources needed to conduct the proposed R&D for FC-PAD.
- FC-PAD is utilizing each of the participating national laboratory's core competencies to meet the DOE R&D goals and addressing the current research barriers. Dividing into six thrust areas to cover all the required fields/needs to advance fuel cell technology is well received.
- The project appears to be further improving collaboration between the national laboratories. The investigators of the Consortium and the thrust areas are outstanding. The new website is a strength.
- Having strong national laboratories with complementary expertise is a good guarantee for success. Splitting the tasks into different thrust areas allows the project to treat each one of the components and its degradation mechanisms in a rather complete and deep way.
- Collaboration with researchers with varied backgrounds is a strength.
- The project's strengths include the excellent research and the unique capabilities of the core team.

Project weaknesses:

- No weaknesses were specifically identified; however, the mechanisms for collaboration and cooperation with industrial partners and data management can be a challenge in terms of impact as the FC-PAD activities are coordinated across six different thrust areas.
- Although engaging different commercial entities in the consortium and getting their SOA materials/ technologies for evaluation is a very ambitious initiative, it will be very difficult to manage such activities unless a robust intellectual property/non-disclosure/confidential disclosure agreement is in place. The team should have clear understanding of the intellectual property ownership and legal pitfalls that often come with such a broad coalition of R&D entities.
- The project will likely have difficulty accessing SOA materials sets. The project is lacking a clearly defined customer. The probability of overlapping stack developer efforts is very high. The project cannot guarantee access to user facilities other than those of the Office of Energy Efficiency and Renewable Energy. There are still some matters to sort out with regard to data-sharing and streamlining the interactions between a given party and all five core laboratories.
- Integration of new partners and coordination of the whole consortium could be a weakness if strong communication means are not clearly set.
- The path forward to work with DOE-funded project teams is not clear. The extent to which the collaboration with new partners will be made is not clear.

Recommendations for additions/deletions to project scope:

- It will be interesting to see how the project milestones for the FC-PAD national laboratories related to newly awarded projects evolve and how outreach and impact evolve.
- The work focuses on components and the related degradation mechanisms and studies each one of them in a rather deep and complete way. The question is whether the methods developed (models and characterizations) are useful in dissociating some close degradation mechanisms (for instance, catalyst growth/agglomeration mechanism and carbon corrosion). Otherwise, the results do not enable a better understanding of these degradation mechanisms and how they affect each other. Validation at system level with real-world operation does not seem to be part of the future work. This could be helpful in validating the improvements done on different components within a complete stack/system. The project should investigate the effect of simultaneous occurrence of degradation mechanisms (for instance, at catalyst and membrane level) vs. the impact of each of them individually on cell durability and the potential consequential impact on the accelerated stress test.
- Extra effort should be directed toward secure data management for all partners. Emphasis should be put on finding ways to streamline the process of working with all five national laboratories. The project should

attempt to remove all activities that overlap with what stack developers are doing. The project should seek fuel cell testing techniques that are novel and go far beyond just taking polarization curves and running diagnostics. The project should also seek running diagnostics in a segmented fashion, in situ material characterization, and other things that only a national laboratory might be able to devise. If stack developers are doing something, national laboratories should seek to go deeper and further to go beyond that "something" and add to it.

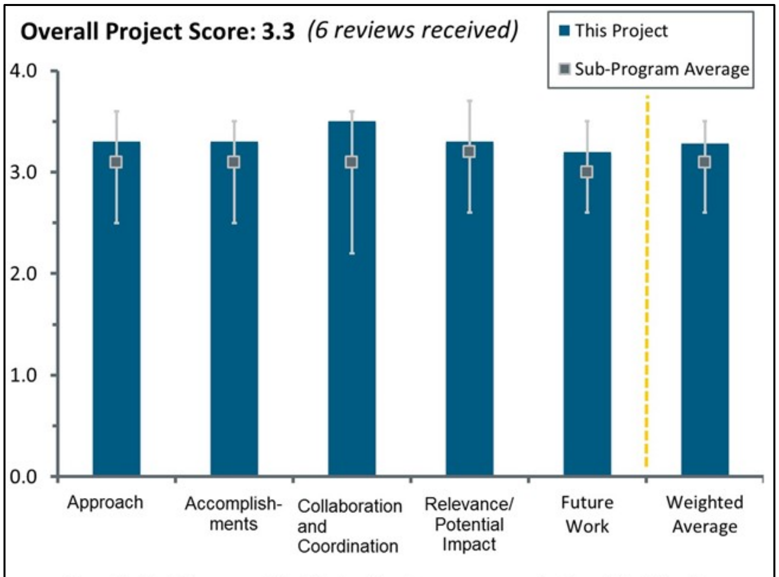
- The project should allow new collaborations on a one-year project effort with an optional one-year follow-on based on performance review. If an activity does not make it to the second year, others would be given an opportunity to contribute.
- The presentation on the Consortium should be more of a high-level strategy explanation.

Project #FC-136: Fuel Cell Consortium for Performance and Durability – Electrocatalysts and Supports

Debbie Meyers; Argonne National Laboratory

Brief Summary of Project:

The Fuel Cell Consortium for Performance and Durability (FC-PAD) coordinates activities that advance performance and durability of polymer electrolyte membrane fuel cells (PEMFCs). FC-PAD efforts include six complementary thrust areas including one on electrocatalysts and supports. This thrust area aims to realize the oxygen reduction reaction (ORR) mass activity benefits of advanced platinum-based cathode electrocatalysts in high current density, with air performance for over 5,000 operating hours, and with low-platinum-group-metal (PGM) loading.



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 team has correctly approached the problem of understanding the catalyst and support degradation mechanisms, understanding mutual interactions between the catalyst and support, and quantifying the impact of catalyst degradation on cell performance using high-resolution transmission electron microscopy (TEM) and x-ray diffraction (XRD) techniques supported by modeling inputs. The team is equipped with subject matter experts and state-of-the-art (SOA) equipment to conduct these studies.
- Catalyst stability and interaction with other electrode components are not fully understood and research on these topics is key to achieving performance, cost, and durability targets. The approach is well designed: elucidating catalyst and support degradation mechanisms as a function of catalyst and support physicochemical properties and cell operating conditions and quantifying the impact of catalyst degradation.
- FC-PAD is a strong effort to coordinate/bring research focus to address the technical barriers to fuel cell development. The focus of this work is on electrocatalysts and catalyst supports. This work could possibly have the highest impact toward achieving cost and durability targets of the U.S. Department of Energy (DOE) Hydrogen and Fuel Cell Program (the Program).
- This is Thrust Area 1 of FC-PAD. The objectives and approach are very clear.
- The overall approach is excellent since it emphasizes focusing on fundamentals and understanding of mechanisms. However, one aspect that appears to be missing is a subsequent goal to distill these learnings into recommendations to the community on how to improve performance and durability of these key components. For example, after determining the key issues with PtCo and PtNi, the team should provide recommendations on how to improve these materials. The project should determine whether the recommendations for these two different alloys would be the same or different.
- Most of the project focuses on taking a roster of commercial catalysts, as well as catalysts developed in DOE-funded projects, and using them to develop fundamental degradation relationships with respect to voltage or voltage cycling. This presents a few problems. First, while the study has been very high-quality and the results are very systematically organized, the results are very familiar to stack developers. The project needs to stay away from doing a very good job studying what stack developers already know. Second, most of the results (not all) are carried out in glass-cell or ex situ environments, which are different from a fuel cell environment in terms of water activity, proton activity, and many other factors. Third, the

project needs to show validation that the relationships derived from ex situ techniques are relevant to what occurs in a fuel cell. To some extent, this is being pursued for carbon corrosion although factors such as temperature and relative humidity also need variation to provide information that developers can use. While the attempt is made to generate systematic degradation data, the different ways in which suppliers treat their catalysts will introduce noise factors to what otherwise would be fairly clean trends. Some accounting needs to be made for this in any modeling efforts.

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 made significant accomplishments toward the objective of identifying Pt alloys that meet or exceed the DOE 2020 ORR activity target (>440 mA/mg Pt). The results from potential cycling to determine the Pt dissolution rate are also very interesting. The Pt dissolution rate was found to be lower for IRD Fuel Cells (IRD) “spongy” Pt_3Co compared to TKK “solid” Pt_3Co, whereas the Co dissolution rate is much higher. This is a great piece of information and clearly shows the merit and limitations of spongy and solid catalysts. The results from the study of thermodynamic and kinetic rates of PtOx formation are relevant to the understanding of the long-term durability/stability of individual catalysts. The measurement of carbon corrosion during drive cycle on three types of carbon supports is also relevant for the goal of understanding the stability of catalyst supports.
- Excellent progress has been made in the given time and with a given set of catalysts under DOE projects. Much work has been done on the PtCo catalyst system and carbon supports.
- The accomplishment of the team is solid and well-coordinated, and is taking methods and results from other projects into account.
- The FC-PAD electrocatalyst and catalyst support thrust already has a wide group of partners/collaborators contributing to baseline work. This is/was an important step for evaluating the new collaborators. The crossover between work from other thrust areas (electrode layer and ionomer/gas diffusion layer) may not be completely defined.
- The number of accomplishments to date is impressive. However, with respect to the presentation, a summary of the oxidation and dissolution mechanisms would be very helpful (there are seven slides on this topic but no summary). The summary on carbon corrosion (slide 23) is great and a good example of what should have been included on the catalyst loss results.
- The systematic study of degradation with voltage or upper voltage limits is very good but there are many other parameters associated with practical fuel cell operation: humidity, temperature, pressure (which translates to oxygen activity), and flow rates. If the information generated in this project is to be of any practical use, it must be incorporated into a model with some ability to predict lifetime. Such a model will prompt questions as to how degradation rates change, not just with voltage and different particle sizes but also with temperature, humidity, ionomer content, Pt weight percent, and other operating conditions or design factors. For what the study has attempted to do, it is very good. The dissolution rates of Pt versus voltage are well plotted and agreeable with other data. The carbon corrosion information also corroborates with other data. However, the unfortunate part of this is that for a publicly funded project, saying the results agree with other data means that others have already studied the same phenomena. With the results as they are, it may be possible for a developer to incorporate some trends into a model that presumes a very limited set of catalyst layer design parameters and operating conditions but that may be the full extent of the usefulness of the data. The project needs to think deeply about what it is that developers still need to predict stack lifetime.

Question 3: Collaboration and coordination with other institutions

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

- Since the consortium brings in all the important research organizations that are involved in fuel cell research and development (R&D), there will be ample opportunities for the Consortium members to conduct collaborative work and technology/knowledge transfer for the mutual benefit of the member organizations and hence toward the advancement of fuel cell technology. The list of institutions involved in the Consortium is impressive and given the fact they are all connected to one another, this project is expected to foster many new collaborative activities that otherwise would not have been possible.

- A positive thing is that the project has engaged most of the most likely catalyst suppliers, but others should also be considered. Automakers and other stack developers need to be better engaged so that the project can understand what data should be delivered to enhance models for predicting stack lifetime. The project should determine, for example, whether there are temperatures or temperature cycles that need to be considered, how humidity should be varied, and how catalyst layer design has an influence. It is good to see a wider representation of the other DOE-funded project materials. The University of South Carolina materials have needed to be included in a project like this for a while.
- There is good collaboration with industry and other academic partners. Collaboration with other thrust areas is mentioned but not evident from results. Collaboration should not be limited to catalysts and supports developed under DOE projects only.
- There are good, expanded collaborations between material and component developers and existing collaborations are well-maintained.
- FC-PAD is a vehicle for 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 **3.3** for its relevance/potential impact.

- The creation of FC-PAD is a great Fuel Cell Technologies Office initiative with a goal to provide technical expertise and harmonize activities of national laboratories with industrial developers. This initiative is relevant to the objectives of the Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The activities are aligned with DOE’s goal to address the commercial barriers, such as performance, cost, and durability. This project is in the thrust area “Electrocatalysts and Supports,” whose objective is to realize the ORR mass activity benefits of advanced Pt-based cathode electrocatalysts in high current density, with air performance for over 5,000 operating hours, and with low0PGM loading (<0.1 mg Pt/cm^2). The objective is aligned with the MYRDDP.
- This thrust area is very well focused and well executed to overcome major obstacles in fuel cell performance and durability.
- If successful, this program will make important strides toward addressing issues of cost and durability for fuel cell implementation.
- The team’s focus on durability issues is warranted based on the current status of SOA PEMFCs.
- The project’s aspects align well with DOE R&D objectives.
- It is difficult to see how the thrust area becomes relevant to advancing the Program. Suppliers deliver catalysts that are not SOA for testing and then trends that may have already been familiar to developers years ago. This is the fundamental problem with being able to say that the project ultimately supports Program objectives. As with other areas of FC-PAD, the project suffers from not doing enough up front to identify customers and deliverables. It is not clear who the customers are or what they need. If the customers are identified to be stack developers, many of whom are automakers, the unfortunate truth is that many of these developers are already very familiar with the potentials at which various modes of Pt oxidation and dissolution occur; developers are familiar with surface oxides on carbon and have models to describe not only CO_2 evolution but also how it varies with Pt weight percent, carbon types, temperature, humidity, and other factors. One example of what a developer might need is a quick screening tool that is ex situ and can predict catalyst lifetime. However, this project is not presently driving toward such a goal or objective.

Question 5: Proposed future work

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

- The team's proposed future research is aligned with the overall objectives of the Consortium. The study of IRD and Umicore Pt_3Co catalysts using TEM-EDAX, x-ray fluorescence (XRF), extended x-ray absorption fine structure (EXAFS), Delta-μ x-ray absorption near edge structure (XANES), oxygen permeability, carbon corrosion, and Pt dissolution studies will provide better understanding of how these catalysts behave under different cell operational and accelerated stress test conditions.
- Future work is well-thought-out and clearly focused on addressing the challenges for PtCo and understanding the degradation mechanism. A PtNi or catalyst system will not be very different from the PtCo system, but other newly developed high-ORR catalysts should be added to the future work. Future work is technically focused but does address the way the project will work with DOE-funded project teams.
- The project has planned its future in a logical manner. It is not clear whether specific decision points will be made with respect to collaborator performance. This effort should allow for great research toward achieving Program objectives.
- Studies of effects such as Pt dissolution as a function of carbon type and correlation with changes in particle size distribution and electrochemically active surface area could be expanded to include studies of a broader range of Pt alloys.
- More work on PtNi, especially dealloyed PtNi, would appear to be warranted based on results shown with these materials.
- There are many experiments listed that are functions of voltage but not of temperature or water activity (with the exception of oxygen permeability in catalyst layers). Many of the experiments listed drive toward understanding degradation rates of Pt, carbon, or Pt in the presence of different carbon types. However, the rates themselves must be validated through insertion into a cell model and then through subsequent cell testing. The feedback loop is not well represented here, which calls into question what the overall deliverable might be. If the overall deliverable is to measure degradation rates only, this may be good for generating publications but not for assisting developers that may already have insight on degradation rates. The claim is that Delta-μ analysis on XANES will be done in collaboration with General Motors (GM). It is not clear whether the project has the capability to perform Delta-μ without GM's assistance.

Project strengths:

- The project is able to carry out degradation testing in a very systematic fashion, perhaps even better than most other DOE-funded projects. The project personnel are very skilled in analysis of catalysts and have numerous resources available to them. Numerous catalyst suppliers are listed as partners. Data presented for Pt and carbon degradation are corroborated elsewhere.
- The overall proposed research for the Consortium and for individual thrust areas is well-thought-out, thorough, and aligned with DOE goals. The project covers individual goals and necessary activities required to address the challenges related to catalysts and catalyst supports used in different commercially available and developmental catalysts.
- The approach to addressing the key challenges to understand the durability issues is very clear. Academic and industry collaboration is good, with most of the high-ORR catalysts under study or planned for study.
- This project has a strong team that seeks input from collaborators to remain relevant and to effectively achieve advanced fundamental understanding.
- A project strength is the major focus on durability (instead of performance) of catalysts and supports, which is good since this is a metric that is not being met, especially with high-activity Pt-alloy catalysts.
- Collaboration is a strength.

Project weaknesses:

- The project lacks an overall direction as to who the customer is and what is to be delivered. There needs to be further direction to understand how data will be used. Suppliers will likely not be compelled to deliver their most SOA catalysts. Many of the data generated will show trends already familiar to developers.

Experimental design needs to be more open to variations in operating conditions and catalyst layer design factors.

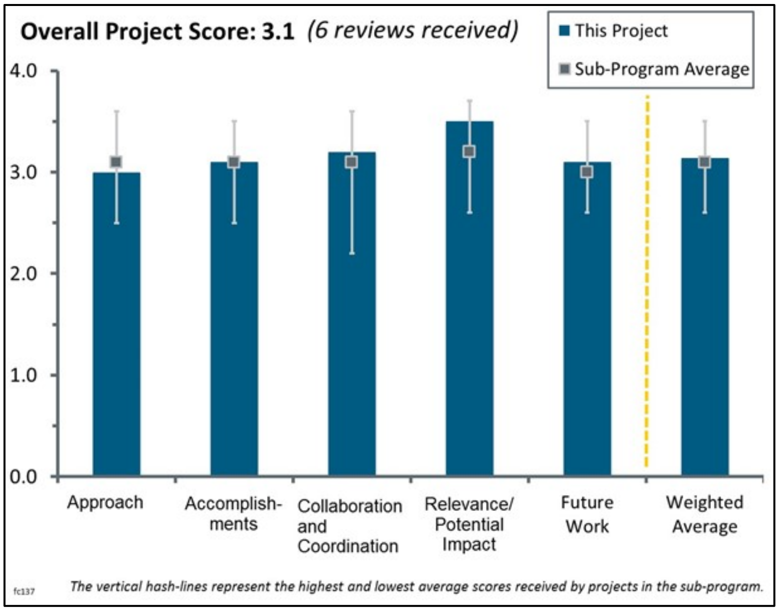
- Although engaging different commercial entities in the Consortium and getting their SOA catalysts for evaluation is a very ambitious initiative, it will be very difficult to manage such activities unless a robust intellectual property/non-disclosure/confidential disclosure agreement is in place. The team should have clear understanding of the intellectual property ownership and legal pitfalls that often come with such a broad coalition of catalyst manufacturing and user companies.
- There are no criteria or mechanism set for future collaboration with DOE-funded projects. Newly developed facet- and/or shape-controlled catalysts should be included in the study. The collaboration mechanism or extent of collaboration with other FC-PAD thrust area members is not well defined.
- The project should be cautious in the work to apply a catalyst corrosion model to membrane electrode assembly (MEA) data. The interaction and interplay between other MEA components should be considered.
- There is a lack of simple summaries of learnings to date, which should ideally also include recommendations on knowledge to date (e.g., whether one should utilize solid or spongy alloys).

Recommendations for additions/deletions to project scope:

- Stronger communication and collaboration with other FC-PAD members (national laboratories) on methods such as in situ TEM to understand the degradation mechanism will be very helpful.
- It would be interesting to include mitigation strategies in the future work on applying a catalyst corrosion model to cell data using TEM-EDAX and XRF quantification of Pt and Co in cell components. Overall, this is well-executed and very interesting work.
- The project should remove tasks that appear to overlap with what developers are already doing or that do not contribute to an overall deliverable. Customers should be identified and the project should find an overall objective or vision of how customers will use data. The project should collaborate with other FC-PAD thrusts to understand how data could be used and validated versus cell testing. Emphasis should be added on other cell operating conditions such as temperature and humidity.
- In future presentations, the project should include more brief summaries of key learnings (and less detail). In scope, the project should be sure to strive to make recommendations to the community on how to make future improvements.

Project #FC-137: Fuel Cell Consortium for Performance and Durability – Electrode Layer Integration

Shyam Kocha; National Renewable Energy Laboratory



Brief Summary of Project:

The Fuel Cell Consortium for Performance and Durability (FC-PAD) coordinates activities that advance performance and durability of polymer electrolyte membrane fuel cells. FC-PAD efforts include six complementary thrust areas, all of which contribute to the electrode layer integration studies. Optimizing electrode layers and mitigating transport issues are vital to meeting U.S. Department of Energy targets. This project is identifying state-of-the-art catalysts; optimizing the catalyst layers; developing diagnostics to help resolve problems with high current density and low loading; and mitigating the problems through the use of novel electrode design, components, and diagnostic techniques.

Question 1: Approach to performing the work

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

- The team has correctly approached the thrust-specific objectives to understand and mitigate the issues with electrode layer integration by identifying state-of-the-art (SOA) catalysts, optimizing them in catalyst layers, developing diagnostics to help resolve the high-current-density/low-loading problem, and mitigating the problem through the use of novel electrode design, novel components, and novel diagnostics techniques, all complemented with modeling.
- This is one of the key thrust areas of FC-PAD, focusing on catalyst layers. The approach outlined to address the barrier is clear and excellent. The overall approach to take learnings from rotating disk electrode (RDE) study to membrane electrode assembly (MEA)-catalyst layer optimization of SOA catalysts with the help of modeling is very good idea.
- FC-PAD is a strong effort coordinating/focusing research to address the technical barriers to fuel cell development. The focus on electrode layers and integration may address issues with durability. Through FC-PAD, this focus area should be integrated with electrocatalysts and catalyst supports.
- The proposed approach in the thrust area is appropriate because it engages numerous partners and models novel electrode designs and diagnostics.
- The focus on the cathode catalyst layer is definitely warranted since catalyst activity targets are being met but high-power-density targets are not being met. The approach, as outlined on slide 7, is good; however, it does not appear that this approach is really being executed. In particular, #1, #2, and #4 on slide 7 are being pursued but not so much #3. In particular, it appears that the team has already decided that the high-current-transport issue is due to the ionomer film. There is no clear evidence presented supporting this hypothesis. The first priority should be to investigate the different hypotheses that have been proposed. The team should first focus on #3 before moving into #4.
- From an overview, the approach sounds logical, but there appear to be many details missing. Yes, the overall power density needs to be met, and there are losses at lower loading that do not appear at higher loading. The project intends to address these by developing new structures, which is good. But what would be interesting to know is how the project will approach developing new structures, especially during ink

processing. The project should define particular ink properties that are of interest and techniques that will be developed to look at how particles aggregate in ink and how this might affect the resulting structure. There is a deeper level of detail that would enhance what has been identified as the approach. The use of two phases for proton transport is interesting and was well-explored in a prior project (FC-125). However, there is a question here as to whether this will become a dominant theme in this project or whether there are other ideas that might provide a more facile change with existing ink processing and application techniques.

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 prepared and evaluated all three SOA catalyst layers. All three have met the DOE mass activity oxygen reduction reaction (ORR) target of 440 mA/mg-Pt. However, the rated power target was not met. Progress has been made on understanding transport through the layer using diagnostic tools and modeling. The team has also demonstrated that the ORR kinetics can be separated from the ionomer effect and true ORR can be measured. The comparison of SOA catalyst performance of three commercial catalysts in slide 17 and other analytical results is a great way to help catalyst developers to improve performance of their catalysts.
- Much progress has been made in the previous similar projects from industry; the national laboratories, academic partners, and principal investigator acknowledge that. Many of the SAO catalysts have been identified, and some very good progress has been made in fundamental studies regarding hydrogen contaminant detector (HCD) diagnostics, oxide dependency.
- Interesting work is presented, and progress has been made on understanding transport through the layer using diagnostic tools and modeling.
- Most of the work shown, particularly that with in situ cell testing, overlaps with what stack developers will do. The inability of high-mass-activity catalysts to achieve high-current-density targets is well understood. Stack developers frequently analyze catalyst layers using limiting current and proton pump techniques. More would need to be known about the nanofibers and the balance of the catalyst layer used for the nanofiber experiments to extract information that could be generalized for all catalyst layers and the fundamental limitations of performance. The microstructural modeling, in conjunction with x-ray tomography, might be a step ahead of developers, particularly in the modeling of liquid water movement in a catalyst layer. Dry imaging and mapping of catalyst layers is slowly becoming familiar, but a good model that can predict movement of condensed water would constitute significant assistance for developers. No accomplishments were shown with respect to how catalyst layers get made and how processing affects structure.
- The FC-PAD Electrode Layer Integration thrust has already shown baseline data that address electrode layer design. The crossover between work from other thrusts (e.g., electrocatalysts and ionomer/gas diffusion layer) may not be completely defined.
- There is too much focus on possible mitigation strategies before establishing the actual limiting mechanisms.

Question 3: Collaboration and coordination with other institutions

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

- Since the Consortium brings in all the important research organizations involved in fuel cell research and development, there will be ample opportunities for Consortium members to conduct collaborative work and technology/knowledge transfer for mutual benefit and to advance fuel cell technology. The list of institutions involved in the Consortium is impressive, and given the fact they are all connected to one another, it is expected to foster many new collaborative activities that otherwise would not be possible.
- This is one of the important thrust areas in FC-PAD, which addresses the key barriers for catalyst layer integration and optimization; close collaboration with other FC-PAD members and industry stake holders is very necessary. The project approach outlines and also exhibits this close collaboration.

- The collaborations are very good and should continue.
- FC-PAD is a vehicle for collaboration.
- The team has done a good job interacting with industry suppliers and has obtained a good variety of materials. However, it is not clear how much the team is interacting with the rest of the FC-PAD team. For example, Lawrence Berkeley National Laboratory (LBNL) shows that MEA ink solvent has a major impact on ionomer and catalyst distribution; however, the National Renewable Energy Laboratory is not studying how this affects the performance of the catalyst layer. Conversely, LBNL should focus on ionomer interfaces.
- Collaboration appears to be a weak point for this aspect of FC-PAD: four catalyst suppliers and General Motors are noted for supplying materials, but it is not clear that the collaboration is deeper than that. While there is collaboration with the other FC-PAD laboratories, collaboration with the other FC-PAD laboratories is a baseline for the project. The project needs to collaborate better with outside developers to understand what needs to be delivered.

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 creation of the FC-PAD Consortium is a great initiative by the Fuel Cell Technologies Office, with a goal to provide technical expertise and harmonize activities of national laboratories with industrial developers. This initiative is relevant to the objectives of DOE’s Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The activities are aligned to DOE’s goal to address the commercial barriers such as performance, cost, and durability. This project is on the thrust area “Electrode Layer Integration,” whose primary objective is to integrate SOA electrocatalysts that meet or exceed the DOE mass activity targets of 440 mA/mg Pt and optimize the catalyst layer to attain the DOE peak power density requirements of $1 W/cm^2$ and 0.125 g-Pt/kW while simultaneously meeting durability targets. The objective is aligned with the MYRDDP.
- While FC-PAD suffers overall from a need to identify customers and deliverables, this particular thrust addresses an area that is highly relevant to lowering cost: enhancing performance at high current density with improved catalyst layers. The relevance slide speaks to activities that have a chance to go beyond what stack developers already do. There is mention of developing new diagnostics and capabilities that perhaps developers have not yet done.
- Potential impact from realizing the activities and durability of SOA catalysts in MEA by integrating them in MEA and optimizing the catalyst layer is very high. The project is clearly focused on key challenges for catalyst layers.
- The project is targeting the main challenges in fuel cell development: improving performance and durability. Diagnostic tools and modeling can be useful to understand transport through the layer and have an impact on designing or optimizing catalyst layers.
- If successful, this project will make important strides to addressing issues of cost and durability for fuel cell implementation.
- The team is definitely focused on a key barrier and has the potential to make significant impact, but the approach could be improved.

Question 5: Proposed future work

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

- The proposed future research aligns with the overall objectives of the Consortium. The MEA screening of remaining SOA catalyst materials is logical. Optimization of catalyst layers to achieve peak beginning-of-life performance for promising candidates is necessary for identifying true high-performing SOA catalysts.
- Key future work already included—catalyst layer (CL) optimization with SOA catalyst to meet performance, alternative CL designs, relevance of kinetics to HCD, and alternative ionomers—is very important.

- Model performance diagnostics data at high current densities and identifying and implementing alternative ionomers in catalyst layers and examining effects on performance will improve understanding.
- The project should define how the team will "optimize catalyst layer" without first obtaining rigorous fundamental understanding of what mechanisms are limiting the performance. The team should consider doing some systematic fundamental studies to prove or disprove possible hypotheses for the HCD limitation. For example, to probe the impact of ionomer or ionomer film, the team could make a series of MEAs with identical parameters except the equivalent weight of the ionomer in the CL (or, alternatively, different solvents in the MEA ink) and see if these parameters have a dramatic impact, as one might expect if the ionomer is the key. This should, of course, be compared with modeling results.
- The project team needs to define how the catalyst layers will be optimized in fiscal year 2016, the design principles, how layers will be optimized if processing/structure relationships are unknown, and tasks associated with understanding how ink properties and processing parameters affect the resulting catalyst layer structure. In principle, identifying alternative designs for catalyst layers is a good idea, but there needs to be understanding of how to achieve those designs. Future work should include examination of inks and processing parameters.
- The project has planned its future in a logical manner. The decision points between other focus areas as well as evaluating collaborator performance needs to be better defined.

Project strengths:

- The overall proposed research for the Consortium and for individual thrust areas is well conceived, thorough, and aligned with DOE goals. The proposed research covers individual goals and necessary activities required to address the challenges related to electrode layer integration of catalysts available from different commercial and developmental catalyst sources.
- Project strengths include close collaboration with remaining FC-PAD members and industry partners; clear understanding of the current status of the technology, not reinventing the wheel; and consideration of most of the key parameters needed to improve the CL to meet the targets.
- Project strengths include the strong team and well-thought-out work plan and its approach.
- The team has the capability and materials required to make SOA MEAs.
- Collaboration is a project strength.
- The project understands that high-current-density performance is lacking with highly active catalysts. The project's access to equipment and facilities within the national laboratories is excellent. Another strength is the use of microstructural characterization to begin to understand—at least on a dry, ex situ basis—how a catalyst layer is structured.

Project weaknesses:

- To this point, no weaknesses were specifically identified for this project.
- A concern is that while the project recognizes the overall problem, there may be too much emphasis on in situ analysis and fairly novel ways of approaching the problem, especially when so much about conventional catalyst layers still remains unknown—other than what can be measured in situ. The project needs to think more broadly about what developers do not have access to, or what still remains a mystery about conventional catalyst layers.
- Although engaging different commercial entities in the Consortium and getting their SOA catalysts for evaluation is a very ambitious initiative, it will be very difficult to manage such activities unless a robust intellectual property/non-disclosure/confidential disclosure agreement is in place. The team should have clear understanding of the intellectual property ownership and legal pitfalls that often come with such a broad coalition of catalyst and MEA manufacturing companies.
- There is no evidence presented that the team can actually make SOA MEAs. There is an overemphasis on mitigation strategies instead of first obtaining a fundamental understanding of the root cause for what is limiting the performance at high current densities. The project is presenting unvalidated modeling results (slide 23) as a motivation for a certain approach.

Recommendations for additions/deletions to project scope:

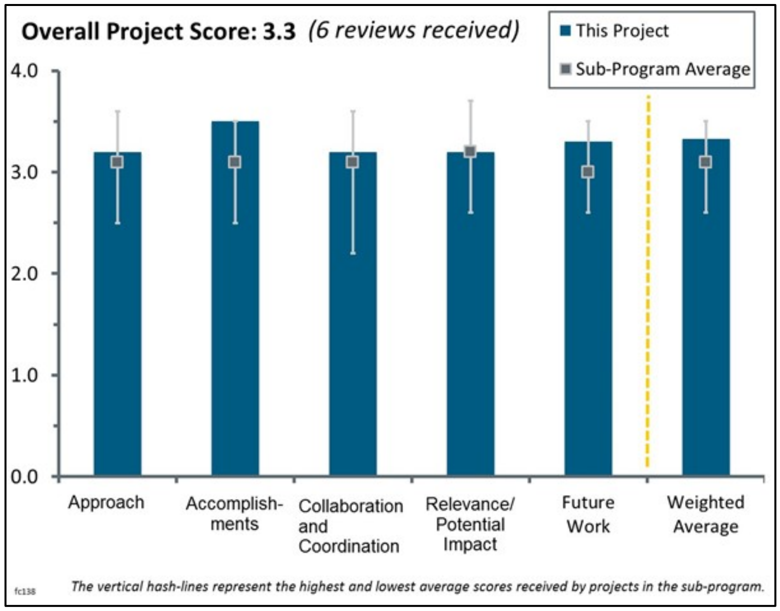
- In general, the project needs to shift from a heavy focus on evaluation toward a better understanding of relationships between processing and structure, structure and layer properties, and layer properties and performance. There is still much that is unknown about what happens in the ink, what happens as ink is applied and dried, and what happens to generate the resulting catalyst layer structure. The increased use of microstructural characterization and modeling to understand the movement and phase changes associated with condensed water in a catalyst layer would benefit the project.
- It would be interesting to see whether kinetics actually come into play at high current densities; the project should examine effects on performance and relationship to durability studies/accelerated stress tests on catalysts (FC-136) coupled to the catalyst/ionomers in electrode layers.
- There should be more focus on fundamentals, which should be used to demonstrate SOA performance and potential mitigations or improvements.

Project #FC-138: Fuel Cell Consortium for Performance and Durability – Ionomers, Gas Diffusion Layers, Interfaces

Adam Weber; Lawrence Berkeley National Laboratory

Brief Summary of Project:

The Fuel Cell Consortium for Performance and Durability (FC-PAD) coordinates activities that advance performance and durability of polymer electrolyte membrane fuel cells (PEMFCs). FC-PAD efforts include six complementary thrust areas, three of which are involved in this project, which entails modeling, evaluation, and characterization of fuel cell components for performance and durability improvements. The components include catalysts, electrodes, and ionomers/gas diffusion layers (GDLs).



Question 1: Approach to performing the work

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

- The project is using operando, modeling, and ex situ diagnostics to elucidate governing behavior and optimize performance and durability in ionomer membranes, ionomer thin films, GDLs, and respective interfaces. This is the correct approach to understand how these components communicate and, hence, perform at their best. Identification of any obstacle to these communications (interactions) is bound to have an impact on performance/durability. The team has taken the correct approach of identifying and mitigating issues involved in individual components and their interfacial junctions.
- FC-PAD is a strong effort to coordinate/bring research focus to address the technical barriers to fuel cell development. This is the third thrust area, which focuses on ionomers, GDLs, and interfaces. This thrust area can have direct impact on both performance and durability barriers to fuel cell development. As part of the Consortium, this project integrates well with other efforts.
- This approach is focusing on several sub-component systems, such as catalysts, electrodes, and ionomers/GDLs, using operando, modeling, and ex situ diagnostics to elucidate governing behavior and optimize performance and durability. The approach is critical and important.
- This is one of the key thrust areas in FC-PAD’s focus on addressing challenges that are overlooked or that have not been prioritized in the past.
- The approach is excellent, with focus on multiple key components and an emphasis on many different types of studies and tools. However, one aspect that appears to be missing is a greater focus on ionomers under the “Interface” category, as these interfaces appear to be hypothesized by some (including other FC-PAD members, such as the National Renewable Energy Laboratory [NREL]) to be important contributors to the transport losses at high current densities with ultra-low catalyst loadings.
- The approach described in the presentation is very general—optimizing performance and durability can lead down many different pathways. What the approach lacks is discernment about which tasks developers need and which tasks have already been covered by fuel cell stack developers. The approach also lacks a defined purpose. For example, all of this is unclear: why transport properties for ionomer thin films are being measured, whether they will be used in a model, whether a developer will be using them in a model, or whether they will be validated in some sense to be relevant for fuel cell operating conditions.

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.

- Significant progress has been made considering the short time span. Though most of the work comes from previous or ongoing projects, the understanding provided for membrane interfacial resistances, water behavior, cerium migration, and ionomer thin films is commendable.
- The overall progress and accomplishments are good. Good progress has been made toward model transport and of microporous layer–catalyst layer and GDL–channel interfaces.
- This FC-PAD thrust area has already shown very good progress with collaborators. This work should be enhanced when the next round of funded collaborators are announced.
- There have been impressive results in a short amount of time.
- The ultra-small-angle x-ray scattering technique demonstrates a good method for characterizing the size of ionomer particles in inks. This may be of assistance to developers in understanding processing–structure relationships. The elemental mapping of a catalyst layer is good, but it is unclear whether this was developed in this project. The weak phase separation of ionomer thin films at very low thickness is useful to understand. It would be useful to developers to have confirmation that this weak phase separation results in thin film properties that affect performance. State-of-the-art (SOA) membranes are fairly complex; therefore, an analysis of changes in mechanical properties of Nafion® with aging may or may not be relevant. Trends with water uptake and alpha relaxations with aging are qualitatively understood by developers. Cerium migration from the membrane into catalyst layers has already been observed by stack developers. For a thorough study, cerium content in effluent water should be checked. The membrane interfacial resistance study could be very useful if combined with a model that would cover one of the more difficult things about fuel cells: predicting hydration throughout a unit cell. Such a goal would be worthwhile, but the objectives of the project do not clearly state this. Nevertheless, there is the beginning of something useful here. Some questions should be asked as to whether the use of a humidity sensor is the best way to go about some of the resistance measurements here, or whether a segmented high-frequency resistance (HFR) technique might be good for validation.
- The experimental evidence of solvent effect and processing conditions for different ionomers is interesting. The separation of different particle sizes with increasing temperature for water dispersion of Nafion® (slide 6) is an interesting phenomenon. It will be interesting to see whether Nafion® dispersion behaves in the same way in alcohol and water or whether the presence of alcohol helps molecules of different sizes to come closer to make larger agglomerates. The d-spacing results for 3M and Nafion® ionomers in slide 8 seems to suggest some effect of side chain size, especially for the bulk membrane d-spacing. It will be nice to see the effect in Solvay's Aquivion® membrane, in which the side chain is much smaller. The aged membrane effect is understandable; however, it is not clear whether this is a true representation of the aging process that the membrane suffers under fuel cell operational conditions. It is very unlikely that the membrane can undergo sulfonic acid dimerization under fuel cell operational conditions unless the membrane is very thick and the cell is running very dry. For a thin membrane (Gore® type), it is very unlikely that the membrane will undergo conductivity loss due to sulfonic site loss or dimerization mediated loss. The cerium washout depends on how the cerium has been imbibed into the membrane. From the slides, it is not clear how the cerium was imbibed into the membrane.

Question 3: Collaboration and coordination with other institutions

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

- Because the Consortium brings in all the important research organizations that are involved in fuel cell research and development, there will be ample opportunities for the Consortium members to conduct collaborative work and technology/knowledge transfer for the mutual benefit of the member organizations and, hence, toward the advancement of fuel cell technology. The list of institutions involved in the Consortium is impressive, and given the fact they are all connected to one another, the project is expected to foster many new collaborative activities that otherwise would not have been possible.

- Collaboration with other FC-PAD members is excellent, which is undoubtedly due to the high degree of interactions between the different national laboratories prior to FC-PAD.
- There is good engagement with current partners, including collaborations with universities and industrial partners.
- The presentation does not provide credit on a task-by-task basis to collaborators, so it is very difficult to judge the quality of collaboration. The GDL images appear to be similar to what investigators from Tufts would produce, but no attribution is given. 3M collaboration appears to be just ionomer material inputs. This project sorely needs collaboration with a stack developer to understand what developers have already done and what has not yet been done. There is much good the project could do if it is focused properly.
- Based on the progress shown, it is difficult to know the extent of collaboration with other FC-PAD members and other partners. Modeling is Lawrence Berkeley National Laboratory's (LBNL's) core competency, so it looks like most of the work is done at LBNL.
- FC-PAD is a vehicle for 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 **3.2** for its relevance/potential impact.

- FC-PAD is a great Fuel Cell Technologies Office initiative, with a goal to provide technical expertise and harmonize activities of national laboratories with industrial developers. This initiative is relevant to the objectives of the Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The activities are aligned with DOE's goal to address the commercial barriers, such as performance, cost, and durability. This project is in the thrust area "Ionomers, GDLs, Interfaces," whose objective is to focus on fuel cell components, their diagnostics, structural characterization, and modeling for both performance and durability improvements. The objective is aligned with the MYRDDP.
- The overall focus of this project—fuel cell components, their diagnostics, structural characterization, and modeling for both performance and durability improvements—is relevant and a vital key to reach the DOE 2020 targets.
- The impact of these studies will be significant, as this thrust area addresses some important challenges that were not prioritized in the past.
- If successful, this project will make important strides toward addressing issues of cost and durability for fuel cell implementation.
- It is not obvious how this work is focused on addressing the two major technical targets that are not being met: (1) balance-of-plant (BOP) performance with SOA catalyst loadings at high current densities and (2) durability of high-activity alloy catalysts. The principal investigator (PI) could do a better job of communicating how this thrust area is contributing to solving these key issues. Currently, it appears that the work just continues to focus on topics of interest to the PI (ionomer thin film properties and GDLs), but it is not evident how the work may result in significant improvements in the two issues noted above.
- The presentation did not discuss relevance. Furthermore, the presentation did not describe clear goals and objectives. There is some acknowledgement that understanding membrane additive migration and voltage losses associated with thin film ionomers would be good, but outside of this, the objectives of the project are not clearly stated. The FC-PAD effort suffers as a result of a lack of identified customers and deliverables. This particular thrust conforms to this trend.

Question 5: Proposed future work

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

- The proposed future research by the team is aligned with the overall objectives of the Consortium. Investigation of side-chain chemistry and governing structure–property correlations, especially the impact of reinforcement, will help in understanding the impact of side chain chemistry on phase separation and ionomer thin films. The model study to elucidate interactions during solvent evaporation with different solvents will be very helpful in understanding the different stages of phase changes on perfluorosulfonic acid dispersion in transitioning from dispersion state to semi-fluid state to dry polymer state. Understanding of the cell conditioning protocols and their impact on various membrane electrode assembly components will also be helpful in understanding how these components behave during the cell start-up, either at BOP condition or after intermittent shutdown.

- The proposed future work is well aligned with the progress made in the first project period. Developing a thin film structure–property model is of high importance. The initiative to study and develop model interactions and examine scale coupling is relevant.
- Proposed future work is very detailed and essential for the success of this activity.
- FC-PAD allows each thrust to incorporate research findings efficiently. The decision points for including work from collaborators are not completely defined.
- The membrane work is interesting, but it is not likely to have an impact on overcoming the barriers for commercialization of fuel cells. Cerium migration habits are fairly well known. The structure of an SOA perfluorinated sulfonic acid membrane is not something that has a high impact on high current performance or even on durability. The GDL modeling aspect of the project should involve experimental validation, if it is not included already, and it should be done for GDLs with SOA thicknesses. Work related to ink dispersions, ionomer thin films, and membrane interfaces is relevant to overcoming barriers associated with cost, robustness, and lifetime. However, these efforts need to be focused by understanding what specific goals and objectives exist. There also needs to be some understanding of what is most useful in light of the fact that access may be limited with respect to actual manufacturing methods and SOA materials.
- More focus on ionomer interfaces should be added.

Project strengths:

- The project is able to explore some areas that other projects have not, such as membrane interfacial resistance to water transport, ionomer thin film properties, and properties of ink dispersions. These are all areas that are relevant. The project has access to an extraordinary amount of characterization equipment and techniques. Unlike other projects, this project is capable of carrying out a systematic study in which phenomenological boundaries are defined. An example is the investigation of phase separation with ionomer film thickness and equivalent weight.
- The overall proposed research for the Consortium and for individual thrust areas is well-thought-out, thorough, and aligned with DOE goals. The research covers individual goals and necessary activities required to address the challenges related to ionomer, membrane, GDL, and related interfaces used in different commercially available and developmental ionomers, membranes, and GDLs.
- The PI and his team are strengths. A major strength is the challenges this team is addressing; the work will result in very good understanding.
- The focus on fundamentals is a project strength.
- The project features a good team.
- Collaboration is a project strength.

Project weaknesses:

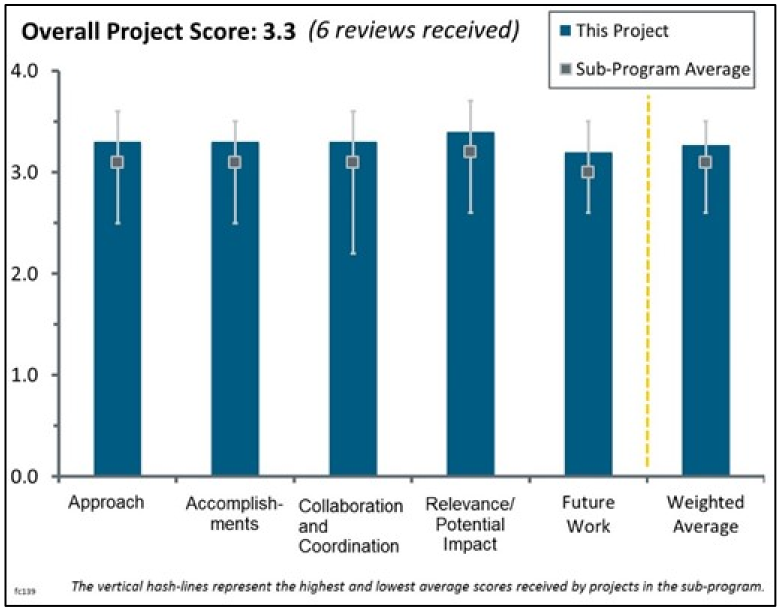
- The project is engaging different commercial entities in the Consortium, but it will be very difficult to manage such activities unless a robust intellectual property/non-disclosure/confidential disclosure agreement is in place. The team should have clear understanding of the intellectual property ownership and legal pitfalls that often come with such a broad coalition of membrane/ionomer/GDL manufacturing and user companies.
- The project has not identified goals and objectives. The project has not interfaced with customers, and it has not defined deliverables. The project does not understand what work it has done overlaps with stack developers and what does not. Collaboration is either fairly light or was not well identified in the presentation.
- To bridge the understanding between interfaces and relations is a challenge. The project would benefit from increased interaction with industrial partners and original equipment manufacturers.

Recommendations for additions/deletions to project scope:

- The project could probably shed the tasks associated with cerium migration, reinforced membrane casting, and GDL modeling. These tasks are less likely to be associated with overcoming barriers to commercialization. The project needs to add considerable levels of collaboration with stack developers in order to identify goals, objectives, and deliverables. The project should maintain emphasis on membrane interfacial resistance and perhaps even consider developing a unit cell hydration model generic enough to assist a wide range of developers, thus allowing developers to figure out how to maintain hydration and stack robustness while limiting the size of the BOP.
- The present investigation of the membrane side chain chemistry and governing structure–property correlations using membrane interfacial resistance measurements is slightly unclear. It is recommended that the project pay careful attention to this.

Project #FC-139: Fuel Cell Consortium for Performance and Durability – Modeling, Evaluation, Characterization

Rangachary Mukundan; Los Alamos National Laboratory



Brief Summary of Project:

The Fuel Cell Consortium for Performance and Durability (FC-PAD) coordinates activities that advance performance and durability of polymer electrolyte membrane fuel cells (PEMFCs). FC-PAD efforts include six complementary thrust areas, three of which comprise this project: modeling and validation, operando evaluation, and component characterization. The project is developing advanced diagnostic, modeling, and characterization techniques to evaluate state-of-the-art (SOA) membrane electrode assemblies (MEAs) and provide insights to improve the durability of the MEA components.

Question 1: Approach to performing the work

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

- The team has correctly approached the operando evaluation and durability to refine accelerated stress tests (ASTs), impurity effects, evaluation of Pt-based MEAs, and development of advanced electrochemical characterization techniques to integrate the analytical results from other thrust groups to conduct modeling on the components.
- FC-PAD is a strong effort to coordinate/bring research focus to address the technical barriers to fuel cell development. The focus of this work covers thrust areas 4 to 6. This effort integrates well with other thrust areas of FC-PAD by providing the performance and durability evaluation of the outputs from thrust areas 1 to 3.
- The approach is strongly focused on meeting U.S. Department of Energy (DOE) targets on development and implementation of characterization techniques and models with the aim to improve performance and durability.
- This thrust area supports the first three areas of FC-PAD with evaluation and characterization. The major efforts are on durability evaluation and developing ASTs to shorten the time.
- There is excellent focus on fundamental understanding in a variety of important topics.
- The development of a combined membrane AST is a direction in which many developers have already proceeded. What is needed instead is a faster mechanical test. A test that isolates mechanical stress is a worst-case scenario for membranes with inadequate swelling properties and, therefore, can be used to define the boundary on how much in-plane swelling is too much. This is needed information for suppliers, and it usually cannot be deconvoluted from a chemical–mechanical combined test. The mechanical test is too long and needs to be shortened. Rather than presuming that the 20× acceleration factor is universal for all materials, it would be interesting to see this approached for an array of Pt particle sizes, ionomer/carbon ratios, carbon types, loadings, and other design parameters. Development of ASTs for PtCo may be of interest, but uncovering failure modes is something that should only be done in conjunction with a stack developer. There are many different types of PtCo, and therefore, PtCo failure modes can vary widely depending on how the cell is operated. The modeling appears to be premised on breaking down mass transport losses into constituents associated with Knudsen diffusion and ionomer thin films, but this kind of task is already being pursued by stack developers. The study of recoverable losses associated with

membrane fragments or SOx was part of a stack developer request. However, the developer was already aware of the association with membrane degradation and with recovery at low potential. The approach to this task should add to what is already known. It is not clear why most of the testing is getting carried out in a serpentine cell or whether a four-channel cell will approximate the same channel flow velocity found in full-size automotive cells or cells for other applications.

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.

- Thorough benchmarking of various ASTs from across the industry is commendable and a great deal of work. Significant progress has been made in terms of AST development and alloy catalyst evaluation and characterization.
- The project made good progress on proposed refined new ASTs, durability of the Pt alloy, and studying and quantifying reversible/recoverable degradation.
- This effort has shown good progress toward evaluating MEAs during cycling. The project has identified degradation mechanisms for evaluation.
- The development of a faster AST for electrocatalyst cycling is good work if the acceleration holds for a variety of cathode catalyst layer parameters. This may assist in shortening development time. The investigators did provide a combined chemical–mechanical test that is shorter than the mechanical test. However, 500 hours is still very long for a combined chemical–mechanical test versus other such tests that exist. The PtCo trends shown with Umicore and IRD catalysts are similar to what has been shown throughout the community. Particularly, it is well known that electrochemical surface area (ECSA) can decrease to a limited extent before performance is affected and that larger particle sizes lend themselves to lower ECSA loss. The presence of Co in the membrane has also been observed. The project needs to go deeper to better understand how industry could do even better to stabilize more active PtCo catalysts. A considerable amount of the modeling context appears to be missing. It is not clear how the model can account for both wet and dry conditions or how well-validated and predictive it is for a wide range of operating conditions. Data from studying recoverable losses appear to mirror what General Motors has already been presenting at the Durability Working Group since December 2014. The project needs to determine how it can proceed deeper to overcome barriers associated with this.
- In terms of percentage of ECSA loss, the old AST seems to be aligned with the Fuel Cell Technical Team drive cycle. The new AST is much more aggressive and does not correlate to the actual fuel cell drive cycle. It is not clear that there is any need for the new AST. It may be faster, but it does not represent the actual degradation pathway that the fuel cell follows during its operation. The study of SOx fragments needs more attention. Typically, SOx fragments are generated when the MEAs are operated under very dry and high-temperature conditions, and the duration of the exposure to such harsh conditions determines the extent of SOx that may form in the MEA. The ex situ result of reduced oxygen reduction reaction (ORR) due to SOx poisoning may look interesting; however, the team members should make sure that in mimicking this condition to develop the AST, they are not overestimating the extent of SOx that may be present in the MEA.

Question 3: Collaboration and coordination with other institutions

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

- Because the Consortium brings in all the important research organizations that are involved in fuel cell research and development, there will be ample opportunities for the Consortium members to conduct collaborative work and technology/knowledge transfer for the mutual benefit of the member organizations and, hence, toward the advancement of fuel cell technology. The list of institutions involved in the Consortium is impressive and, given the fact they are all connected to one another, the project is expected to foster many new collaborative activities that otherwise would not have been possible.

- There is excellent collaboration with both other FC-PAD members and with industry. In addition, international collaboration with the National Physical Laboratory (NPL) appears to be resulting in useful new capability.
- There is strong collaboration among FC-PAD members and external collaborators.
- FC-PAD is a vehicle for collaboration. This effort ties into thrust areas 1 to 3.
- There is clearly a high degree of collaboration.
- Reference electrodes can be very useful in studying fuel cells, but at the moment, it is difficult to see where the reference electrodes from NPL are being implemented for tasks that will help to overcome barriers. Furthermore, the reference electrodes are being implemented in a serpentine cell, which comes with its own questions, especially with regard to how channel flow velocity matches flow velocity in a full-size cell. General Motors, Gore, IRD, and Umicore appear to be materials suppliers. Further depth in these collaborations does appear to be represented in the slides. Collaborations with Ion Power and Tanaka Kikinzoku Kogyo (TKK) 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 **3.4** for its relevance/potential impact.

- FC-PAD is a great Fuel Cell Technologies Office initiative with a goal to provide technical expertise and harmonize activities of national laboratories with industrial developers. This initiative is relevant to the objectives of the Multi-Year Research, Development, and Demonstration Plan. The activities are aligned with DOE’s goal to address commercial barriers such as performance, cost, and durability. This project is on the thrust area “Modeling, Evaluation, Characterization,” whose objective is to develop and implement characterization techniques and models to improve performance and durability of fuel cells.
- Testing and evaluation will provide the final data to determine quantitatively whether thrust areas 1 to 3 have been successful. Findings from this effort can guide future efforts in thrust areas 1 to 3. Thus, this effort will make important strides toward addressing issues of cost and durability for fuel cell implementation.
- Durability continues to be a major barrier, especially with respect to high-activity alloy catalysts at ultra-low catalyst loadings.
- This is an extremely relevant project and is critical to advancing technology toward the DOE 2020 goals.
- Better, faster, and relevant ASTs can shorten the development time. This thrust area is key in developing those.
- The relevance of the project is premised on measuring durability as well as on developing the ASTs by which durability can be estimated on a component basis. Fuel cell system developers are capable of measuring durability, extending durability with system mitigations, and measuring component durability with either their own ASTs or with more widely adopted ASTs. Therefore, a considerable amount of the project exists to serve the public interest in understanding fuel cell durability. This is not crucial to fuel cell system developers, but it is within the scope of what DOE is trying to accomplish. The development of ASTs is, by itself, not necessary for overcoming barriers. Stack developers have been working with their own ASTs for decades. What the project needs to show is how to provide better ASTs that are shorter and still premised on the same failure modes that exist in a realistic drive cycle.

Question 5: Proposed future work

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

- The proposed future research is aligned with the overall objectives of the Consortium. A complete durability evaluation of PtCo-alloy-catalyst-based MEAs and complete development of a reference electrode set-up is the correct course. However, the evaluation of sulfate infusion’s effect as a function of potential and during durability cycling protocol is questionable. Quantifying the effect of reversible degradation under durability cycling protocol is a logical choice.

- Overall, the future work appears excellent. If possible, it would be good to see the team make more specific recommendations/suggestions on how the community might mitigate certain degradation mechanisms and demonstrate some of these mitigations.
- The proposed future work, as outlined on slide 23, is appropriate and will further progress toward DOE goals.
- This effort has an effective plan for evaluating novel MEAs. It is not clear when inputs from other FC-PAD thrust areas would be evaluated. An “as available” approach may not be effective/appropriate for the success of the FC-PAD effort. The interaction between the FC-PAD thrust areas may need better definition.
- The evaluation of PtCo and PtNi durability appears to be a direct overlap with stack developer work, and it is possibly being done with materials that have not been heat- or acid-treated to a state in which they might be considered to be SOA. In the case of PtNi, if it is from the DOE-funded work, there may be some SOA work proceeding, but the overlap with developer work still exists. This seems to be a benchmarking effort. It is not clear what cell will be segmented with reference electrodes to understand durability effects. If it is a quad-serpentine cell, this may have an entirely different hydration profile from a cell for a full-size stack. Yes, the voltage trends may be there, but hydration and temperature may have very different profiles. Adopting a differential cell for single-cell testing would be a great improvement over what has been proceeding. There is no way that stoichiometry sensitivities that exist for a small single cell will be the same for a cell for a full-size stack.

Project strengths:

- The project should continue to revisit, update, and disseminate AST protocols. Developing insights into loss of mass activity without loss in ECSA is a project strength, as is developing insights into increases in transport losses with loss of Co in PtCo alloys. New local reference electrode capability is being developed. The international collaboration with NPL is productive.
- The overall proposed research for the Consortium and for individual thrust areas is well-thought-out, thorough, and aligned with DOE goals. The research covers individual goals and necessary activities required to address the challenges related to modeling, evaluation, and characterization necessary for evaluating different commercially available and developmental fuel cell MEAs.
- The project has access to considerable characterization and test stand resources. The project personnel in the past have been able to systematically benchmark supplier materials with ASTs. The project has done a better job than other parts of FC-PAD in identifying specific goals and objectives.
- Collaboration and team members, especially the principal investigator, are key strengths of this project.
- Project strengths include efficient teamwork and a well-balanced approach.
- Collaboration is a project strength.

Project weaknesses:

- The project is still using quad-serpentine cells premised on reactant gases delivered in stoichiometric ratios (for most tests) instead of using differential cells. The project has limited access to SOA materials. It would help the project to have a broader understanding of how much variation can exist in catalyst layer design factors as well as in catalyst powders (even while keeping the composition essentially the same) and how these variations can affect durability. The durability of PtCo cannot be fully represented with just two flavors of PtCo. There needs to be greater clarity in how reference electrodes are to be implemented. The quality of collaborations with stack developers can be enhanced.
- Although engaging different commercial entities in the consortia and getting their SOA catalysts for evaluation is a very ambitious initiative, it will be very difficult to manage such activities unless a robust intellectual property/non-disclosure/confidential disclosure agreement is in place. The team should have clear understanding of the intellectual property ownership and legal pitfalls that often come with such a broad coalition of catalyst, MEA, and GDL manufacturing companies.

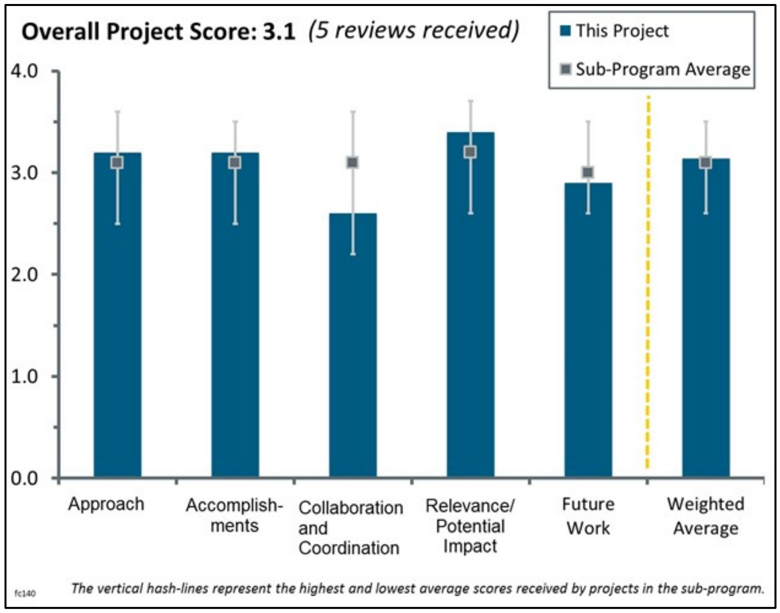
Recommendations for additions/deletions to project scope:

- There needs to be focus on decreasing the time needed for a mechanical stress test for membranes. A chemical–mechanical combined test cannot serve as a replacement. There needs to be better definition for the modeling work or else it should be removed. In general, the project needs to be able to move beyond what is already understood about failure modes such as those associated with PtCo or recoverable losses due to membrane fragments or SOx. So far, the project is confirming what is known, but it needs to move toward exploring mechanisms or toward working with collaborators to help validate solutions to these problems.

- For newer high-ORR facet-/shape-controlled alloy catalysts, ASTs may need further refinement. The correlation factor from AST cycles to stack durability is very important so it can be explored.
- The project should study the relationship between model durability of MEAs—under both AST and durability cycling protocols—and a real fuel cell system.
- There should be even more engagement with industry and universities (new awards from the latest funding opportunity announcement should help in this regard).

Project #FC-140: Tailored High-Performance Low-Platinum-Group-Metal Alloy Cathode Catalysts

Vojislav Stamenkovic; Argonne National Laboratory



Brief Summary of Project:

A primary focus of the U.S. Department of Energy Hydrogen and Fuel Cells Program (the Program) is development of highly efficient and durable Pt alloy catalysts for oxygen reduction reactions (ORRs) with low Pt content. This project will go from fundamentals to real-world materials to achieve rational design and synthesis of advanced materials with a low content of precious metals. Researchers are taking a materials-by-design approach to design, characterize, understand, synthesize/fabricate, test, and develop tailored high-performance low-Pt-alloy nanoscale catalysts.

Question 1: Approach to performing the work

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

- The project has an excellent approach that builds on outstanding fundamentals to inform the scale-up to complex systems. It would be preferable to have industrial partners to help with scale-up, such as making larger batches of catalysts, which is proceeding but could probably be done faster with companies that specialize in this area.
- The project has an excellent balance of fundamental electrocatalyst study, development of high-activity nanocatalysts, directly relevant analytical capabilities, and strong interaction with electrode optimization experts (Debbie Myers, Argonne National Laboratory [ANL]). The approach of incorporating catalyst scale-up is unique among national laboratories and is a substantial strength.
- The project aptly draws fundamental concepts of catalyst activity and durability into the Program through a coordinated program of catalyst preparation, very detailed characterization, and kinetic activity testing. The project needs to evolve into testing in membrane electrode assemblies (MEAs), reducing its reliance on rotating disk electrode (RDE) testing for activity and performance evaluation.
  - ANL has not paid adequate attention to the limits posed by local oxygen transport at high current density on the practical utility of low-loaded catalysts with very high area-specific kinetic ORR activity but low (<30 m^2/g) Pt specific surface areas. Unless new means of avoiding the local transport resistance can be developed, low-surface-area catalysts will not be cost-effective for applications requiring current densities of ~1 A/cm^2 or higher (which is most applications). ANL therefore needs to give increased attention to maintaining high specific surface areas. Caution should be exercised in thinking that the additional thermodynamic stability of ordered intermetallic compounds versus disordered alloys will necessarily give catalysts with superior durability. The experimental experience with such an approach has given at best mixed results.
- For AuX-PtNi, the concept of stabilizing the Au so that the Au can stabilize Pt is interesting. However, there is much in this concept that would have to go right, so the probability of success is very small. First, AuX cannot dissolve (as of now, it is unknown). Second, Ni still cannot dissolve. In many other PtNi species, this has been a problem, and it has been a problem outside of RDE cycling. Third, a particle has to be made that preserves the layering described for the thin film. For Pt_3Co or PtCo, suppliers have covered this type of particle well.

  - The m-SiO_2 on Pt_3Co is a very interesting structure, although it may suffer from Co dissolution. It is not clear how SiO_2 is eventually removed without affecting Pt_3Co.
  - For Pt nanoframes with better segregation of Pt on the surface, this catalyst will have to overcome what was revealed in the last project. It has to be scalable, and it has to perform well in a cell. Given the activity that has been measured, it is worth trying, but because it is a dealloyed PtNi, the probability of Ni leaching during MEA fabrication and the probability of low high-current-density performance are both high. There are no parts of the approach that discuss what will be done to provide for high performance at high current density. The use of vacuum processes does provide an opportunity to divert from processes that impose numerous acid/heat treatment steps or that introduce impurities.
- There is a real emphasis on fundamental understanding and improvement. The criteria for selection of which technologies and paths are followed are not clear.

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.

- Many impressive accomplishments have been made already, including a new experimental technique (RDE + inductively coupled plasma mass spectrometry [ICP/MS]) that is yielding very interesting results and MEA testing results with high performance, which already exceed some DOE targets.
- So far, the major points of progress have been the following: (1) the development of a thin film for AuX-PtNi that has shown stability in glass cell testing, (2) the formation of the three-dimensional novel structure for Pt_3Co using SiO_2, and (3) the enhancement of PtNi nanoframes with Pt segregation. This represents a fair amount of progress for a project that has been active since October 2015. However, none of these developments alone, or even in combination, represents a development that overcomes barriers to commercialization.
  - The scale-up of PtNi nanoparticles is reported to have reached 200 mg. It would be best for the project to move even more aggressively toward batch sizes that are multiple tens of grams. Collaboration with a supplier would be useful. The project needs to move more aggressively toward cell testing, especially for use in durability studies. Too many projects in the Program have been content to report results from RDE only over the first few years of a project.
- One accomplishment is the development of in situ external calibration (EC)-ICP/MS while providing tremendous insight into electrocatalyst durability. Development of a method to stabilize Pt through X-PtNi in a nanoparticle catalyst is very promising. It needs to be demonstrated in an MEA. It appears that a key structural factor leading to variable PtNi nanoframe catalysts has been identified, a critical first step. It is unclear whether the issue has been resolved. There has been excellent progress toward MEA integration of catalysts. MEA hydrogen/air performance is good for the low loading and relatively low electrochemical surface area. MEA mass activity exceeds the DOE target.
- The project's new real-time measurements of Pt dissolution provide critical insights into how to improve durability of Pt-based catalysts against voltage-cycling effects. The correlation of Pt dissolution rates with different surface atomic structures and extents of order should greatly improve the ability to rationally design catalysts with improved durability, and the analysis should be extended to alloy systems.
  - The project has placed the use of subsurface gold in improving the durability of Pt-based catalysts against voltage cycling onto a much firmer basis. The development of "additive X" to prevent Au from segregating to a Pt-based catalyst surface could provide a practical route to more durable catalysts. The identity of X should be communicated to the fuel cell community without undue delay because of patent-filing considerations. Patents filed by national laboratories tend to hinder, rather than promote, incorporation of national laboratory ideas into U.S. industrial development.
  - This project and its predecessors have promised catalyst scale-up at ANL for years without notable results to date. It is time to deliver on this or to give up. Closer ties with industrial firms that manufacture catalysts would likely be a better way to proceed. Modestly larger quantities of catalysts to allow MEA testing to largely replace RDE work are sorely needed, as the relevance of RDE results to real fuel cells is increasingly under question.

- The progress toward the stated goals is good, despite the reviewers' contention that performance must be in relevant systems: MEA, durable supports, and DOE accelerated stress test use.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.6** for its collaboration and coordination.

- The partners included are good, and the collaboration appears to be good as well, based on the results to date. However, it is unfortunate (but understandable) that the team is limited to national laboratories. Hopefully, this will change soon with the addition of industry and universities to the Fuel Cell Technologies Office's (FCTO's) new consortium models.
- Collaboration with both national laboratories and universities is evident. Inclusion of industrial input is needed.
- Despite the mention of non-disclosure agreements signed with automakers, the majority of the catalyst synthesis and design work appears to be getting done at ANL. The slides speak to Lawrence Berkeley National Laboratory's role in scaling up catalysts, but the actual conceptualization appears to reside at ANL. Greater collaboration in conceiving new catalysts could help the project. Oak Ridge National Laboratory and Los Alamos National Laboratory collaborate, but their roles seem to be to carry out particular tasks for which they are well-suited: microstructural characterization and fuel cell testing, respectively. It does not appear that these partners play a role in strategizing the project approach. Interactions with automakers are represented in somewhat cryptic fashion in the slides. Perhaps even more helpful than automaker collaboration would be collaboration with an industrial catalyst supplier—or even a small company that can carry out vacuum deposition at higher scale—but that appears to be lacking.
- The project needs to improve its ability to get catalyst samples to other laboratories for testing. Selected catalyst types arising from this project need to be synthesized and tested in other laboratories to give full credibility to the results generated solely within ANL. The project should avoid excessive delays in technology transfer.

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 has adopted the FCTO Multi-Year Research, Development, and Demonstration Plan catalyst targets as its targets. The g/kW target implies that high-current-density performance has to be acceptable, while mass activity speaks to low-current-density performance. The project is also committed to meeting loading and durability targets. The project relevance is predicated on the relevance of Pt alloys. The one criticism that could be levied is that the Program has not been a stranger to Pt alloys for oxygen reduction. Dealloyed PtNi has been studied in morphologies such as nanoparticles, nanoframes, and nanowires and in nanostructured thin film catalysts. It can perhaps be said that DOE has relied far too heavily on Pt/base metal alloys, to the detriment of other possible materials. This project must show that there is new ground to be broken in the Pt alloy field.
- To meet the ultimate DOE targets, higher-activity catalysts with improved stability are certainly required; however, another major barrier has been the incorporation of these new catalysts (e.g., thin films) into effective high-performance MEAs. Fortunately, the project does include MEA work. However, it is not clear what the team is doing to understand the performance losses in the MEAs (e.g., transport and ohmic) to accelerate the scale-up to this key component.
- This project and its predecessors have been one of the primary sources of ideas for innovation in ORR catalysts for the entire industry. There is more to catalyst performance than kinetic ORR activity—this project needs to start paying attention to the needs for adequate active surface area to avoid excessive losses at high current density due to the apparent local oxygen transport problem. Very high area-specific activities can increase fuel cell efficiencies at low current density, but behavior at high current density is what currently defines the cost of a stack for a wide range of applications.

- The need for better catalysts is the focus of this work; more emphasis on full electrode impact is needed.

Question 5: Proposed future work

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

- The proposed work is excellent; the only recommendations would be to incorporate the following: (1) some scale-up with catalyst suppliers (e.g., industry) and (2) more MEA-level diagnostics to determine what is limiting the performance (this may also require additional collaborators).
- The presentation gave reasonable but overly general suggestions for future work.
- It seems early for this consideration because the continuous process at this stage is so different from what would be used for ton levels. For example, 10 g/car 5 kg per batch per day is 500 cars/day and ~200,000 cars per year. Hence, continuous operation catalyst synthesis is not necessary. The topics highlighted are good, but details are lacking, so commenting is very difficult.
- Most of the material in the presentation pertaining to future work seems to describe how all tasks will continue forward in parallel. New catalyst concepts, new synthesis, characterization, cell testing, and scale-up of synthesis—all of this will just keep going. There is no indication of how go/no-go decisions will be made, or which catalysts hold greater priority toward scale-up and cell testing. The project needs to have some strategy to know which experiments need to proceed first, as opposed to just presuming it is acceptable to proceed in the usual fashion from thin film to small powder batches to glass cell tests to characterization, and so on. Instead, questions should be asked early on. These questions should consider what is most likely to cause failure for AuX-PtNi, the novel structure of Pt_3Co, and what would prevent commercial adoption (e.g., inability to produce at scale). Once these questions are answered, the project should move aggressively and quickly to pursue the most challenging tests rather than wait until the final year of the project.
- The proposed "scaling up" work is not clear.

Project strengths:

- The alloys from which the project is building have been found in the past to have high oxygen reduction activity. The project has access to the very best materials characterization techniques that can be found in the national laboratory system. The project does have the ability to conceptualize novel structures such as the one found for Pt_3Co. The project personnel are some of the best electrochemists in world, especially in fuel cell science. The quality of the measurements is very high.
- The project's greatest strength lies in its nearly unique ability to correlate surface structure and surface and near-surface composition (all at the atomic level) with ORR activity and durability. Real-time measurement of Pt dissolution is a very powerful new tool that increases still further the value of atomic-scale control of catalyst structure.
- The project strengths are the analysis of technical-like PtNi/C alloy annealing and performance, development of in situ EC-ICP/MS, balancing nano-/meso-scale characterization (ACTEM, XAS, and XRD and some attempt at reproducibility.
- The project strengths are the strong fundamentals with respect to catalyst activity and composition, development of new techniques, and MEA testing.

Project weaknesses:

- Catalyst scale-up is included but appears to be slower than might be the case with more experienced collaborators. Fundamentals with respect to MEA diagnostics are a project weakness.
- ANL needs to realize that high kinetic ORR activity and durability of that kinetic activity are not enough to ensure that a catalyst will have practical utility. One must advance to MEA testing and also probe the catalyst's utility in MEAs optimized for high-current-density performance in air. ANL needs to increase the strength of its collaborations with other organizations to rise above its current focus on kinetics alone.
- The project has not collaborated with industrial catalyst suppliers, and collaboration outside the national laboratory network is still pending. This is particularly a problem for scaling up catalysts. While alloys have high activity, many of the catalysts being worked on in the project may not be stable in either MEA

preparation or in cell testing. Past projects from the investigators have revealed a tendency to encounter the most challenging aspects of the work in the final year. The project needs to move quickly toward the experiments that might be most revealing or disappointing.

- There is insufficient utilization of in situ EC-ICP/MS for verification. It is not clear what leading technical catalysts that are demonstrated to work in cars do. It is not clear how to improve those using this system. Other project weaknesses are the inclusion of Fe in multi-metallic nanoframe development despite its incomparability with membranes; emphasis on RDE for performance testing; proposed continuous scale-up, which does not make sense; and lack of comparison of performance to leading commercial catalysts (e.g., to PtNi/C, Pt/silica structure).

Recommendations for additions/deletions to project scope:

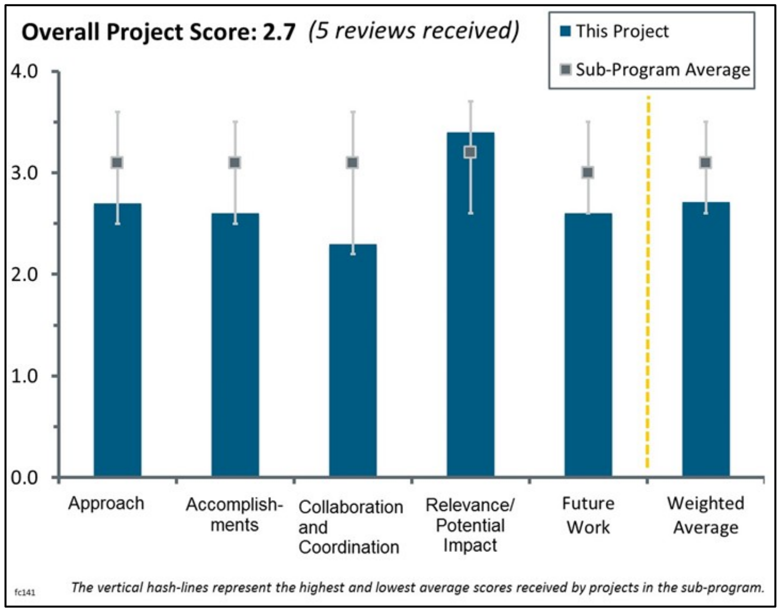
- The project should put a greater emphasis on better understanding and stabilizing a single-catalyst-system approach, rather than pursuing multiple formulations, form factors, and synthesis approaches. Greater emphasis on full electrode testing to confirm stability and durability is also needed. In situ EC-ICP/MS for verification is also recommended. It is unclear what the leading technical catalysts that are demonstrated to work in cars do. It is unclear how to improve those using this system. The project should eliminate Pt/Si whiskers. The very nature of extended whiskers like these is thermodynamically unstable, and they will likely sinter under heating. In addition, integration into an electrode is unclear. It is not clear how one collects current from these efficiently and facilitates ORR. It is not clear how Si is removed. It is not clear how the surface is cleaned off. It is not clear whether cleaning requires heating. If so, it is not clear whether extended structures collapse.
- Collaboration with an industrial catalyst supplier should be added. Faster progress toward scale-up of catalysts, toward cell testing, and toward durability testing in a cell needs to be pursued. Fabrication of PtNi or PtCo nanoparticles that do not appear to advance beyond what has already been done should be eliminated where necessary.
- The project should add major collaborations with organizations experienced in catalyst scale-up and in fabrication of MEAs optimized for performance at all current densities.

Project #FC-141: Platinum Monolayer Electrocatalysts

Radoslav Adzic; Brookhaven National Laboratory

Brief Summary of Project:

This project aims to synthesize high-performance platinum monolayer (ML) electrocatalysts for the oxygen reduction reaction consisting of a platinum ML shell on stable, inexpensive metal, alloy, metal oxide, nitride, or carbide nanoparticle cores. Three low-platinum catalysts will be developed that will meet the U.S. Department of Energy (DOE) technical targets for 2020.



Question 1: Approach to performing the work

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

- The project approach toward development of high-specific-activity catalysts with high Pt utilization, based on Pt monolayer catalysts, is sound. Both activity and durability in rotating disk electrode (RDE) work appear to be routinely assessed. Too little effort is put toward addressing two key issues: use of platinum group metal (PGM) in the cores and high-current-density performance in membrane electrode assemblies (MEAs). Evaluation of durability in MEAs is much more aggressive and is necessary to truly assess the catalyst durability. No such data were presented this year.
- Monolayer catalysts, which can provide good PGM-mass activity along with the high-PGM-mass-specific surface area that is needed to mitigate high-current-density losses due to local oxygen transport effects, are currently the most promising direction for decreasing PGM loadings to DOE targets while maintaining fuel cell performance over the whole operating range. Non-precious cores could provide the above benefits with a wide range of core sizes if durability could be achieved. Precious-metal cores could provide benefits vs. Pt or Pt-alloy catalysts only if the core–shell particle size were kept below the 4–5 nm of reasonably stable Pt-alloy catalysts. Too much of the work still involves cores containing excessive amounts of precious metals. More of the effort should be focused on non-precious cores such as the promising work with NbN. The project should reduce its reliance on RDE testing, which does not always predict behavior in MEAs for novel catalyst systems.
- The non-precious-metal-based cores using niobium–nitride and tungsten–nickel show promise of high activity and stability. The project needs to find collaborators who can successfully translate these improved activities into MEA-level demonstrated activity and high-current-density performance.
- The approach in this project continues to address the needs for reduced-cost and increased-performance catalysts. Questions remain on the utility of replicating an existing approach on various substrates, such as whether there is a rationale to trying Nb, Mo, and Y. The rationale behind the support variance is also unclear; the support variance is a solid and needed addition to the project, but the choice of carbon nanotubes is questionable. Further, the three-dimensional structured supports mentioned are not defined in any way. Lastly, graphene cannot survive carbon corrosion and hence should not be used. The project has some integration with other projects such as developing characterization techniques but does not appear well reintegrated with either MEA project.
- PtY is thought to dissolve, based on prior work. This project should move aggressively to test PtY in a fuel cell.
- In general, Brookhaven National Laboratory (BNL) needs to move much faster toward durability experiments. The project's usual operating mode constitutes surveying a wide range of catalyst samples

before proceeding with cell testing or durability studies (other than those studies pursued in a glass cell, which have little relevance to what happens in a fuel cell). Instead, once a new catalyst is found to be active in a glass cell experiment, the project should move immediately to have inks made (perhaps through collaboration with a supplier or national laboratory), followed by MEA preparation and cell testing. Some evidence of accelerating to cell testing has been shown for nitrided PdNi cores and PdMo cores, but this needs to happen for each catalyst. The use of Au cores (such as with the Ti-decorated Au cores or the AuNi alloy cores) is not likely to lead to a cost benefit versus the use of a PGM core. The nitrided PdNi core and the Pd_3Mo core may also not provide for a cost benefit. There are only two classes of catalyst particles in this project that do not make use of a precious metal in the particle core: Pt monolayers on NbN and Pt/Pd monolayers (and some variations thereof) on a Nb core. Other than niobium or niobium nitride cores, most material approaches risk high cost and low durability.

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 preliminary work with non-precious NbN cores is very encouraging and should be intensively pursued. The work with small amounts of Pd over Ni-Nb cores may prove useful if the amount of Pd can be held down and if these systems give adequate durability (always a worry when Pd is used). Mo-modified cores rely on strong bonding between Pt and Mo for stability as Mo oxides can be soluble in acid. If these systems give adequate durability, work should proceed to replace most of the Pd in the core with non-precious metals. The $Pt/TiO_2/Au$ system is scientifically fascinating but contains too much Au for practical application unless the total particle size is kept below 4–5 nm. The Pt/AuNi alloy system is also of interest if the particle diameter can be kept down to 4–5 nm. If one could replace Pt in the core of a small particle with Au and maintain stability, one would choose to do so because Au is much more widely available than Pt. The non-aqueous deposition of PtY is a step forward that needs to be followed up with tests of the durability of this system, which would seem to be dubious because of the high reactivity and solubility of Y.
- Mass activity (A/mg of Pt) is exemplary, exceeding the DOE 2020 target by several fold. It is unclear whether progress has been made toward improved mass activity, reduction in core PGM content, or MEA performance since last year (FC-009).
- There are a few catalysts with which mass activity needs to be normalized by total mass of precious metals. Examples in the presentation include PtPdMo, AuPtCo, and PtAuNi. Pt monolayers on PdNi reach about 480 mW/cm^2 at 0.6 V and 22 psig (a realistic high-end operation air inlet pressure) with 0.2 mg/cm^2 precious metals. This is only about 0.42 g/kW (cathode metals only) in spite of a PGM-based mass activity of 600 A/g from glass cell testing. Durability data are needed. Pt/NbN/C holds promise as a catalyst from this project that may decrease precious metal loading and be durable. However, its mass activity from RDE work is 350 A/g, which is decent but could be improved. Similarly, Pt/Pd/Nb is 380 A/g PGM at best on RDE, which also could be improved. Pt/PdMo/C shows approximately 460 mW/cm^2 at 0.6 V and lower pressure for only 0.098 mg/cm^2 precious metals. This appears better in comparison to Pt/PdNi but is still far short of the high-current-density objectives. Again, durability data is needed. The open circuit voltage is very low. Mass activity is only 340 A/g precious metals for Pt/Ti-decorated Au/C.
- Using gold and other PGM cores reduces the overall mass activity on a PGM basis; the state-of-the-art Pt-Co alloy catalysts are achieving over 600 mA/g Pt. Therefore, this project should focus on non-PGM cores and de-emphasize work on using PGM-based cores.
- Despite repeated requests, and published papers indicating instability, stability tests in MEAs of these types of core–shell catalysts are not presented. Without these, it is simply impossible to evaluate the potential of the technology. There is also a significant amount of incomplete analysis in the slides. For example, slide 6 is unclear on how the sample is ball milled. One should not be able to use ball milling to reduce sizes to 50 nm. Either the sample is degrading or the analysis in incorrect. On slide 8, it is not clear why the peak is shifted relative to Pd_3Mo, whether this is segregation of Pd and PtMo or why the Pd_3Mo is asymmetric—perhaps this represents multiple phases. On slide 10, there are striations on one of the particles and none on the other, indicating bulk alloying rather than surface alloying—and negating the core–shell structure. On slide 12 (A), it is unclear why the shapes of the particle in the high-angle annular dark-field imaging are different from the energy dispersive spectroscopy (EDS) maps.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.3** for its collaboration and coordination.

- Good collaborations are in place, but the output/outcome is unclear. This principal investigator has excellent strength in developing new catalysts and new ideas and testing them at RDE level. However, the collaborators do not seem to be engaged in moving this technology forward and scaling up. While some of this technology has been licensed, the licensed catalysts are still very similar to the PtCo alloy catalyst. More work is needed to optimize these at Technology Readiness Level 4.
- The project itself involves little collaboration, but patents from earlier in the project have been licensed and brought into production by an experienced catalyst manufacturer. The project would benefit from more formal collaboration within the project, with catalyst manufacturers and fuel cell developers to modestly scale up catalyst synthesis to allow testing in MEAs. The project should interact with the Fuel Cell Consortium for Performance and Durability (FC-PAD) to do early MEA testing of new catalyst systems. RDE testing of activity, and particularly RDE testing of durability, are of questionable relevance to real fuel cell operation.
- Many collaborations are listed on the slides. However, the presentation does not provide evidence of how collaborations are being used in the past year. Attributions are not given. The slides did not show evidence of where reactive spray deposition had been pursued in the past year. Perhaps this points to collaborations quietly ending. The same might be said for density functional theory (DFT) studies. Technology transfer to N.E. CHEMCAT Corporation is mentioned, but the licensing did take place four years ago. It is not clear whether there has been any follow-up in recent years.
- Collaboration in this project seems limited to Los Alamos National Laboratory.
- It is unclear if any of the work was done outside BNL.

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.

- Pt monolayers on non-precious cores are the most promising pathway to obtain both high kinetic mass activity and good high-current-density performance with low-loaded catalysts. This type of catalyst should therefore get priority-level attention, both within this project and throughout FC-PAD. The project must progress past RDE testing to MEA work if relevance is to be improved and the full impact is to be realized.
- The project has a very high potential to have an impact on the fuel cell industry and can be a game-changer if these catalysts can be mass-produced reliably.
- The project is addressing key commercialization barriers toward cathode catalyst activity, cost, and durability.
- DOE has funded work on Pt monolayer catalysts at BNL for many years. While there is no question about the relevance of developing novel oxygen reduction catalysts for commercializing hydrogen fuel cells, it is fair at this point to begin asking questions about whether the development of Pt monolayer catalysts is helping to accelerate commercialization. It has now been over four years since early 2012 when N.E. CHEMCAT licensed BNL technology. Since then, there has been mostly silence with regard to whether these catalysts have been applied toward commercial programs, despite the initial promise that that would occur. If activity or durability needs to be improved, then that would underline the relevance of continued work. However, if there is something fundamentally flawed with high-volume production of catalysts premised on Pt monolayers, then the relevance of this project is questionable. The use of inexpensive cores and a noted interest to improve performance at high current density are relevant pathways to explore.
- The potential impact of the project is high, but accomplishments and promise are not clear.

Question 5: Proposed future work

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

- Plans for more facile synthesis of Nb-based non-noble cores are good. NbN cores should get more emphasis in the planned future work—attention should be concentrated on non-precious cores even if the RDE activities are not as good as with precious cores. The plans given for improving catalyst response at high current density do not seem to focus properly on the prime advantage of the core–shell—the ability to have high-Pt-mass-specific surface areas to keep local current densities at the Pt surface low thereby reducing local oxygen transport losses. Following this line of reasoning, the core–shell should be able to do the job without messing with ionomer content, derivatized supports, or the like.
- Task 1 is reasonable in goals, but focusing on a single material set is recommended. Task 2 is a welcome route to seeking alternative synthesis routes, but the details seem scattered. Task 3 is provocative but poorly defined. Regarding Task 4, while higher current densities are desired, the approaches suggested are unlikely to succeed.
- Proposed future work sounds good. There is a need for more detailed collaboration with industry and other groups that can integrate this catalyst into MEAs with other best-in-class components.
- The future work describes a future emphasis on refractory metals and hollow cores. It would be preferred for the project to focus on these materials rather than those that include precious metals in particle cores. The approach to reach high current density appears scattered. The use of carbon nanotubes appears novel, but it may be frowned upon in certain organizations because of the expense of handling and safety concerns. Furthermore, there should be some way of facilitating high current density with these catalysts without using carbon nanotubes; otherwise, there is no clear reason to investigate the catalysts as they are. Reducing the Nafion® in the catalyst ink would be a start, but it is not clear how much to reduce it. It is unclear what is providing direction as to how to restructure catalyst layers for high-current-density performance. This project contrasts with other DOE-funded projects in which tasks were included to model catalyst layer structure and figure out how to deposit catalyst inks. More information needs to be shown as to how three-dimensionally deposited layers will solve issues at high current density.
- The project should directly involve industry or a national laboratory partner with demonstrated experience in development of optimized electrodes. Work should have a significant focus on demonstrating viability of the catalyst platform. There should be a focus on fully optimizing Nb-based non-noble metal cores to drive up PGM mass activity to well above DOE targets.

Project strengths:

- Owing to their high Pt-specific surface area, Pt monolayers on non-precious cores are the one clear path to low-loaded MEAs that meet DOE activity targets and performance goals at high current density. The project continues to develop promising new catalyst systems. The project has developed non-aqueous synthesis for the calculated-to-be-good PtY system; the synthesis work should allow the critical durability questions about this system to be answered.
- The project is able to conceive of numerous catalyst species and work at fairly high throughput. The project is able to quickly report RDE activities for catalysts. Some of the ideas entailed in the project include means of removing precious metals from the core of catalyst particles.
- The strong technical team continues to demonstrate novel electrocatalysts with high specific activity and high durability in RDE. Novel approaches toward decreasing overall core PGM content are promising.
- Strengths include catalyst synthesis, generating new ideas, and fundamentally characterizing these new catalysts.
- The project has a strong scientific team.

Project weaknesses:

- Many of the project's catalyst concepts still displace Pt with other precious metals. Furthermore, reporting of mass activities normalized by precious metal content is not consistent. Fuel cell data for many of the catalysts do not show high power density at potentials near 0.6 V (a rough thermal limit) and at reasonable high-end operation pressure. Furthermore, the plan for advancing performance at high current density has not been shown to be thorough. Rather than leverage collaborations and understanding of porous media or mathematical modeling, the project seems to frequently respond to performance challenges with material novelty. Examples include depositing thin catalyst layers on gas diffusion media or pondering the use of carbon nanotubes. Instead, it would be preferred to see the project find a way to take the catalysts that have already been studied and make them work. The answer to a performance challenge is not to generate an alternative material (usually).

- The project remains overly dependent upon RDE testing of activity and particularly on RDE testing of durability. Most of the core system studies still involve excessive amounts of precious metals other than Pt. This need not be a problem if the total particle size is not larger than the particle size of competing Pt-alloy catalysts, around 4 nm. The currently listed plans to improve high-current-density performance are not the most promising path to utilizing the inherent advantages of core–shell catalysts. The project should go for high-specific-Pt surface areas.
- The materials set is poorly focused. The project has continued focus on “discovering” new materials combinations without appropriate stability testing despite reported stability issues of previous materials. Analysis of the characterization data presented is inaccurate. There is a dearth of more representative analytical methods like small angle XRD, synchrotron XPS for depth/composition analysis, and XAS in structure analysis.
- There is a lack of ways to make these catalysts into meaningful MEAs and translate the same to high-current-density performance.
- There is too little focus on demonstration of performance and durability in MEAs. PGM mass activities remain below expectations for this promising approach.

Recommendations for additions/deletions to project scope:

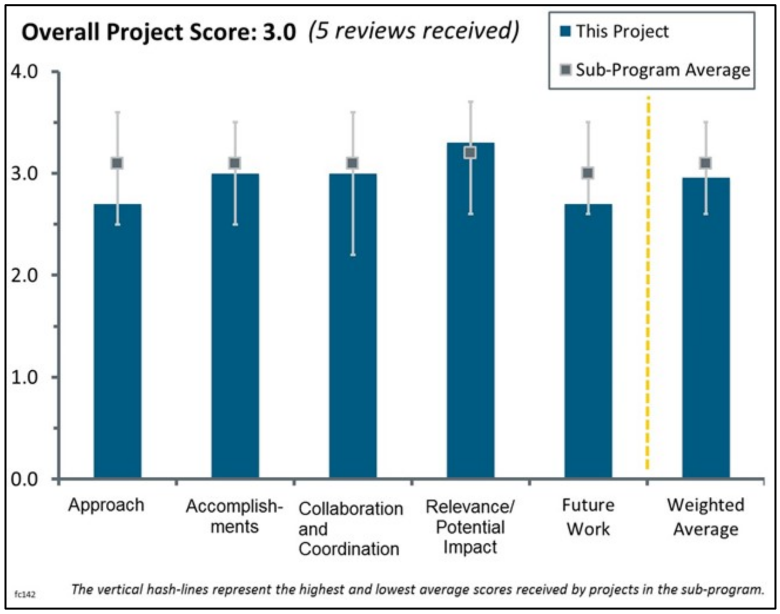
- The project should try TaN in analogy to NbN with possible durability advantages. The team should rigorously test the (suspect) durability of the PtY system and talk with those who conduct DFT about the results. When the project considers which alloy systems should make useful catalysts, considerations should include stability of dissolved species derived from alloying metals. The project should scale up the preparation of several of the most promising systems (including Pt/NbN) and test activity, durability, and high-current-density performance in MEAs.
- All catalysts performing below 300 A/g precious metals should be removed. The focus should shift toward materials with non-precious-metal or hollow cores. Niobium, niobium nitride, and other refractory cores should be emphasized. The project should seek to minimize Au and Pd as much as possible. Each catalyst should be tested in a fuel cell as quickly as possible to understand durability and high-current-density performance. Much greater emphasis should be placed on how to achieve high-current-density performance—an area in which collaborations would help.
- The focus should be increased on stability (and possibly durability) testing in MEA and on fine-tuning selected compositions that should be stable—such as PtPd.
- The project should collaborate with a team with good knowledge of making MEAs to scale up these catalysts. Work on PGM-based cores should be deleted.
- The project should directly integrate an electrode development and testing partner to enable MEA-level performance and durability evaluation.

Project #FC-142: Extended Surface Electrocatalyst Development

Bryan Pivovar; National Renewable Energy Laboratory

Brief Summary of Project:

Platinum catalysis remains a primary limitation for fuel cell commercialization. This project is developing durable, high-mass-activity, extended-surface platinum-based catalysts for decreased fuel cell cost, improved performance, and increased durability. Researchers are focusing on novel extended thin film electrocatalyst structures (ETFECS), a particularly promising approach. Parallel efforts include novel extended nanotemplates; atomic layer deposition (ALD) synthesis of platinum–nickel nanowires; and membrane electrode assembly (MEA) optimization and testing including multiple architectures, compositions, and operating conditions.



Question 1: Approach to performing the work

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

- The project has just begun, but it builds on previous efforts of most of this team. There is good team overlap in specialization, balancing materials synthesis, characterization, and testing. Selection of ALD to attempt the technically challenging goals is a good approach, balancing challenging needs with an industrially viable approach.
- The research team has changed the approach from galvanic displacement to ALD on Ni and Co nanowires to develop a new class of electrocatalysts for oxygen reduction reaction. The team's previous approach for preparing extended, continuous Pt nanostructure did not show good performance in MEAs, as reported in the 2015 Annual Merit Review.
- The proposed synthesis–characterization strategy is rational and may provide a useful way to overcome many limitations that currently impede the cost-effective commercialization of Pt-based alloy cathode materials. However, there is room for significant improvement. For example, the "nanoflowers" direction should be abandoned because—irrespective of the shape of particles—during operating conditions, the "flowers" will be transformed into a spherical shape with activities that will be dependent on the size of the particles and an optimal segregation profile. The nanowire direction is more promising but also needs to be improved, particularly regarding the stability of low-coordinated Pt and Ni atoms. The ALD method may not be the best one for optimizing the thickness and the composition of the film. The investigator may consider developing a pulsed laser deposition (PLD) method, which is more suitable to "synthesize" well-defined films. Last but not least, the approach focuses on testing rather than understanding, which will not lead to optimization of the PtNi alloy systems.
- The project approach of optimization of Pt overcoated Ni nanowires is generally effective. The approach is lacking in development of improved Ni-leaching mitigation strategies in the MEA. The Ni loading in MEA electrodes is very concerning; it would appear that even if a small fraction leaches, significant fractions of the MEA ion exchange capacity (IEC) will be consumed. More effort toward quantifying and improving is needed.
- The U.S. Department of Energy (DOE) has funded a considerable amount of work on dealloyed PtNi catalysts for oxygen reduction, including PtNi nanoparticles (General Motors Company [GM], Johnson-Matthey Fuel Cells Inc.), PtNi nanostructured thin films (3M), PtNi nanoframes (Argonne National

Laboratory [ANL]), and PtNi nanowires through spontaneous galvanic deposition (the former National Renewable Energy Laboratory [NREL] project). Many of these projects have reached considerable maturity in realizing the barriers associated with Ni-leaching, high-current-density performance, and maintaining performance at larger-scale batches. It is difficult to understand how adding another project investigating a PtNi system adds to what is being done, especially a project premised on ALD, which is widely thought to be a low-throughput means of producing catalyst batches. Data presented so far show samples that have been made with up to 13 wt.% Pt from ALD with oxygen chemistry that have in excess of 600 A/g-Pt mass activity. Furthermore, ALD with hydrogen was said to be able to make nanowires with up to 6 wt.% Pt. However, these samples imply that an extraordinary concentration of Ni will be entering into the process of fabricating MEAs and eventually into fuel cells themselves where the Ni will contribute to aggregating the nanowires in ink (based on findings from FC-106), displacing protons in the membrane, and increasing the hydrophilicity of all porous layers. The researchers understand that Ni needs to be leached, but no data have been shown so far to ensure that activity is preserved after nanowire leaching. The project has recognized that there needs to be an understanding of how to allow high performance at high current density, but so far the results seem to indicate the project is cornered; ionomer levels have no impact on mass transport (except at high relative humidity [RH]), and they have no impact on proton conductivity of the layer. Alternative approaches for trying to devise catalyst layers that will operate at 1 W/cm^2 are apparently not allowed in this project. Essentially, the project is a catalyst powder project in which making layers is almost an afterthought until there is a problem, which there already is. Partners such as GM need to be engaged to systematically begin to address this problem through modeling, an experimental design for ink processing, better diagnostic measurements, or all of the above.

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.

- An impressive number of results have been presented in the report; therefore, the investigators should continue collecting data with the same quickness. However, the quality of the results is more important and will define the success of this project. It will be extremely important for the next review cycle to demonstrate high reproducibility of the results because the preparation of samples with the ALD method will require a careful analysis of "a single nanowire." For the next review, it would be highly desirable to discuss more about the stability than the activity of nanowires, as the latter is well understood.
- A significant amount of work has been carried out since the start of the project. Pt deposition was carried out on Ni and Co nanowires, and their electrochemical properties have been evaluated. Enormous physical characterization has been performed in order to understand the ALD of ETFECS.
- A good amount of work has been carried out considering the recent start of this project, but results are not promising as of yet. It is clear that modification of the approach is needed.
- The team has continued to make substantial and steady progress toward development of very high-activity electrocatalysts that exceed the DOE target by several fold in rotating disk electrode (RDE). Little apparent progress has been made towards addressing MEA hydrogen/air performance. MEA limiting current densities of <1 A/cm^2 under hydrogen/oxygen, after repeated MEA acid washes, is indicative of substantial (and perhaps continuous) Ni leaching (slide 16).
- Whether or not the project can overcome barriers associated with cost will hinge primarily on the ability to demonstrate high performance at high current density. So far, the project has shown no more than 0.42 W/cm^2 at 0.6 V for 0.16–0.20 mg-Pt/cm^2. At best, this represents 0.38 g/kW gross, which is much higher than the target of 0.125 g/kW net. Granted, this performance level is with samples from spontaneous galvanic displacement, but in principle, the same performance would be expected with ALD. Of greater concern, however, is the lack of options known to improve this performance level. At 100% RH, some mass transport improvement can be found by lowering the amount of ionomer (according to the limiting current measurements), which would likely result in a modest change but not the >100% improvement needed to meet high-power performance with low Pt loading. The project is showing neither the high current performance it needs nor the line-of-sight to obtain high current performance. The amount of Ni in high-activity samples is concerning. High activity has been shown only for PtNi nanowires with 13% Pt or less. Samples with much higher percentages of Pt have been made with acid leaching, but the activities of these samples have not been reported. Higher weight percentages of Pt on nanowires (prior to acid

leaching) can only be generated after greater than 100 cycles on ALD. Some perspective should be reported on the cost implications of this and whether ALD would represent a practical commercial process.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- The team has an impressive number of collaborators, including an original equipment manufacturer, a catalyst manufacturer for scale-up, and universities.
- This is a team with a good collaborative background, and the geographic proximity makes further coordination likely to continue to be effective.
- It is too early to judge the collaboration; the project has just started.
- The University of Delaware expected to deliver novel Ni nanotemplates, but this still appears to be future work. It is difficult to tell from the presentation, but it would appear that the University of Colorado has been involved in making catalysts through ALD. There does not yet appear to be a contribution from ALD NanoSolutions on the business case analysis, although it would be very interesting to hear more about the business case for ALD. The partnership with GM should be used more to sort out strategy with respect to achieving high-current-density performance. Showing that the project is working with GM to sort out how to achieve the attribute with perhaps the greatest impact on cost would benefit the project significantly.

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 anomalous high performance of extended surfaces continues to be proven, and yet translation to high-surface-area and therefore economic catalysts is needed. This project aims to make high specific activity extended surface area and is attempting to achieve this using ALD rather than the previous focus on galvanic displacement. The goals of this project are high-performance catalysts that are manufacturable, a great need for the Hydrogen and Fuel Cells Program (the Program).
- The project objectives are relevant to DOE's cost and durability goals. Demonstration of hydrogen/air fuel cell performance and durability would significantly advance the state-of-the-art Pt/C and Pt-alloy/C catalyst.
- There is no doubt that any project that focuses on relationships between activity and stability of cathode materials is of paramount importance for developing a new generation of fuel cells. If the investigators would be able to establish such relationships, the project will contribute significantly to DOE efforts for full implementation of electric cars.
- The project is directly relevant to key Program objectives toward cost and performance.
- New oxygen reduction catalysts can still lead to significant advances in lowering the cost and improving the durability of polymer electrolyte membrane fuel cells. In fact, such catalysts present the greatest opportunity for affecting the cost and durability of the entire system. Extended thin film catalysts are also relevant since their high specific activity represents the scientific opportunity of how active an oxygen reduction catalyst can be, which happens to be much higher than what is conventionally observed with Pt/C or PtCo/C. Furthermore, durability can be higher since particles are large enough to avoid high-surface-area loss through agglomeration. The one question about relevance is whether the project addresses cost by addressing performance at high current density. Technically speaking, the answer is "yes" since the presentation reveals that researchers have already grappled with how to improve performance at high current density. However, if the project does not have the right resources to address this (modeling, ink and layer characterization, processing, diagnostics, etc.), then the entire effort will have been for naught.

Question 5: Proposed future work

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

- The team has specifically proposed to perform catalyst evaluation in fuel cell and durability studies, which are essential to fulfill the project objectives.
- The project in general is strong, but greater emphasis should be placed on more averaging characterization techniques rather than microscopy, which is very "sexy" but highly localized. Future work was not clearly identified, but it can be inferred since this is early in the project. A method for evaluating the stability of the Ni substrates would be very useful (e.g., annealing to determine stability of the flake-like materials [unless they are of the correct crystallographic orientation, they are unlikely to be stable thermally and thus probably also electrochemically]).
- The project should reconsider directing research toward synthesis of "nanoflowers"; it may sound nice, but it could be a huge misdirection. In addition, in the proposed future work, one key direction should be fundamental studies of catalyst stabilities. Testing stability in real fuel cells is usually misleading, providing no quantitative information about the corrosion of Pt and Ni. If the corrosion of Pt and Ni is not understood, it will be impossible to know what type of PtNi alloy should be synthesized.
- The project should significantly direct focus toward understanding Ni leaching in MEA and improving Ni stability in the electrocatalyst. If this cannot be overcome, this promising technology will never be commercially relevant.
- On the future work slide, two of the three major categories shown describe work that is more or less already happening or that does not truly address what threatens the success of the project. Yes, the nanotemplate synthesis with greater shape control could produce more active PtNi nanowires, but it is unclear how they should be integrated into a catalyst layer and how the Ni will be prevented from leaching. Unless the shape control and nanotemplate synthesis routes are being dialed in to address both high current performance and durability, some of this effort might involve considerable guesswork. Similar comments could be made for electrocatalyst synthesis work, although to its credit, this work will contain Ni leaching tasks. Optimization of the electrode structure is where the project needs to go. However, it is unclear how it is known that electrospinning is the answer—that is, how electrospinning will provide an enhancement in oxygen flux or proton transport to enable high current density performance. There should be work devoted to understanding how to make a catalyst layer better before committing to particular methods for doing it. It is unclear why carbon should be added as well as what carbon should be added and how much. There should at least be an experimental design for how to approach this. Isolating voltage losses is good, but it is also unclear whether different types of mass transport losses can be separated out.

Project strengths:

- The project has access to considerable catalyst layer processing equipment available at NREL. The overall concept of the catalyst powder lends itself to high specific activity, which means the "ceiling" for activity and improvement in fuel cell performance is high. The investigators are well-connected to the fuel cell research community and have added an automaker as a partner.
- The project has an excellent team, proven benefit, and a rational approach.
- The team has strong collaboration.
- The project has a novel approach to generate extraordinarily high specific activity and mass activity.
- It is too early to make a judgment about the project strengths. Nevertheless, the project strength might be the existence of many tools that are needed for exploring the feasibility of implementation of extended PtNi thin film catalysts in polymer electrolyte membrane fuel cells.

Project weaknesses:

- The project needs to have better direction with respect to addressing high-current-density performance. It needs to have a strategy for how this weakness is to be addressed. The project might address which variables will be studied and why, how prior knowledge can be used to decrease both mass transport and ohmic losses, and how ink processing should be done. These questions all seem relatively unexplored at this moment (with the exception of the ionomer ratio data). The results of the past NREL project indicate a

tendency not to consider fuel cell performance until late in the project. The future work shown indicates that this tendency may carry over into this project. The project is one of many PtNi projects that DOE has funded, which has yielded a common trend: high RDE activity results, followed by poor fuel cell performance, particularly at high current density, which may be related to Ni leaching. The project will have to work hard to avoid this trend and not resemble a duplication of efforts with other DOE-funded projects.
- One weakness is reliance on microscopy rather than more sample-averaged techniques. The “continuous films” produced previously are not continuous, but demonstrated per surface area activity is higher. Without understanding this phenomenon, it is not clear whether this approach is needed. The high variability in deposition due to ALD is worrisome, and it is unclear whether this can be addressed.
- Mitigating measures to inhibit Pt dissolution from ALD ETFECS have not been discussed in the presentation.
- One key weakness is the lack of a clear path toward understanding and minimizing the dissolution of Pt and Ni atoms during fuel cell operation.
- Ni leaching in MEAs is a substantial concern and is not being addressed with enough emphasis.

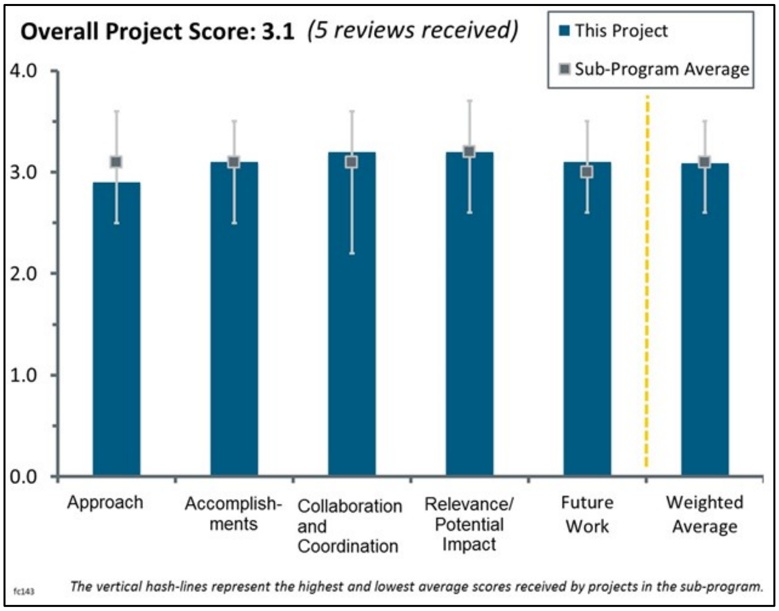
Recommendations for additions/deletions to project scope:

- First, the stability of the hollow Pt-skeleton-like structure (acid-leached 75% Pt sample shown in slide 15) under accelerated stress test conditions is questionable. Second, the team should focus on the methodology for minimizing the wetting problem associated with the ETFECS, which will affect the high current density performance.
- Identifying a method for evaluating the stability of the Ni substrates would be very useful (e.g., annealing to determine stability of the flake-like materials [unless they are of the correct crystallographic orientation, they are unlikely to be stable thermally and thus probably also electrochemically]). The project should shift to more averaging characterization methods (e.g., small angle x-ray diffraction (XRD), x-ray absorption spectroscopy (XAS), synchrotron x-ray photoelectron spectroscopy (XPS)). ALD should be demonstrated on more spherical Ni powders rather than tubes.
- The project should decrease emphasis on making the catalysts and increase emphasis on integrating the powders into catalyst layers. It does not matter that it is early in the project. This kind of task should not be saved until the end; it needs to be planned out now. Collaboration with GM and ALD NanoSolutions needs to be significantly increased with respect to high-current-density performance and the ALD business case respectively. The project needs to work with GM to develop an experimental strategy for assuring high performance at high current density. It should not be enough to say that electrode optimization will happen; questions need to be answered about how this will be conducted from dealloying to ink-making to ink-processing to catalyst layer deposition to cell conditioning. Some indication needs to be given about cost and high-volume throughput that could be expected for ALD—both oxygen and hydrogen chemistries.

Project #FC-143: Highly Active, Durable, and Ultra-Low-Platinum-Group-Metal Nanostructured Thin Film Oxygen Reduction Reaction Catalysts and Supports

Andrew Steinbach; 3M

Brief Summary of Project:



This project is developing thin film oxygen reduction reaction electrocatalysts on nanostructured thin film (NSTF) supports developed by 3M. The aim is to exceed all U.S. Department of Energy (DOE) 2020 cost, performance, and durability targets. The electrocatalysts will be compatible with scalable, low-cost fabrication processes. The project will integrate the catalysts into advanced electrodes and membrane electrode assemblies (MEAs) that address traditional NSTF challenges, which include operational robustness, contaminant sensitivity, and break-in conditioning.

Question 1: Approach to performing the work

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

- The approach is good in achieving DOE’s goal for electrocatalysts and supports.
- The approach is good with the focus on NSTF as a platform. Although NSTF has issues for use in some fuel cell applications, as a catalyst platform it is good. One does want to know how it translates to other systems in terms of understanding. The combined experiment and modeling has good potential, although it needs to include explicit solvent techniques to understand how activity will perform under operating conditions. It is unclear whether it is more thermodynamic in nature and it is not clear what mechanisms are being assumed.
- 3M and the NSTF product remain interesting materials for fuel cell applications. Pursuing these materials using a nanoporous and ultrathin film approach seems to be a reasonable mechanism to potentially improve electrochemically available surface area and, thereby, mass activity. Slide 7 shows a high dependence on density functional theory (DFT) to guide/interpret results. It is not clear how valuable/accurate this effort will be in improving the performance or designing higher performing electrodes. The team has already shown the ability to perform high-throughput experimental work in this area and this may be the better route. The inability to share more information about composition/processing inhibits the ability to judge the merits of following different approaches.
- Without any details being given in the presentation on the types of synthesis and/or post-treatment changes used to generate the two modified catalyst types (nanoporous and ultrathin) of this project, it is hard to evaluate the claim that significant improvements require the optimization of a large composition/process space. Previous NSTF projects extensively explored the PtNi composition space and the presentation did not seem to give any data that showed that earlier optimization was no longer valid (other than a lack of composition dependence in the [lower] activity and specific areas of the non-annealed P4A). Increasing the specific surface area of NSTF should be helpful in addressing contamination issues and high-current density performance durability as the ionomer migrates from the membrane to the electrode. However, the area target of 30 m^2/gPt for the nanoporous catalyst is at best marginal and the target of 20 m^2/g for the ultrathin film (UTF) is inadequate for dealing with local oxygen transport. Part of the UTF approach was listed as maximizing the NSTF support surface area. This has always been an obvious approach to addressing robustness as well as activity issues, but past efforts in this area have not seemed productive.

- This project continues to develop NSTF-based catalyst technology and seems to be a continuation of FC-104. This NSTF-based catalyst technology showed significant potential to achieve high catalyst activity. However, the most significant problems are its generic catalyst layer structure, the so-called ionomer-free catalyst layer, and the difficultly achieving the sufficient operational robustness and reproducibility (conditioning). The MEA conditioning is one of the technical questions. FC-104 could not demonstrate a sufficient performance in the short stack at General Motors even though a good performance was shown in the MEA at 3M. The objective of this project is focused on obtaining 200% of the 2020 mass activity target. On the other hand, robustness and reproducibility are not discussed. One of the pre-work activities of this project showed significant improvement of operational robustness of the MEA but the principal investigator (PI) did not disclose how the project achieved it. It seems that the project focuses on the strength of this technology and avoids generic technical problems. Because we see a potential for this unique catalyst technology, it is more meaningful as a pre-competitive research to investigate the catalyst structure in order to gain mechanistic understanding of ionomer-free catalyst layer technology. Some of the approaches that the project discussed, such as high-throughput fabrication, seem to be more like engineering phase approaches than pre-competitive phase approaches.

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.

- Substantial work has been performed and one of the DOE 2020 targets for catalyst durability under accelerated stress test conditions has already been achieved.
- At least one of the advanced electrode structures seems to give improved operation robustness versus traditional NSTF. If this is done without use of a dispersed-catalyst interlayer, it could give significant durability advantages (versus traditional NSTF and interlayer), reestablishing one of NSTF's prime durability advantages versus dispersed-catalyst electrodes. The addition of "M," presumably to PtNi, seems to stabilize the specific surface area versus cycling of the nanoporous films, which could improve overall durability significantly. 3M appears to have developed a method that can deposit significantly thinner and more uniform Pt-alloy layers on the NSTF substrate whiskers. This could increase the utility of increasing the area (i.e., length or areal number density) of the substrate whiskers. The combination would be more useful than either development alone. The utilities of the compositionally graded MEAs and the segmented cells seem questionable because processing variables are likely to be more important than composition in the optimization of both nanoporous and UTF catalysts.
- The project was just started, and many technical accomplishments are not expected. Some interesting early work data was presented, such as high performance with hydrogen/air at lowered platinum group metal loading and good retention of performance after the potential cycle tests. Particularly, operational robustness data showed significant improvement over a traditional NSTF electrode. The PI declined to disclose details of this MEA information.
- Accomplishments are limited, but it is a new project. Initial work seems promising, especially the high-throughput work. So far, most new work has focused on performance and not durability, so the feasibility of some of the approaches over time is unknown.
- As the project is still just starting, this category is perhaps more difficult to judge, although the results presented—both for new materials and as background for previous materials—show a lot of samples being screened and some significant improvements in "operational robustness," allowing advanced electrodes to achieve a broader operating window starting to approach that of Pt/C. Improvements in electrochemical surface area (ECSA) are encouraging, although these have been attributed to "pre-project" work. Performance optimization approaches are difficult to evaluate because arbitrary catalyst variables have limited value. The NSTF work seems more consistent with previous work, and the UTF work seems more novel, but also less advanced. The high-throughput aspects presented should help the project advance.

Question 3: Collaboration and coordination with other institutions

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

- The project has a good team, comprising laboratories, academia, and industry. Everyone seems to play separate roles, so good coordination will be key. It is not clear how many 3M scientists contribute significant time on this project. Because this is an electrocatalyst project, it would be good to provide the NSTF catalysts to others that make MEAs, such as the Fuel Cell Consortium for Performance and Durability and the project from the National Renewable Energy Laboratory.
- The project has a very strong team. It would be nice to have an original equipment manufacturer (OEM) participate so that relevant operating conditions, an area of challenge for NSTF, would be specifically probed.
- The team has very good collaboration with universities and national laboratories.
- Collaborators are addressed in the academic area and they seem to be enough for the characterization of MEAs. Operational robustness evaluations should involve automotive OEMs.
- The kinetic Monte Carlo (kMC) calculations on alloy surface structure predictions at Johns Hopkins University should provide useful comparisons with experimental results. It is not clear what new findings will come out of the DFT calculations at Purdue University. It is too early to know whether the national laboratories' characterization efforts will be significant.

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.

- If successful, the project will meet all or most of the DOE 2020 metrics for electrocatalysts and supports.
- The approach is taking on the critical issues of performance, cost, and durability, and it is only really finding problems at peak power and/or dynamic operation. These are the most significant concerns for automotive applications in which a wide range of operating conditions are to be expected.
- The project objectives are meeting and exceeding DOE 2020 targets, particularly for the project target of catalyst activity (mass activity), which is two times higher than the DOE target. More generically, the NSTF catalyst demonstrated very high specific activity so that the project now needs to significantly reduce the catalyst loading to achieve significant high mass activity. However, the most significant technical barrier of this catalyst technology is operational robustness and reproducibility of the performance. It was considered that the ionomer-free catalyst layer of this catalyst technology could be a problem. There is not enough mechanistic understanding of how this ionomer-free catalyst works. Study in this area is one of the most important work areas. The current DOE research, development, and demonstration (RD&D) target does not address the robustness attribute enough.
- If the new catalysts give operational robustness without dispersed-catalyst interlayers (and if membrane improvements lessen NSTF degradation by membrane fragments), they could enable the full cyclic-durability promise of NSTF to finally be realized in fuel cell applications. There is a significant possibility that the changes generated by this project will provide only incremental improvements that are insufficient to get NSTF into significant fuel cell applications. High-throughput methods are unlikely to contribute significantly to the productivity and impact of this project.
- NSTF in OEM cells is still unproven but the catalyst scaffold could be good to determine. The impact is dependent on MEA tests as well although electrode structure development is outside the scope per the funding opportunity announcement, so it is hard to ascertain impact.

Question 5: Proposed future work

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

- Considering the strengths of various collaborators, the proposed experimental and model studies for the upcoming year can be completed on time.

- Based on project approaches currently addressed, the future work makes sense. DFT models would help characterization work. Again, the most significant technical barrier of this catalyst technology is operational robustness and reproducibility of the performance. It was considered that the ionomer-free catalyst layer of this catalyst technology could be a problem. There is not enough mechanistic understanding of how this ionomer-free catalyst works. Study in this area is one of the most important work areas.
- While quite solid, it is unclear whether performance and durability should be the primary focus (as laid out in the project) or whether "operational robustness," the historic Achilles' heel of NSTF, should have more emphasis. It is also unclear how the kMC and DFT models will lead to improved materials/performance.
- Proposed work seems adequate although a bit nebulous. There is a lot of work to be done and it would be good to know what options they have if certain activities do not occur (e.g., cannot get reliability of techniques). It is not clear how predictive the models will be because they will be based on calibration of data sets. It is unclear whether there is a way to test them.

Project strengths:

- NSTF catalyst technology has a significant potential to achieve high activity and durability. It shows very high specific activity and the project is focusing on the improvement of mass activity. Also, the thin film may have a more bulk-like nature and strong potential to make catalysts durable compared to nanoscale particle-based catalysts.
- Project strengths are the combined experiments, computation, and high throughput. The two main approaches have made seemingly good progress and had good performances in a short time.
- The prime recipient has a track record of continuously working on the NSTF-based catalysts, which is an important factor in understanding the catalyst system and possible commercialization in the near future.
- NSTF is a great platform for high throughput with a tremendous amount of background to build on.
- Two modifications of the basic NSTF catalyst are now available for optimization: (1) porous and (2) thin uniform deposits. One can rationalize reasons why each of these could be helpful, and there are some encouraging initial data.

Project weaknesses:

- There are two project weaknesses: (1) the ultrathin film catalyst layer may not be stable under 0.6–1.0 V potential cycling conditions, and (2) there is no clear proposed work to minimize the water flooding on the thin catalyst layer under high current density operation.
- NSTF has had problems in the area of operational robustness and, while it is good to see that some advances may have been made, it is unclear why/how these occurred and whether they can be further advanced.
- The extent of catalyst improvements achievable versus standard NSTF may be limited by the limited surface area of the NSTF support structure and the thinness of NSTF catalyst layers. The presentation made some mention of increasing the area of the support, but in the absence of detailed plans for this and the limited progress made in past attempts, the ultimate improvements from this project may be incremental at best.
- Stability of gradient structures is unknown. It is unclear as to how rapid the high-temperature route and process is and whether it is truly high-throughput. Also, it is not clear what the focus and evaluation mechanisms are.
- The most significant technical barrier of this catalyst technology is operational robustness and reproducibility of the performance. It was considered that the ionomer-free catalyst layer of this catalyst technology could be a problem. There is not enough mechanistic understanding of how this ionomer-free catalyst works. Study in this area is one of the most important work areas. The project does not cover this.

Recommendations for additions/deletions to project scope:

- The project should make a renewed effort at growing significantly longer NSTF whiskers to increase overall surface area and thickness of the catalyst layer.

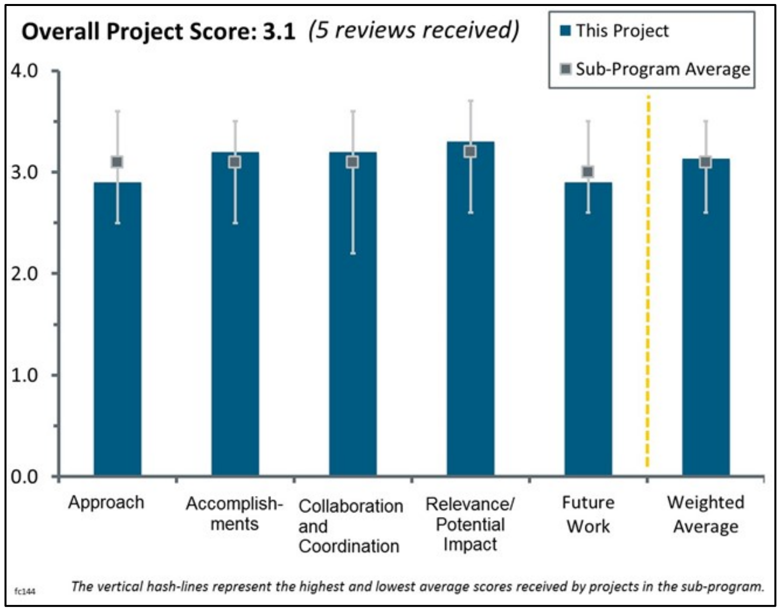
- The project needs to report conditioning procedures in the robustness of these materials. The project should measure transport and surface properties related to operation in cells as well. For dealloying, it would be good to be in contact with the ionomer as well to see whether it continues any leaching, which could explain performance differences between liquid and MEA.
- The project should have less DFT/kMC modeling related to catalyst performance, increase effort in operational robustness, and perhaps add water management/fuel cell performance modeling to help this area instead.
- The project emphasized the strength of this thin film-based catalyst technology to achieve further high-catalyst activity. On the other hand, the project scope does not include the generic problem of this catalyst layer structure—the so-called ionomer-free catalyst layer—and it is difficult to achieve sufficient operational robustness and reproducibility. It is highly recommended that the team revise the project scope and cover the operational robustness and ionomer-free catalyst layer study. Also, DOE RD&D should address the operational robustness in the technical target. The current transient performance target is not enough to address this attribute.

Project #FC-144: Highly Accessible Catalysts for Durable High-Power Performance

Anu Kongkanand; General Motors (GM)

Brief Summary of Project:

This project aims to reduce overall stack cost by improving high-current-density performance in hydrogen/air fuel cells that meet U.S. Department of Energy heat-rejection and platinum-loading targets. Investigators will maintain high kinetic mass activities and mitigate catalyst degradation by using supports with more corrosion resistance than the current high-surface-area carbon (HSC). The project takes a four-pronged approach: (1) improve oxygen transport with new carbon support, (2) reduce electrolyte–platinum interaction, (3) enhance dispersion and stability of platinum–cobalt particles, and (4) improve understanding and control of leached Co2+.



Question 1: Approach to performing the work

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

- The approach is very sound and is tackling local resistance, although the exact cause is still unknown, so it is a bit of a shotgun approach. It is not certain that the dispersion of the PtCo is the most efficient route, especially compared to the interactions with the Pt and support and ionomer interactions. It would be good to see some more down-select of the different strategies and what happens. It is not clear how Pt findings translate to Pt alloy. There is a good systematic approach for MEA integration but not so much so for the catalyst. Multiscale modeling is a good approach, but how the bridging will occur is unclear. Anchoring of the Pt is a good approach and is interesting.
- One of the approaches for this project—namely, enhancement in dispersion and stability of PtCo particles—has been widely studied and is well documented in the literature. The other approaches, such as improvement in oxygen transport with new carbon support and reduction in electrolyte–platinum interaction, may provide useful information in designing a stable catalyst for the oxygen reduction reaction (ORR).
- There is a problem with the proposed approach. On slide 6, the approach is discussed in light of what the investigators would like to do rather than how they will organize and perform the experiments. Slide 6 summarizes many challenges that polymer electrolyte membrane fuel cell (PEMFC) systems currently present. The strategy or approach is a methodology of how these challenges may be overcome or significantly reduced. That said, the four challenges listed on slide 6 are very important in the quest to move the field of fuel cells forward. On the other hand, in slide 7, the investigators describe what they are going to do rather than how they are going to do it, which is necessary when defining an approach. Furthermore, some questions raised on slide 6 are puzzling. For example, it is not clear what HSC has to do with the kinetics of the ORR. The kinetics of any electrochemical reaction are clearly defined by the electronic properties of the catalysts and should not be dependent on the surface area of either the catalysts or the support. Certainly the number of active sites will control the measured current but the specific activity should be the same. Rigor should be used in defining terms that control activity. Returning to the approach, it would be of paramount importance to discuss what experimental and computational tools will

be used for addressing the many question marks on slide 6. Slide 6 should be followed by slide 20 (selection methods).

- This project is basically taking an empirical approach and characterization with conventional materials. The overall project approach seems to be balanced between material fabrication, modeling, and characterization. It is hard to see what the new development is. Nitrogen-doped anchoring would be one of the new developments but it is a small fraction of the entire project. It is hard to understand why this project is still looking at HSC as a support, and the rationale to choose a conventional PtCo catalyst is also unclear. There is no information about new ionomer materials to be used for this project. Nanoscale diagnostics (visualization techniques) seem to be effective but how it connects to the modeling is still a question.
- This project was awarded and presented as a catalysis project, yet it is nearly exclusively a membrane electrode assembly (MEA) development project. In terms of the approach, half is MEA and half is catalysis; this does not seem appropriate for a catalysis development project. Of the half that is catalysis (New Carbon Support and Enhance Dispersion and Stability of PtCo Particle), even the portion related to the carbon support is described fully as an MEA task. The approach for “Enhance Dispersion and Stability of PtCo Particles” was missing from the presentation; what is intended to be done here is unclear, let alone new.

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.

- For a newly funded project with a start date of April 1, 2016, the amount of information presented under Technical Accomplishment is quite impressive.
- An impressive number of results have been presented in the report; therefore, the investigators should be encouraged to continue with collecting data with the same speed. Understanding the role of Co2+ in oxygen transport is very important. It is somewhat puzzling, however, how it will be possible to resolve this issue simply from testing the fuel cell stack. It would be of principal importance to develop a fundamental program that can provide more reliable data on relationships between concentration of Co2+ and oxygen transport. It was difficult to understand from the presentation what methods will be used to establish dissolution of Co2+ from PtCo during operation and how to separate the loss in activity from Co being leached out from the alloy (a true kinetic effect) versus the effect of Co2+ on oxygen transport. As a consequence, it is very important that effort is directed toward acquiring more reliable data on relationships between the activity and stability of a PtCo alloy and the concomitant effect of Co2+ on oxygen transport.
- The project just started, but some interesting pre-work or early work data was presented. In particular, the pie chart showing the fraction of local oxygen transport resistance in the entire mass transport overpotential is interesting, but whether it is empirical data or conceptual estimation is still a question.
- This project is new, is described as 1.2% complete, and had been ongoing for approximately two weeks before the slides were submitted. It is too early to comment on accomplishments and grade. Past projects involving this principal investigator have gone well.
- This is a new project.

Question 3: Collaboration and coordination with other institutions

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

- The subcontractors for the project include an original equipment manufacturer (OEM), three universities, and a national laboratory. All the individual entities have their proven expertise in the proposed research topic.
- It is a large but structured team. It is unknown how well the interactions— including knowledge and material transfer—will occur throughout the various members. It is not clear how this project overlaps and works with the Fuel Cell Consortium for Performance and Durability because, as presented, it is an MEA project and not a catalyst project. It is not clear whether MEAs will be provided.
- This project is new and subcontracts were not yet in place. As described, there is a good set of collaborators intended for the project but it is yet to be determined how they will interact.

- There is balanced collaboration among academia and industry.
- It is too early to judge the collaboration; the project has just started.

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 critical to the Hydrogen and Fuel Cells Program (the Program) and fully supports DOE research, development, and demonstration (RD&D) objectives. In particular, the project aims at a platinum-group-metal content of <0.125 g/kW, mass activity of 0.44 A/mg, mass activity loss of <40%, and performance at rated power of >1.0 W/cm^2.
- Adding the area-specific current density to the target is relevant to the DOE RD&D and automotive goals.
- There is high relevance with a potentially high impact on the Program.
- The project is addressing a major problem of local resistance. Goals are in line with needed improvements. This is not an electrocatalyst project.

Question 5: Proposed future work

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

- The team has proposed an enormous amount of future work, including carbon support selection, ionomer selection, ionic liquid selection, electrode design selection, and intermetallic alloy development for the first-year work flow.
- Future work is relevant to the project objectives and approaches.
- In general, the proposed future work is reasonably well organized. One direction that needs to be improved is developing methods capable of resolving the issues of stability of Co, as well how to optimize the physicochemical properties of nanoparticles to optimize activity and stability of catalysts.
- Future work seems in line with the goals and down-select. What types of MEA testing will be done, including cell assembly and conditions, is not clear. Although the future work is focused on performance, one must also worry about durability, especially at low loadings. The stability of the ionic liquid during operation is unclear, especially low temperatures and startup. There is no electrocatalyst development.
- The project is new; the entire approach can be considered the future work as there is 99% to go. The project as presented is an MEA project and not a catalyst project. Little to no information was presented in terms of what the project intends to do to develop higher-activity, more-stable catalysts. It appears that this project will primarily select materials already developed and then incorporate them into MEAs to determine the performance. It is unclear how this is appropriate for a catalysis project.

Project strengths:

- The project is tackling a critical problem with divergent approaches. There is a strong team and a systematic approach.
- The goals are clearly defined. This synchronized experimental and modeling effort will guarantee a fast transition from understanding transport limitations and performance of PEMFC.
- The strength is characterization of catalyst layers to improve the performance for both catalyst activity and mass transport at higher current density.
- The approach of minimizing oxygen transport and selecting materials for the highest transport should lead to improvements in MEA performance.
- The team has very strong collaboration.

Project weaknesses:

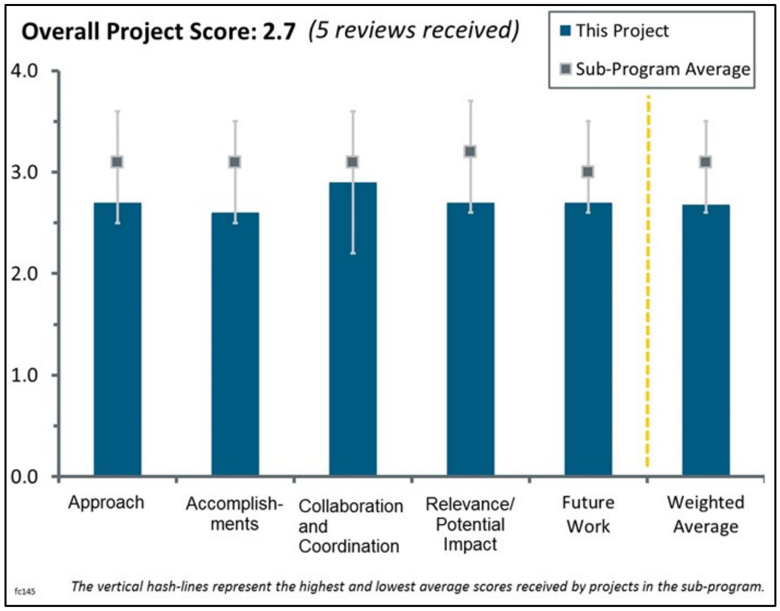
- One key weakness is the lack of a clear path toward understanding and minimizing the dissolution of Pt and Co during fuel cell operation. Currently, understanding the activity of Pt-based materials is of lesser importance than understanding the stability issues.
- It is hard to see a factor of the new development, such as new materials and diagnostics, in the project. Nitrogen implantation for catalyst anchoring seems to be a part of the new development. The new ionomer material is still unknown.
- How the members of the large team interact with each other is not clear. This is an MEA and not a catalyst project. The project should examine and be concerned about stability and durability.
- The project should comment on the costs of processing with ionic liquids versus cheaper solvents.
- There is no catalyst industry partner to scale up the proposed catalyst(s).

Recommendations for additions/deletions to project scope:

- More factors of new development, e.g., new materials and diagnostics, would improve the quality of the project. So far, it is hard to distinguish from conventional characterization, which is usually pursued by fuel cell OEMs.
- This is supposed to be a catalyst development project and thus catalyst development should be stressed and shown and not just its integration and electrode layer performance.
- If the project is to be a catalysis project, the project should concentrate on being a catalysis development project, not an MEA development project.
- A catalyst manufacturer should be included in the initial stage of the project.
- There are no recommendations at this stage of the project.

Project #FC-145: Corrosion-Resistant Non-Carbon Electrocatalyst Supports for Proton Exchange Fuel Cells

Vijay Ramani; Illinois Institute of Technology



Brief Summary of Project:

Carbon's high electrical conductivity and low cost make it an excellent electrocatalyst support but corrosion leads to kinetic, ohmic, and mass transport losses. This project is synthesizing doped non-platinum-group-metal metal oxides as non-carbon alternatives. Along with being corrosion-resistant, the project supports would have high surface area, exhibit strong metal-support interaction with platinum, and demonstrate high electrocatalyst performance.

Question 1: Approach to performing the work

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

- Use of non-carbon supports provides the one real hope for completely materials-based solutions to start–stop and fuel starvation challenges—no carbon can survive these conditions without systems-level mitigations. This project is a rational continuation of the predecessor project that obtained some encouraging results albeit by introducing some potentially expensive and corrodible platinum group metal (PGM) (Ru) into the oxide support. This project attempts to further improve performance while removing all PGMs from within the support. The targeting within the project seems reasonable, and the choice of materials to be tested seems good though not particularly innovative. The leaching of sacrificial silica seems to be an effective way to achieve decent support surface areas. The proposed evaluation techniques seem appropriate. It should be noted that getting an oxide support to work at all in a fuel cell is much more difficult that getting it to work in a rotating disk electrode (RDE). One needs to completely reengineer the electrode layer because of the different hydrophilicity and density of an oxide support vs. carbon and the challenges in maintaining electrical conductivity and avoiding dissolution over the full operating range of the fuel cell. Seeing any fuel cell performance at all with an oxide-supported electrode layer is a major achievement.
- The approach is good, but it is unclear whether this is a catalyst or a support project. Use of modeling to guide materials development is good; however, it is not clear that the density functional theory (DFT) will include solvent to understand surface and leaching under operation. It is also not clear what the Pt particle sizes are and also whether alloys will follow the same trend for interactions.
- The investigators proposed to use DFT calculations to understand how doping may change the electronic structure of TiO_2 and, in turn, its conductivity. Although this is an important step, it is also very important to point out that the deposition of Pt on doped semiconductors will also affect the conductivity of Pt via the very well-known semiconductor–metal interactions (e.g., the Schottky barrier). The investigators also proposed testing the stability rather than using analytical probes such as inductively coupled plasma mass spectrometry to quantify the stability of Pt and the dopant use to increase the TiO_2 conductivity.
- Finding alternatives to corrosion-prone carbon supports is a worthwhile endeavor. Simply improving on the corrosion resistance without maintaining or improving other properties (conductivity, surface area, mass/specific activity) is insufficient. Focusing on Ti, Ta, and Nb is justifiable. Most of the presentation focused on approach; and the value of DFT modeling, which was the focus of two slides, is unclear as is

the value of porous supports (the accessibility of which is uncertain) or scale-up (which is not really relevant at this time—not until high-performance, durable materials are demonstrated).

- The project should clarify lessons learned from previous projects on metal oxide support materials (FC-085). Although some catalysts with metal oxide support developed under the previous project (FC-085) showed impressive durability under accelerated stress tests (ASTs), the catalyst performance was very low. Electrochemical surface area (ECSA) was very low. The basic catalyst performance is one of the most crucial criteria of non-carbon support catalyst material. However, it is not addressed in this project. Doped TiO_2 could be conductive (semi-conductor), and it is still questionable whether it can be adequately conductive as a catalyst support material. The project should clarify the basic idea of how the catalyst performance can be achieved with non-carbon-based support materials. The project should clarify criteria for material to be used as a catalyst support. The project indicated that porous TiO_2 support is an idea and this material may increase the surface area of the support materials. The question is whether the dispersion of Pt catalyst particles can be enough to achieve the catalyst performance, including ECSA.

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.

- This topic is harder to judge as the project is at its beginning stages. The fact that some data is included is good plus the inclusion of a new support made by sol-gel and the characterization of $Ta_{0.3}Ti_{0.7}O_2$ that can be attributed to this work. Much was made of strong metal–support interactions (SMSI) with either modeling or XPS/XAS (x-ray photoelectron spectroscopy/x-ray absorption spectroscopy) results being suggested to support the statements made. Not enough data were presented (nor was a methodology sufficiently discussed) to make an assessment of the ability to quantify SMSI in a meaningful way or show how it has a meaningful impact on any relevant properties. The BET (Brunauer–Emmett–Teller) surface areas achieved/targeted seem low. All mass activities reported to date are so low as to be unexciting.
- The project was just started, and there was not enough time to show significant accomplishment. Some pre-work and early work were introduced. The project sees potential for Ti-based materials to show good corrosion resistance under the fuel cell operating environment. However, the promising information or data shown is contingent upon adequate electrical conductivity. The project indicated that it was pursuing porous TiO_2 support, which may increase the surface area of the support materials. The question is whether the dispersion of Pt catalyst particles can be enough to achieve catalyst performance, including ECSA.
- Preliminary work has shown some performance of a Ta-doped TiOx-supported electrode in a fuel cell (it is not clear whether this was in oxygen or air). Although that performance is inadequate, this alone is a major achievement. The claims for substantial stabilization of Pt on Ta-TiO_2 via SMSI seem to be stretching the interpretation of the data a bit far. SMSI should help (particularly after strong reduction of the oxide), but how much it should help seems unclear.
- It is too early to judge the accomplishments of the project; the accomplishment slides are a mixture of previous results and “new” data that are very difficult to decouple.
- This is a new project.

Question 3: Collaboration and coordination with other institutions

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

- While limited in the number of participants, the inclusion of two academic institutions with an original equipment manufacturer (OEM) is reasonable for this effort. The best addition to the team would be an industrial entity with experience synthesizing/selling/supplying similar materials to those targeted in this project.
- This is a decent team with an OEM and academia. It would be good to see more interactions with some of the national laboratory consortia. The exact role of the Illinois Institute of Technology (IIT) is not clear as a good deal of materials development is seemingly done by and at the partners.
- The project includes both academic and industry partners; however, the roles of each partner were not well defined. In particular, there is a still question as to who would be responsible for the catalyst performance.

Corrosion-resistant and conductive metal materials are similar to the metallic bipolar plate requirement. TreadStone Technologies worked on doped TiO_2 as the bipolar plate coating under the DOE Small Business Innovation Research Program. TreadStone Technologies can be a candidate partner for the support materials development.

- The project seems highly dependent on Nissan for testing in membrane electrode assemblies (MEAs). As long as such MEA work happens early and often in the project, this is okay. RDE work is close to irrelevant in oxide support work. IIT would be well-advised to develop in-house MEA testing capability, or at least to draw the Fuel Cell Consortium for Performance and Durability's MEA testing capabilities strongly into the project.
- The project was initiated four months ago, so it is very difficult to provide any objective judgment on the 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 **2.7** for its relevance/potential impact.

- If oxide supports could be made to work, they could provide a materials solution for startup and fuel starvation. This would simplify fuel cell system operation, would very modestly reduce system cost, and could ease the entry of new fuel cell system developers into the field. It should be noted just how hard it is to engineer new oxide-supported electrode layers with the full performance of carbon-supported electrodes, so the probability of this project's full success should be considered quite low.
- The subject of support is very important, although there is skepticism that it will be possible to replace high-surface-area carbon in the near future. Certainly, such an effort is a logical response to many problems presented by the carbon supports currently used.
- It is not clear how much of a concern carbon corrosion still is as there are system mitigation strategies, although a materials solution would be better. It would be good to know the cost comparison between the new supports and standard Pt/C materials.
- Corrosion concerns are an area that can be improved upon; however, system solutions have been developed that allow today's corrosion-prone materials to meet durability targets (while also achieving performance targets). The poor relative performance of the materials developed in this project (and its predecessor) leave a significant concern that materials with comparable performance can be generated. It is unclear that if such materials are created, they will have a meaningful impact on commercial viability of fuel cell systems.
- Basically, the research focus is shifting from the catalyst itself to the catalyst layer to maintain high kinetics of mass activity *and* enhance the performance at the high current density. In the industry, carbon corrosion has been mitigated by so-called system solutions. The importance of this durable catalyst support is relatively lowered.

Question 5: Proposed future work

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

- Based on the addressed approach, the future work is relevant.
- The testing proposed is reasonable and relevant. The key issues are the performance and durability of materials generated within the project. Based on the data presented and the proposed material sets investigated, it is not clear that performance and durability improvements are possible that would lead to novel supports of high commercial relevance.
- The future plans are reasonable except that essentially all testing should be done in MEAs rather than in RDEs to avoid developing false hopes (although if the support does not work at all in an RDE, it is not worth testing in MEAs). A few more specific ideas on other dopants to use with TiO_2 and on other base oxides to be tried would be helpful in justifying the continuation of this project.
- Future work is a bit broad and so some specificity would be good, especially against metrics. The presentation was vague concerning ways to enhance activity and stability.

- Some elements are there, but a key element that is missing is quantifying the stability of the support and the catalysts.

Project strengths:

- The principal investigator (PI) seems to have a good grasp of the challenges associated with developing effective oxide-supported electrodes and of the methods needed to assess the origins of performance shortfalls. The project plans follow rational if not innovative lines of development.
- The PI has proven in the past that he is able to develop and execute similar projects. The methodology is rather well developed.
- The project has a systematic approach for new materials and supports and for conducting oxides. There is good use of modeling to guide materials development.
- The team is investigating reasonable materials for advanced supports, and the proposed work is scientifically sound.

Project weaknesses:

- The project does not address technical problems of metal support, including the catalyst performance issue experienced by the previous project. Also, this project does not scope how the adequate catalyst performance can be achieved with metal oxide support.
- The proposed approaches are rather obvious and not particularly innovative. It is not clear why they would not already have been followed through to completion under the predecessor project.
- There is no alternative direction if the proposed systems will not work as planned, and there is skepticism that it will work.
- It is unclear that the materials being pursued will ever come close to the performance of current state-of-the-art materials in use today.

Recommendations for additions/deletions to project scope:

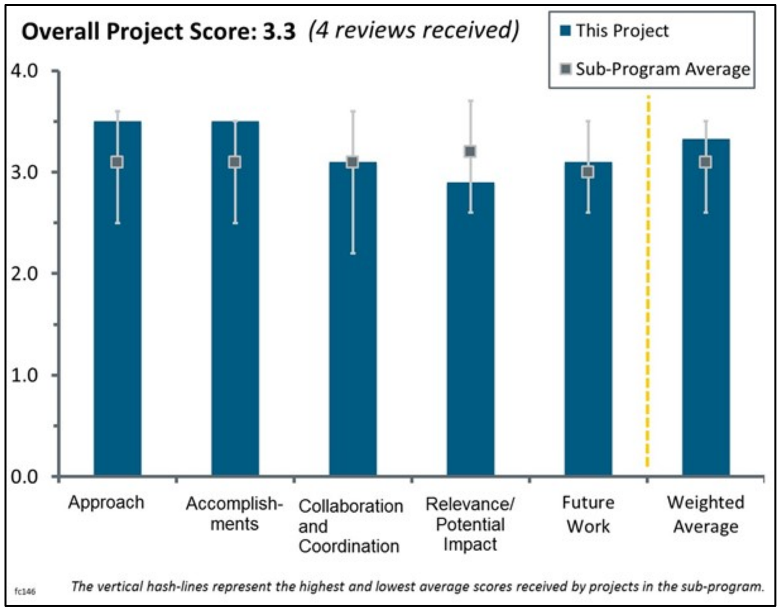
- The project should clarify lessons learned from the previous project that investigated metal oxide support materials (FC-085). Although some catalysts with metal oxide support developed under the previous project (FC-085) showed impressive durability under AST, the catalyst performance was very low. ECSA was very low. The basic catalyst performance is one of the most crucial criteria of non-carbon support catalyst material. However, catalyst performance is not addressed in this project. Doped TiO_2 could be conductive (semi-conductor), and it is still questionable whether it can be adequately conductive as a catalyst support material. The project should clarify the basic idea of how the catalyst performance can be achieved with non-carbon-based support materials. The project should clarify criteria for the material to be used as a catalyst support.
- The project should publish a series of protocols for the evaluation of oxide-supported electrode layers. The protocols should include tests for electronic conductivity after (separate) exposure to electrochemical potentials at both oxidizing and reducing potentials, tests for support dissolution, tests for surface area retention of both the catalyst and the support, and tests of hydrophilicity and changes thereof during electrochemical operation. The project should recommend at least three other oxide/dopant compositions for future investigation. More attention should be given to control of hydrophilicity in oxide supports.
- Key are the interactions between support and ionomer, as well as the surface properties, and so it would be good to add such characterization. It is recommended that the project investigate some high-throughput techniques and approaches to progress the supports faster, which DOE can leverage. The project should focus on and address possible passivation under different operating regimes.
- It would be preferable to see one support family pursued in more detail, with the project trying to define how much improvement in performance and durability might be possible within a single class of supports, as performance to date of these materials falls significantly behind current state-of-the-art materials.

Project #FC-146: Advanced Materials for Fully Integrated Membrane Electrode Assemblies in Anion Exchange Membrane Fuel Cells

Yu Seung Kim; Los Alamos National Laboratory

Brief Summary of Project:

This project is developing advanced materials for fully integrated membrane electrode assemblies (MEAs) in anion-exchange membrane fuel cells (AEMFCs), enabling fuel cell cost reduction without sacrificing performance. The improved anion-exchange membrane (AEM) materials are based on highly conductive and stable hydrocarbon polymers. The project also aims to address challenges with integrating catalysts and AEMs into high-performance MEAs. The approach involves (1) preparing AEMs without aryl-ether linkages in the polymer backbone and (2) developing different ionomeric binders for anode and cathode.



Question 1: Approach to performing the work

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

- This team is managed out of Los Alamos National Laboratory, but it includes three groups making alkaline ionomers, which are all being tested in parallel. The team members follow some guiding principles they have developed over the last five years regarding what sorts of chemical functional groups are likely to be stable or not stable in concentrated alkali. In particular, they seek to avoid aryl ethers, which they say will always be unstable in alkali. This approach is sensible and likely to lead to materials with superior properties. Their stability studies are done in such a way that relative stabilities cannot be easily assessed because they simply show that materials are stable under a particular set of conditions. It would be better to compare materials, or to use conditions that eventually lead to degradation, in order to discuss lifetimes and not just the presence or absence of decomposition. On the effect of organocation adsorption on redox reactions, the effect of cations on oxygen reduction reaction (ORR) kinetics is known, but the effect on hydrogen oxygen reduction (HOR) is unclear.
- The approach focuses on the critical barriers of backbone and cation stability in the presence of hydroxide ions. The principal investigator (PI) has a long history with AEM development and has narrowed the approach to a system that has a good chance of success.
- The approach to improve AEM stability by preparing AEMs without ether or electron-withdrawing groups in the polymer backbone in promising. Replacing benzyltrimethylammonium with alkylammonium has been done before, but it is still an effective means of improved stability. It is encouraging that a wide variety of relevant properties were measured and cost was considered in the down-selection, although selection criteria and factor weighting were not defined. The approach for electrode ionomer down-selection is also rational.
- The project is addressing key AEMFC challenges with a multi-faceted approach towards optimization of ionomers for each electrode independently in addition to the AEM.

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.

- Selected AEM poly(biphenyl alkylene (PBPA1+) has excellent ex situ thermal stability and reasonable conductivity. Swelling is still quite high. The films show reasonable mechanical properties (e.g., 100% elongation). The areal specific resistance (ASR) measured in fuel cell operating mode is also promising. The selected cathode ionomer with side chain shows improved performance, but there is still much improvement needed. The anode binder stability looks good, but the cathode binder is not stable in tetramethyl ammonium OH (TMAOH). Overall, the project has demonstrated excellent progress in a short time period.
- The team has made very good progress, having identified several ionomer systems with high alkaline stability and good ASR in preliminary MEA/single-cell tests. The work on organocation effects on ORR and HOR is good, but it needs a little more fleshing out. For example, the nature of the films on Pt is unclear and could use some clarification regarding thickness, nature of bonding, etc. It would be nice if there was some evidence besides the infrared (IR), which was described in only general terms. It not clear how the IR experiment distinguishes between signals from surface and bulk-solution species. There are several ways to make that distinction, and the PI should clarify how he did it. Regarding other approaches, perhaps some quartz crystal microbalance (QCM) studies would be helpful to determine how much material has deposited on the Pt. Film formation in water may not be indicative of what happens in an MEA. Cell tests with various combinations of membrane and electrode ionomer indicate progress, but a firm understanding of factors affecting cell behavior is still lacking. Perhaps an injection of modeling expertise would help.
- The PBPA+ approach demonstrated very good conductivity and durability. The side-chain poly(phenylene) (PP) ionomer for the cathode yielded significantly improved hydrogen/air performance over the benchmark ionomer. There was good integration of rotating disk electrode studies on ORR poisoning. Understanding the impact of tetramethyl ammonium (TMA) adsorption is key to understanding HOR deactivation.
- While the final goals have not been meet, significant progress has been shown toward meeting the resistance target of less than 0.1 Ω*cm^2 and the performance target of 0.6 V at 0.6 A/cm^2. Good progress has been made towards understanding AEM ionomer stability to hydroxide attack.

Question 3: Collaboration and coordination with other institutions

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

- This highly collaborative project brings together a well-matched team to provide a wide range of materials with high alkaline stability. The PI provides very good support for materials characterization that complements the synthesis expertise of his collaborators at Rensselaer Polytechnic Institute (RPI) and Sandia National Laboratories (SNL). The team members contributing catalysts have not yet played a significant role but presumably will do so as the project progresses.
- The project has a strong team with relevant capabilities. It is unclear which achievements are attributed to which partner organizations.
- The collaborations with RPI and SNL on polymer synthesis are good. It is unclear what role Argonne National Laboratory has had, as only standard Pt catalysts have been tested.
- The plan to include all the partners looks good. It is unclear if the partners have been actively contributing or if their roles will become more important in the future.

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 does an excellent job of meeting the Fuel Cell Technologies Office and DOE goals with respect to alkaline approaches to hydrogen fuel cells. It is not yet clear how AEMFC approaches will compete with polymer electrolyte membrane fuel cells (PEMFCs) for hydrogen-based energy conversion; AEMFC approaches are at a much earlier stage, and much is still unknown about the ultimate limits of AEMFCs. Research such as that being done in this project is needed to discover these limits.
- This project is well aligned with the DOE goals and is positioned to advance the understanding and state of the art of AEM membranes. The approach to develop different ionomers for anode and cathode is important for maximizing AEM MEA performance.
- It is unclear what potential impact AEMFCs will have commercially. The apparent need for high-Pt HOR electrode loadings to overcome sluggish alkaline kinetics, coupled with intrinsically lower OH conductivity, ultimately raises significant questions of relevance.
- It is unclear for what applications AEMFCs will be used, and the ultimate targets are not defined. It is highly unlikely that AEMs will ever be competitive with PEMFCs for automotive applications.

Question 5: Proposed future work

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

- The future work seems well aligned with the objectives. More MEA testing and in-cell durability testing to demonstrate that the progress made in out-of-cell testing can be translated to in-cell performance would be appreciated.
- The proposed future work seems reasonable, with the one caveat that the work with fluorinated materials is not well described.
- It is not clear why future work on perfluorinated anion-exchange ionomers is needed. The results on the hydrocarbon ionomers are encouraging, and the National Renewable Energy Laboratory (NREL) is already working on the perfluorinated ionomers. Future work does not include non-platinum-metal-group (PGM) catalysts. Ionomer selection may be sensitive to catalyst type, so binder selection would need to be repeated, especially considering catalyst interaction was a key criterion in the down-selection process. Work to further reduce membrane ASR is recommended to be competitive with PEMs.

Project strengths:

- The relatively wide range of ionomers with apparently good alkaline stability is a project strength.
- The project has a solid approach and excellent synthetic chemistry expertise. The project addresses all key performance and durability properties in the down-selection process.
- The project is addressing key AEMFC challenges with a multi-faceted approach toward optimization of ionomers for each electrode independently, in addition to the AEM.
- The project has a solid approach to address critical issues with AEM fuel cell ionomer development for membranes and electrodes. Significant expertise in the area of AEM development has led to the identification of viable ionomers.

Project weaknesses:

- Targets are not tied to any application. Even if targets are met, the technology will not compete with PEMFCs. There is no in situ durability testing planned.
- The approaches to low-PGM loading or non-PGM were not addressed. This project looks like an ionomer development project, but two of the partners are catalysis experts, and their contributions are not clear at this time.

Recommendations for additions/deletions to project scope:

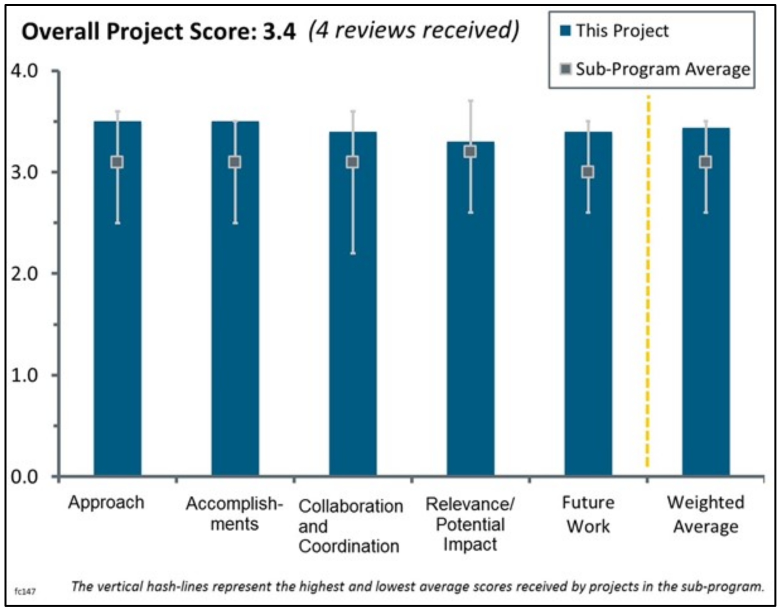
- The MEA/cell building and testing work could possibly benefit from a modeling contribution.
- Gas permeability should be measured rather than estimated from the chemical structure because of uncertainty about the accuracy of the estimate. The project should eliminate the perfluorinated ionomer work or at least ensure that it does not overlap with NREL’s work; the perfluorinated ionomer work is also likely to be more expensive. The project should continue to focus on ASR reduction and work on reducing swelling of membranes. Non-PGM catalysts should be considered in the binder selection process. In situ, non-steady-state durability tests should be run.

Project #FC-147: Advanced Ionomers and Membrane Electrode Assemblies for Alkaline Membrane Fuel Cells

Bryan Pivovar; National Renewable Energy Laboratory

Brief Summary of Project:

Alkaline membrane fuel cells (AMFCs) offer promise for improved performance and decreased cost. This project aims to develop novel perfluoro (PF) anion-exchange membranes (AEMs) with improved properties and stability; employ high-performance PF AEM materials in electrodes and as membranes in AMFCs; and apply models and diagnostics to AMFCs to determine and minimize losses (water management, electrocatalysis, and carbonate-related). Researchers will synthesize, characterize, and optimize alkaline exchange membranes and fuel cells for performance and durability.



Question 1: Approach to performing the work

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

- The principal investigator (PI) continues an approach from a previous project, seeking to modify pre-formed perfluorinated sulfonyl fluoride polymers with diamines with subsequent quaternization to make fluoropolymer anion exchangers. The approach is reasonable, and the work is quite tightly focused on barriers and goals. The inclusion of modeling in the present project is especially welcome because it brings key insights into some of the reasons for different levels of performance in early-stage membrane electrode assemblies (MEAs) and related devices.
- The project addresses the two main barriers for fuel cells: cost and durability. By looking at alkaline membrane fuel cells, which can enable platinum-group-metal (PGM)-free catalysis, the project addresses cost. The project addresses durability of AEMs by utilizing a stable perfluorinated backbone and by investigating methods to add stability to the pendant quaternary ammonium group and sulfonamide linkage. The modeling effort is being used to help guide the electrode and MEA design. The approach of utilizing perfluorinated backbone polymers to enhance water transport is interesting and should provide benefits, as water management is an issue in AMFCs. The perfluorinated backbone approach also complements other U.S. Department of Energy Office of Energy Efficiency and Renewable Energy AEM work that focuses on utilizing aromatic backbone polymers, providing a diverse portfolio.
- The project has a good balance of materials synthesis, characterization, and modeling. Perfluorinated AEMs and AMFCs may provide key ultimate benefits toward high-performing MEAs owing to higher water transport capabilities than hydrocarbon AEMs. Development of AEM models and AEM characterization techniques are immediately relevant to addressing issues.
- The approach is well defined and balanced between synthesis, characterization, and modeling.

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 PI has made excellent progress. In the past year, the PI has solved several longstanding problems that have plagued this approach, particularly in the area of completeness of conversion for adding amines to sulfonyl fluoride, and subsequently quaternizing the amines to make ammonium salts. As a result, he has in hand multi-gram quantities of ionomer of high quality, which has enabled quick and excellent progress in nearly all other project areas. Conductivity data are quite reasonable in comparison with acid systems. Stability data show problems with the first generation (Gen 1) PF AEM polymer, but the nature of the instability is clear, and the PI is well positioned to make changes that will greatly improve stability; his second-generation (Gen 2) polymer promises to be excellent. Progress on MEA work is very good and reveals a need for deeper understanding of transport losses in alkaline systems, which differ from those in acid systems. The accomplishments from modeling are excellent in such a short time. Diagnostics from CO stripping and hydrogen pumping are also excellent, obviously reflecting the fact that this project is a continuation of prior work with significant investment already in place for both synthesis and diagnostic studies.
- Excellent progress has been made toward development of the Gen 1 PF AEM polymer with good synthetic yield and high conductivity. Durability, however, is very poor. Development of improved durability in the Gen 2 AEM is promising, but conductivity and other properties were not disclosed.
- The project has made very good progress towards the overall target, especially from the MEA performance modeling point of view.
- The project has made good progress developing an alkaline exchange membrane and integrating it into an MEA. The Gen 1 PF AEM polymer performs better at the beginning of life than the Tokuyama membrane. The modeling efforts have identified the importance of anode flooding and water management in the MEA. The project has refined techniques to determine hydrogen oxidation reaction /hydrogen evolution reaction HOR/HER exchange current density.

Question 3: Collaboration and coordination with other institutions

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

- The team has excellent participants well known in the polymer design, characterization, and modeling of fuel cells.
- The team’s collaboration is very good, particularly with Lawrence Berkeley National Laboratory for modeling. Collaborations with Oak Ridge National Laboratory were in just one area, and work with Colorado School of Mines was not clear from the presentation, but both are reasonable and probably contributing important information either now or in the near future.
- Collaborations within the team are good. Membranes are being characterized and incorporated into the MEAs. Characterization and modeling feed back into the MEA and membrane preparation. 3M’s input has helped utilize the perfluorinated backbone. The project lead is collaborating with other AEM projects as evidenced by the workshop organized by the National Renewable Energy Laboratory and Los Alamos National Laboratory PIs of AEM projects.
- The team consists of recognized leaders in ionomer development and characterization.

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 one of the best projects for making progress in alkaline fuel cells. Alkaline systems are at a much earlier stage of development relative to polymer electrolyte membranes (PEMs), particularly with fluorinated ionomers, but work such as that being pursued in this project will rapidly help identify the similarities and differences between the two systems, which is needed to learn where the correct niche is for alkaline systems.

- The successfully finished project will have a substantial impact on implementation of novel AEMs as a viable alternative to conventional PEM fuel cells (PEMFCs).
- The impact of the AEM membrane work will be dependent on whether an effective PGM-free anode catalyst material can be developed or whether the anode and catalyst loadings can be reduced below those currently observed in PEMFC systems. With Tokuyama advertising their plans to stop supplying their AEM to developers, a membrane material with good performance and high enough stability to allow performance measurements is needed. This project could provide a baseline (or develop a material that could be a baseline with a small company manufacturing and supplying a large benefit to the field) for the AMFC community. A baseline material is needed to help develop catalysts and electrodes, and in this regard, an AEM that can be made at scale would have a large impact by allowing the research community to develop other materials while the membrane is being developed further.
- It is unclear whether AMFCs will ultimately achieve commercial relevance because of poor HOR kinetics and lower conductivity than PEMFCs. Performance under hydrogen/oxygen with high Pt loadings is similar to PEMFCs under hydrogen/air.

Question 5: Proposed future work

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

- The future work is relevant and addresses AEM issues. Variations in conditions for the model should provide useful data to guide the polymer chemistry and MEA design changes. Details about strategies (other than lengthening the side chain and getting rid of the sulfonamide linkage) for increasing stability need to be presented.
- The proposed future work is in good alignment with remaining challenges and barriers as well as with the overall goal of the project.
- The proposed future work is fully appropriate.

Project strengths:

- As far as the reviewer is aware, this is the only current work exploring tetrafluoroethylene/trifluorovinyl ether (TFE/TFVE)-based fluorinated systems for alkaline energy conversion. The project has made excellent progress and is bringing together a team well matched to project needs.
- The project team has good chemistry, and the team members have worked together. 3M’s partnership provides a wealth of perfluorinated polymer backbone chemistry.
- A project strength is the understanding of materials design and synthesis. The characterization methods proposed are well established for such types of materials.

Project weaknesses:

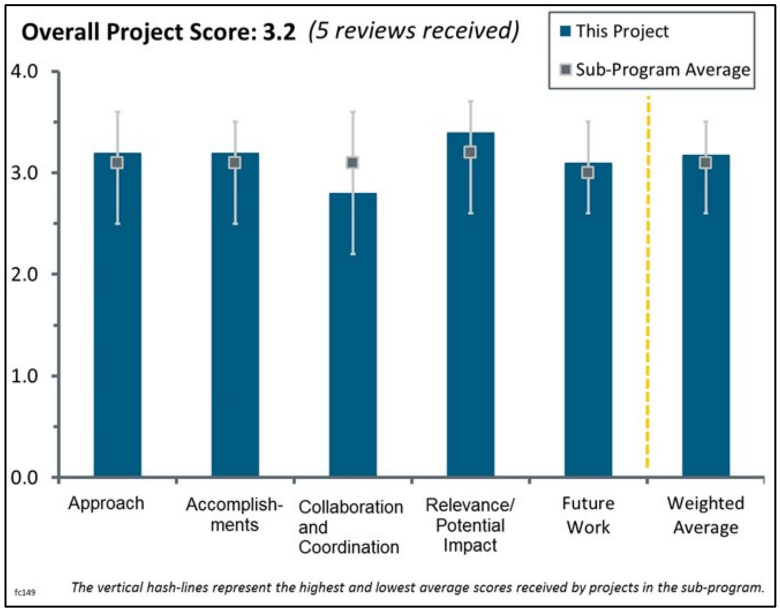
- AEMFC systems in general are not as well developed as PEMFC systems, so the ultimate limits of what can be done are not known. It may be that there are power limits, or durability limits, or other as-yet unidentified limits that will ultimately make AEM systems not competitive. This is a potential weakness of this general area, but it will take projects such as this one to determine whether AEM systems can be competitive.
- A project weakness is the lack of details on MEA fabrication.

Recommendations for additions/deletions to project scope:

- The project should expand the part on achievement of MEA performance using new types of ionomers and membranes. Overall MEA performance depends on many parameters and characteristics of the ionomer, and membranes are only part of optimizing the whole process.

Project #FC-149: Multiscale Modeling of Fuel Cell Membranes

Adam Weber; Lawrence Berkeley National Laboratory



Brief Summary of Project:

Despite broad use of ionomer membranes such as Nafion® in energy research, operando behavior prediction is unavailable. Understanding multi-ion transport in various ion-rich solvents would enable ionomer and system optimization. This project will help optimize and explore design criteria for transport in ion-conducting membranes across length scales in various environments. Researchers will use a novel multiscale modeling methodology to examine and detail controlling interactions for ion and solvent transport.

Question 1: Approach to performing the work

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

- Membrane performance is well defined in the model. It is a unique approach to take a network model to solve the membrane performance with nanoscale phenomena. If this approach can work (i.e., be validated), it can be used to simulate the aged membrane performance with structural changes. Modeling and characterization in this project seems to be duplicative of FC-PAD. The project scope should be distinguished from that of FC-PAD.
- This is a very ambitious project with a relatively small budget. The principal investigator (PI) is addressing, or plans to address, all of the key barriers. The approach of using multiscale models to bridge macro and nanoscale models may be required to model bulk membrane transport properties. To accurately model water uptake and transport, it is likely that long-range polymer motion will need to be included, which will be computationally intensive. It is also unclear how the historical dependence of the microstructure will be addressed. Addressing cation mobility (Ce, Fe, Co, Ni, etc.) should be very valuable, as very little is known about this mobility. The PI may have overreached a bit on the goals of this project, but it is hoped that he is successful.
- This approach is good and is needed. Exploration of design criteria for transport is an objective. The PI could discuss manufacturing constraints with the Fuel Cell Consortium for Performance and Durability (FC-PAD) and with membrane manufacturers before and during the work on developing the design criteria. This discussion is necessary because some design criteria might not be feasible for mass manufacturing.
-
- The multiscale modeling of the membrane seems to be a development of pore-network modeling of electrodes, so the approach seems logical. However, such simulations might be computationally intensive.

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 modeling results seem promising and compare well to the experimental results. It is uncertain whether these results can then be used to predict how a new structure could look.

- Lawrence Berkeley National Laboratory has set up the pore and polymer electrolyte membrane (PEM) networks, has run initial simulations for Nafion, and has been able to obtain tortuosity values by fitting conductivity data. There is still much to do, and they are halfway through the project. The project partners claim they are on track for modeling water flux through a Nafion membrane measured for four different relative humidities and three membrane thicknesses, but it is not clear that they will complete this modeling.
- The model is validated in macroscale membrane performance data (e.g., proton conductivity) and shows good fit. A question is how to measure (or define) the water content of the membrane in the empirical data.
- The conduction network modeling with the pore network is very good.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- It is good to collaborate with FC-PAD. The modeling and characterization of this project seem to be duplicative of FC-PAD. Membrane/ionomer suppliers should be included.
- There is some interaction with FC-PAD and other developers, but it is unclear whether a team exists for this continued project. It looks like the project needs some collaboration partners.
- Collaboration is limited to discussion only. The project may benefit from stronger collaboration with other FC-PAD members and academia leading in modeling efforts (both in the United States and outside the United States).
- It is unclear how collaborators are contributing to this project. There should be opportunities for the team to work with FC-PAD for both data collection and structural evaluation.
- More interactions with membrane manufacturers are recommended to address feasibility of design criteria for mass manufacturing.

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 structural base membrane performance model is highly useful for membrane development and design of the operational conditions of the fuel cell system. The model is effective for performance and robustness.
- The membrane is one of the important components for fuel cell systems affecting the balance-of-plant system. By defining the transport better, the membrane can be designed more effectively, so the overall system can be more economical.
- If the PI can accomplish what he has set out to do, this project will be of great value to membrane developers seeking to create an ideal membrane microstructure. There will also be great value in being able to model cation transport within a membrane electrode assembly.
- This project has been used to model various aspects of fuel cell performance and electrode interactions. However, the relevance of this year's work is unclear. Even assuming the project is wildly successful in being able to model the membrane and even recommending a great structure, there is no evident impact on the two biggest issues raised by DOE for fuel cell commercialization.

Question 5: Proposed future work

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

- Most of the future work planned should be very valuable, albeit unlikely to be completed within the timeframe of the project. This valuable future work includes expanding the model to include solvent uptake and transport, making the model dynamic, and expanding the model to include transport of impurities and contaminants such as cations.
- Proposed future work looks ambitious, but it is unclear whether the transient responses can be predicted well. It might help to focus on steady-state response and understanding material interactions. Further, it

might help if this work can develop this model in a highly parallel-processing computing environment (e.g., NVIDIA CUDA®).

- Dynamic modeling is challenging and a very good idea.
- Exploration of design criteria for transport is an objective. The PI could discuss manufacturing constraints with FC-PAD and with membrane manufacturers before and during the work on developing the design criteria. This is necessary because some design criteria might not be feasible for mass manufacturing.

Project strengths:

- The project strengths include using the multiscale modeling approach and the outstanding modeling and microscale structural analysis capabilities of the team at Lawrence Berkeley National Laboratory.
- The project should leverage the partner's knowledge of mechanistic understanding of membrane performance.
- The brilliant PI and his team's capabilities are strengths.

Project weaknesses:

- There is little evidence of collaboration. It is unclear whether the learnings will apply to membranes made from other ionomers. There is not enough time to complete the future work.
- The lack of strong collaboration and the lack of model validation with experimental data are weaknesses.
- There are no experimental interactions with collaborators.
- The project objectives are similar to FC-PAD's and might be duplicative.

Recommendations for additions/deletions to project scope:

- Perfluorosulfonic acid (PFSA) membranes should be included with other side chains (e.g., by 3M, Aquivion) as well as hydrocarbon ionomers. The PI claims that the main benefit of this project is that it can be used to determine an "ideal" structure for optimum conductivity. For this to be true, the model must account for different polymer structures with different chain mobilities. The PI should also address gas (H_2, O_2, N_2) permeance within the model.
- Project objectives are similar to FC-PAD's and may be duplicative. Performance degradation of aged membranes can be analyzed with this project approach (i.e., structure-based membrane performance model).
- If possible, the PI should develop this model in an open-source, highly parallel, multithreaded computing environment.

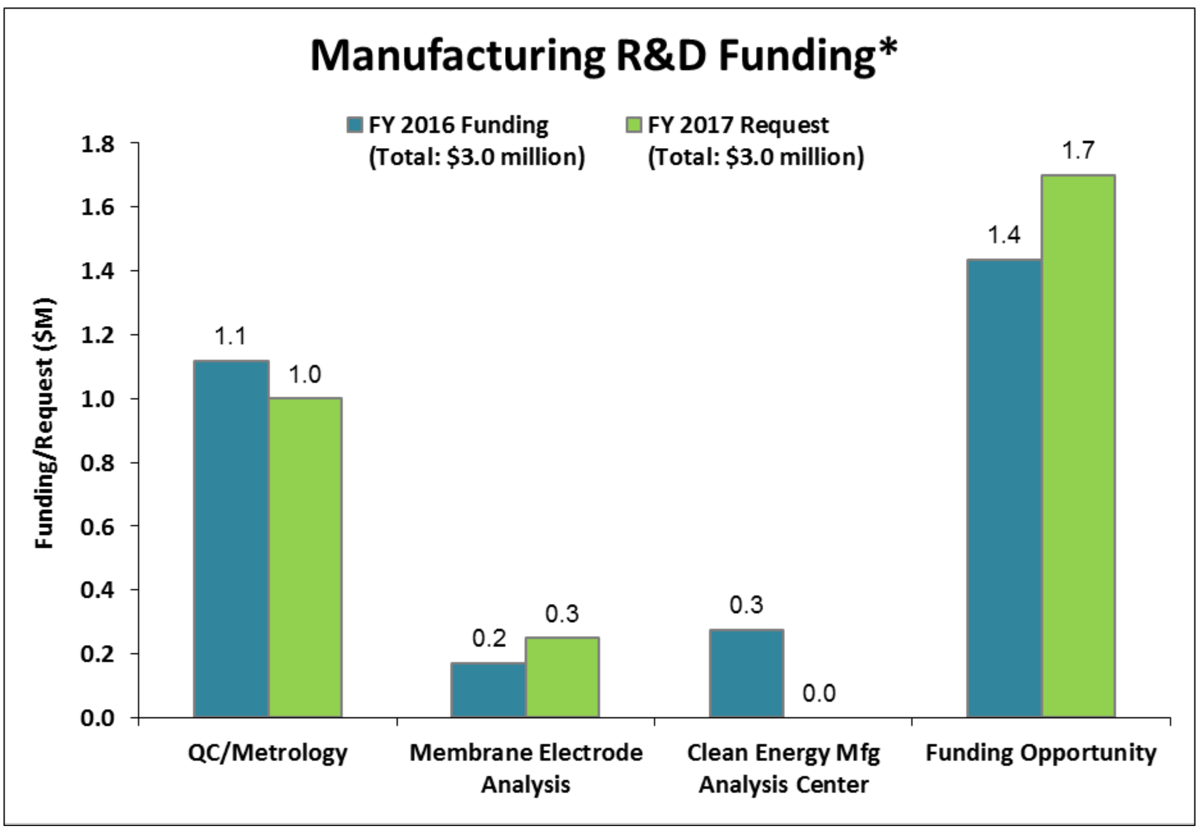
2016 — Manufacturing Research and Development (R&D) Summary of Annual Merit Review of the Manufacturing R&D Program

Summary of Reviewer Comments on the Manufacturing R&D Program:

In general, the reviewers felt that there was great potential in breaking through the manufacturing R&D barriers and challenges that the U.S. Department of Energy's Hydrogen and Fuel Cells Program faces. They commended the program for having projects that are well structured, effective, and significant in reaching goals and milestones. In particular, they recognized the Hydrogen Fuel Cell (HFC) Nexus website, which was established to provide product information on hydrogen and fuel cell components and systems to the entire community, thereby enhancing domestic supply chains and enabling further widespread commercialization of hydrogen and fuel cells. One key recommendation was further exploration of the differing assessments of manufacturing readiness by original equipment manufacturers and Tier 1 suppliers.

Manufacturing R&D Funding:

Funding for the Manufacturing R&D program was $3 million for fiscal year (FY) 2016, and $3 million was requested for FY 2017. In FY 2016, funding is provided to continue existing Manufacturing R&D projects for quality control (QC)/metrology and membrane electrode analysis in addition to projects from funding opportunities. The FY 2017 request-level funding will continue existing Manufacturing R&D projects and provide funding for new projects 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:

This year, 5 projects funded by the Manufacturing R&D program were presented and reviewed. The reviewers' scores ranged from 2.8 to 3.5.

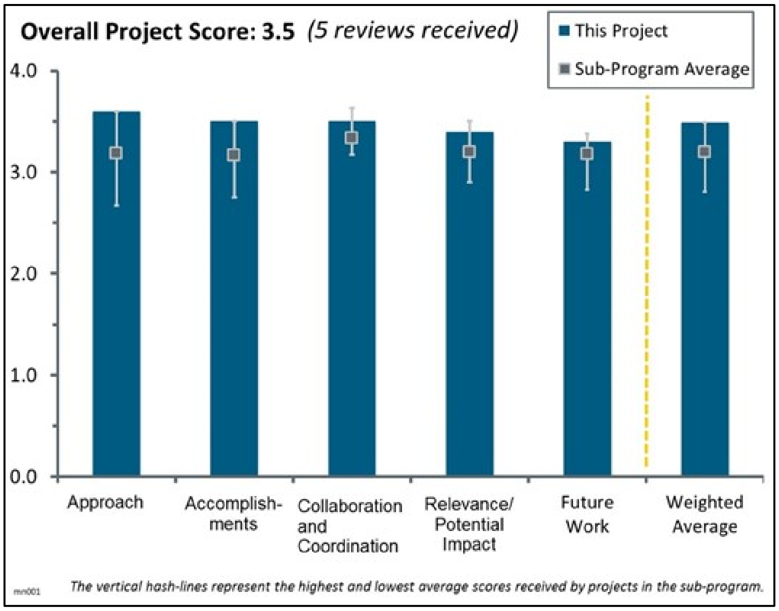
QC/Metrology: One project was reviewed in the area of QC/metrology, receiving a score of 3.5. Reviewers stated that the approach was very good and that there was little that can be improved upon. They also noted that the project was well-designed to provide quality information on various control technologies. Reviewers stated that the project team has a formidable collection of facilities and people with the highly specific skills required by the task; they see little room to improve the team's collaboration. The reviewers suggested providing a summary chart of inspection techniques, including information such as the target defect or variable, required detection limits, required scanning or detection rate, state of development, and state of adoption.

Analysis: Four projects were reviewed, with three projects receiving a score over 3.0 and one project receiving a score of 2.8. The reviewers were impressed by the highest-rated project for the significant progress in establishing a website that provided product information on hydrogen and fuel cell components and systems to the fuel cell community. However, the reviewers expressed concern about maintaining and updating the website once federal funding for the project ends. The reviewers felt that the lowest-scoring project provided an interesting approach to creating and supporting future regional technical exchange centers for manufacturing. However, some reviewers questioned the importance of these centers and the impact on the manufacturing and industrial needs for hydrogen and fuel cells. They noted that the project needed to improve its focus and clearly track the project's impact by further collecting and analyzing other technical exchanges.

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 (1) understand quality control needs from industry partners and forums, (2) develop diagnostics by using modeling to guide development and in situ testing to understand the effects of defects, (3) validate diagnostics in-line, and (4) transfer technology to industry partners.

Question 1: Approach to performing the work

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

- The approach is very good, and there is little that can be improved upon. Convincing industry to open up and adopt project outputs on manufacturing is a difficult task. There is a consistent and concerted effort to engage industry.
- The project is well designed to evaluate various quality control technologies while working with industry, laboratories, and academia.
- Correlating defect parameters with cell/stack performance/degradation is very useful. Identification of dangerous defects would reduce inefficiency and waste related to building stacks with faulty components. Two different complementary approaches to defect detection are being pursued. A table comparing defect parameter ranges with detection capabilities would clarify the status of the project. The evolution of defects during operation needs more consideration.
- The approach slide does not offer much detail, but the first impression is that this is an excellent approach based on the diagnostics and validation. The National Renewable Energy Laboratory (NREL) should continue getting broad industry input and pushing technology transfer. NREL should also consider hosting a workshop for every interested supplier and integrator.
- The NREL work is valuable and interesting. The studies presented at the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program Annual Merit Review focused on one barrier—the membrane quality. There are many other barriers, and some of them are equally valuable and interesting. A funded cooperative research and development agreement (CRADA) is in place that involves that emphasis, which is good.

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.

- Through-plane reactive excitation efforts are showing good progress and potential utility. Technology transfer efforts are improving.
- The progress shown is good.
- Feasibility of the techniques has been demonstrated. Developing a table comparing thickness and defect parameter ranges and detection capabilities would clarify the status of the project. The go/no-go gate has been passed, but the quantitative results relative to significant defects are difficult to decipher.

- It is difficult to evaluate the accomplishments and progress. Clearly, technical results did lead to the "discovery" of polymer film defects, and that was accomplished using several different approaches. Also, it was clear that results were of interest to at least one original equipment manufacturer. Even so, the DOE goals are to enable a fuel cell system, and there are many more issues that were not mentioned that are beyond the narrow focus of this activity.

Question 3: Collaboration and coordination with other institutions

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

- The team has responded to reviewer comments and continued to add and rotate collaborations as needed. There is little room for improvement here.
- General Motors lends reality and credibility to the project. Technology transfer to Mainstream Engineering facilitates scale-up, adoption, and commercialization of the techniques. Other team members provide modeling, fabrication, and cell testing capabilities. The team would be enhanced by the addition of cell component manufacturers.
- The project incorporates useful contributions from industry, national laboratory, and academic partners.
- This NREL activity collaborates with a national laboratory and is integrated with the NREL National Center for Photovoltaics and a few universities. This is considerable "horse power." The project partners have excellent facilities with highly competent staff. The interactions with 3M, a company with a long history of coating plastic (e.g., Scotch Tape®), are sensible and useful. However, the exact role of the partners was not very clear.
- There is broad involvement. There may be room to identify and include custom membrane electrode assembly (MEA) suppliers.

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.

- This is a quality control activity, which is an essential part of firming up the fuel cell manufacturing base. Durability in fuel cells is a complex issue with many pathways that can lead to early failure. Robust assessment tools are essential and are especially critical in the early phases of commercialization.
- Development of in-line, real-time diagnostics for cell component quality is clearly relevant to improving performance and durability and reducing cost.
- NREL is tackling a variety of manufacturing goals.
- NREL has done excellent technical work. The pathway to industry adoption of the manufacturing tools needs more explanation. It is not clear how the resulting improvements in manufacturing bring down the costs or performance.

Question 5: Proposed future work

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

- The future work appeared to be a continuation of the current activities. There is a "target of opportunity" approach, identifying technology target areas and then addressing the issues found there. This is a smart way to do this kind of support technology.
- The future work logically continues the approach. The correlation between relevant defects and cell behavior will be elucidated, and modeling will continue. Platinum-group-metal (PGM)-free catalysts will be explored.
- Future work has been planned appropriately; most important, the effects of defects will be included.
- Future work should also include an assessment of benefits and an approach to getting industry adoption. Also, the technology transfer should have a more detailed plan and include more than collaborators.

Project strengths:

- The project is well run, the principal investigator clearly makes an effort to garner end user input, and the inclusion of project partners is managed well (i.e., included where needed). Technical progress is continuous, and technology transfer is moving forward.
- The formidable collection of facilities and people at NREL is the strength of this project. The tasks require highly specific skills, and few organizations in the United States could address this as well as NREL.
- This project is well designed and executed with pertinent contributions from collaborators.
- The project has great research and development and tools.

Project weaknesses:

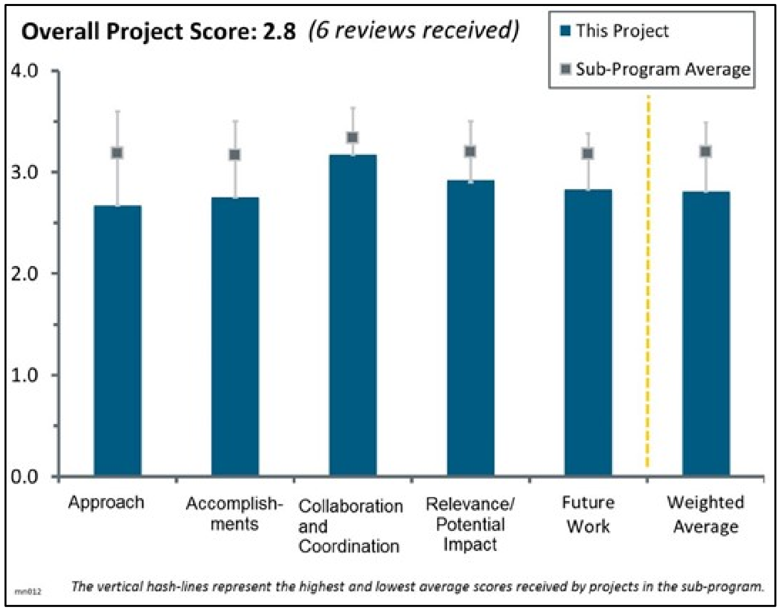
- The team has done a very good job. The only weakness that may exist is the potential for some level of pushing technology, rather than it being pulled from industry as a need. However, this weakness is a stretch. The project has been run very well; every effort seems to be focused on industry needs with periodic validation.
- History shows that the fuel cell stack is quite reliable. Certainly bad things happen when the stack is run under adverse conditions. Hydrogen starvation in the anode is just one example, but a telling one. Like all other “engines,” the fuel cell stack can survive only if operated within a set of appropriate operational parameters. Just like running an internal combustion engine at rotational speeds above the “red line,” a fuel cell stack cannot be expected to work through any number of untoward events.
    - History also teaches that 80% of all fuel cell system issues result from misbehavior of balance-of-plant (BOP) components. Hence, durability is more a function of the BOP than the stack, yet NREL is focusing on the stack. This focus is necessary, especially because industry is so bewildered by that complexity; however, the project would be stronger if some number of BOP and control strategies were included in its emphasis.
    - It is also obvious that the final system design of a fuel cell engine is still to be determined. What DOE has “invented” may have little in common with the future fuel cell electric vehicle power plant. NREL should be chartered to spend some time on alternative system designs. It is important that the power electronics are included in the system—this is a fuel cell–battery hybrid. Consequently, the fuel cell probably does not have to do much load following. It does not have to start up promptly or shut down immediately. It might work intermittently, similar to how photovoltaic arrays operate. It would be unwise to limit the engineering to the problems with the alleged system instead of considering what advances could be made to nullify the technical problems of the existing designs.

Recommendations for additions/deletions to project scope:

- It would be helpful to see a summary chart of all the methods being developed and considered for development under this project. This summary could include brief information on the target defect or variable, required detection limits, required scanning or detection rate, state of development, and state of adoption. Summarizing the progress in a chart would make it easy for an end user to scan to see if NREL has developed a solution of interest. In addition, the inclusion of potential future methods and defects to be investigated may garner input from end users as to the usefulness.
- The project should conduct industry workshops to distribute this good work to a broader audience beyond the collaborators. The pathway to industry deployment, including costs and benefits, should be assessed.
- The scope should be expanded to the entire fuel cell system, inverter and all. The emphasis should be on the question of what operating parameters can be optimized to keep the fuel cell stack healthy. Also, the fuel cell stack should be considered as a heterogeneous catalytic chemical reactor, and those many, well-established tricks in chemical engineering known to enhance performance and durability need to be evaluated for the fuel cell system case. One of the several “laws” in early times (when the polymer electrolyte membrane technology was a solid polymer electrolyte) was “never put or admit air into the anode.” That could still be an appropriate design consideration.

Project #MN-012: Clean Energy Supply Chain and Manufacturing Competitiveness Analysis for Hydrogen and Fuel Cell Technologies

Pat Valente; Ohio Fuel Cell Coalition



Brief Summary of Project:

The objectives of this project include the following: (1) establishing regional Technical Exchange Centers to increase communication between original equipment manufacturers (OEMs) and hydrogen and fuel cell component and subsystem suppliers; (2) establishing a readily web-accessible database containing inputs from suppliers and OEMs along with a supplier contact list; (3) standardizing component and subsystem component specifications; and (4) developing strategies for lowering cost, increasing performance, and improving durability of components and subsystem components.

Question 1: Approach to performing the work

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

- The approach for executing the project's efforts is generally good. A Gantt chart (or similar) would be helpful for the approach and milestones. Consideration should be given to how best to measure the effectiveness or outcomes of the efforts.
- The project stresses creation and support of a "supply chain," an interactive group of companies that produce and sell components for fuel cell systems, with an obvious bias toward those that support fuel cell electric vehicles.
    - The approach is to assemble a group of companies with that common interest and provide information to improve cooperation and coordination among the suppliers. Certainly, creation of a supply chain is an admirable objective. Even so, that task is difficult. Some of the interested companies are competitors. Some may not be prepared for the complexity of this market.
    - There is no apparent effort to match specific, quality companies with potential buyers of the hardware that those assemblers might need. Although some necessary components are not complex (e.g., electrical connectors), other essential fuel cell components may take considerable investment before marketing is possible. An example might be the Eaton Scroll Compressor.
    - The project might include publishing routes in which public money could be available for product development and product improvement.
- The "Approach" slide contained milestones. It would be better if the slide presented the high-level goals, metrics for success, and strategy to get there. The slide should include the success criteria for the regional centers. It is not clear what they will they look like.
- The approach begins with some potential, but it has failed to produce any tangible outcome to date. Holding industry collaboration workshops can produce valuable information and relationships. However, there is little to no indication that anything has been garnered and disseminated from the work to date. Perhaps the groundwork has been laid during year one and future work will yield results.
- The approach of this project was not as focused as the other projects. This project had four completely different objectives while others were far more focused with mainly one objective. The strength of this project is the establishment of technical exchange centers to improve communication. The project should use this strength and leverage other projects for the website and database tasks.

- The regional technical exchange centers are virtual entities that exist only online, so it is not clear why they have regional names; why we need regional centers rather than one online center; and how these centers contribute in developing communications between manufacturers and suppliers, which is one of the key barriers listed.

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.

- The project has done a good job in establishing a national database center and arranging supply chain exchange and partnership development forums. These address two key barriers: (1) the lack of a national database and (2) the lack of communication between suppliers and manufacturers. However, the importance of regional exchange centers is perhaps overstated.
- The accomplishments for the first objective are excellent, with the October 22 and May 5 events and plans for the next event in Ohio. The plans for accomplishments for the second objective are credible, with the database being designed to be compatible with MN-013, the Virginia Clean Cities project. However, there was little presented on the other two objectives. There was no discussion of the status of the working groups, who the members are, and how the project plans to identify pathways to standardization of components and subsystems. It was also unclear what is being done to lower cost and increase performance and durability of components and subsystems.
- The project has made acceptable progress toward its goals. Better correlation of the project's goals to DOE goals is needed.
- Some companies are showing interest. Even so, the largest fuel cell trade show is the Fuel Cell Expo in Tokyo, which attracts about 30,000 individuals. One has to start somewhere; however, it seems that interest is modest.
- The project has executed tasks to improve communication between OEMs and suppliers. However, there is no indication of progress measured against performance indicators. It is not clear what the performance indicators are. To date, none of the milestones listed on slide 7 has been completed, even though some were scheduled for the first and second quarters of the project. It is not clear if there were unforeseen hurdles; the presentation does not indicate so. The project has failed to even produce a brochure within the first nine months of operation.
- More time is needed to assess accomplishments and progress.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration with MN-013, Virginia Clean Cities, is excellent. The project outreach to suppliers and OEMs is great. Collaboration with MN-014, GLWN, has potential to benefit both projects. It would also be informative to see the budget distribution between the various collaborators/partners.
- The project appears to include collaborations across multiple organizations and has organized industry-wide sessions.
- Partners have appropriate breadth and depth of experience to support this project.
- There is great collaboration, at least as in regard to the project participants.
- The principal investigator (PI) seems connected with others who are doing similar activities in other sections of the United States. The lack of any interactions with universities is puzzling. It would make sense to tie in with engineering programs that emphasize manufacturing technology. The thrust seems centered on the fuel cell stack, which is fine. However, that stack involves the assembling of a large number of replicated parts, and the most likely on-ramp may be to break-in with some necessary component of the larger product.

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 key barriers required for manufacturing cost-effective fuel cells outside the technical research and development issues.
- The potential for this project to have an impact is high. However, the project needs to do a better job of tracking this impact. Having the technical exchanges is excellent. However, the project needs to identify outcomes from these exchanges that are benefiting the Hydrogen and Fuel Cells Program (the Program). The working groups could have high relevance to the Program if they have the right people in them and if they can identify ways to lower cost while improving or maintaining performance and durability.
- DOE appropriately is encouraging a build-out of the manufacturing base of clean technology, such as fuel cells, which could be a primary plank of the global 21st century economy. Getting a piece of the action involves building up a credible technical manufacturing base. Creating supply chains is one approach.
    - There seems little question that, globally, clean technology hardware is a huge business—the value of photovoltaics and wind power equipment is obvious. The real concern is market share.
- This project should be very relevant, but a set of better-defined success criteria would be helpful.
- The project intent appears to align with the Program objectives. However, it difficult to believe the project will yield any value if progress and the quality of work are not improved.
- The project needs to better express its relevance with respect to the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan's barriers.

Question 5: Proposed future work

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

- The future work is apparently a continuation of the current activities. There is a plan for a brochure, which is good. In addition, the plan includes building a database (perhaps contributing to the existing database). The PI certainly understands the issues and is trying hard to address a rather tough situation.
- The project has a well-structured plan to address key challenges for the stated barriers.
- The seven tasks listed in the future work are very good and appropriate.
- Future work seems reasonably well planned. A Gantt chart (or similar) would be helpful for this.
- It would be beneficial to see more detail regarding future work.
- There is little detail provided to indicate that the future work will have substance. More detail would be helpful.

Project strengths:

- The project has well-qualified people, has organized and well-developed plans, and addresses key barriers in manufacturing cost-effective fuel cells.
- Setting up technical exchanges is great. Coordination with MN-013, Virginia Clean Cities, on the database website integration is good.
- Broad involvement is a project strength.
- Pat Valente is the primary individual involved with this activity. Clearly, he is the "strength."

Project weaknesses:

- The funding seems adequate to support the work. However, the funding is not sufficient to be effective.
- The project may be too ambitious, particularly in the development of standards for parts and subsystem components. Also, there is no well-defined performance matrix to evaluate progress. For example, it is not clear how many people are using the database and what its impact is. For each of the tasks, a performance matrix should be defined for the future years.

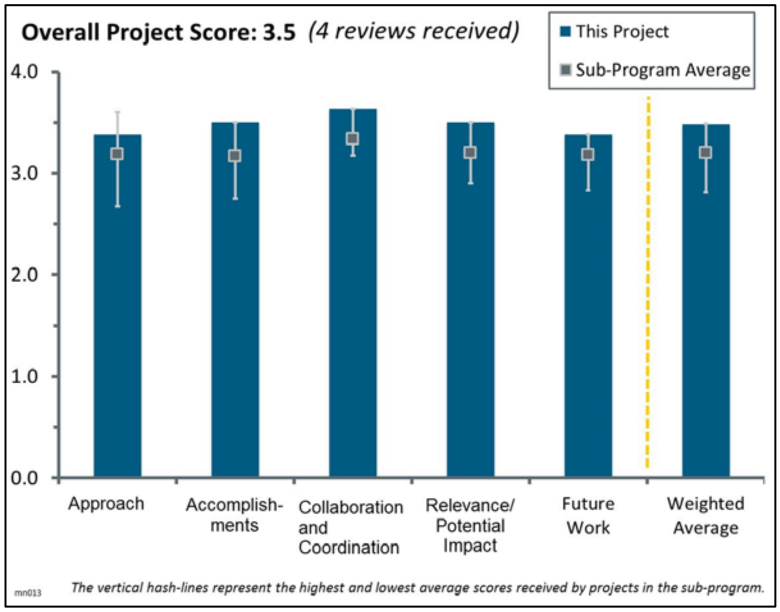
- Few details were presented on the working group other than in the approach. This part of the project needs significant attention to achieve several of the project goals.
- The presentation should have provided more detail and better addressed the scoring criteria. There was no definition of success criteria.
- The project lacks detail and substance. The inability to produce a brochure within nine months of the project start is one example. It is not clear what the “creation of four regional technical exchange centers” means. It seems like these centers already existed, so it is unclear what work was actually performed. The project needs to be more effectively managed to produce value.

Recommendations for additions/deletions to project scope:

- The data collected from the technical exchanges need to be carefully analyzed, and coordination with MN-014, GLWN, will help achieve DOE goals better.
- The “standardizing component and subsystem specifications” scope should be revisited. It is not clear what this really means or what it involves. At the least, this scope should be much better explained and defined.
- The issue is like matchmaking. Success will come when the PI learns of an opportunity and then contacts the company that is competent and interested in that business and the economic starts. Within small companies, the real value is usually people. One task might involve assembling a “directory” of interested companies, which includes the biographies of key employees, and then shopping that around the assembler community.
- Adding to the project scope is not recommended.

Project #MN-013: Fuel Cell and Hydrogen Opportunity Center

Alleyn Harned; Virginia Clean Cities at James Madison University



Brief Summary of Project:

The project aims to facilitate the widespread commercialization of hydrogen and fuel cell technologies by expanding the domestic supply chain of hydrogen components and systems. The Fuel Cell and Hydrogen Opportunity Center is building and populating a comprehensive communications database and using an aggressive outreach campaign to drive U.S. companies to the database website.

Question 1: Approach to performing the work

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

- The approach is very good. The team is close to launching the website and seems to have done a good job of creating a useful tool for the community.
- The plan to accomplish this work is well laid out, and significant progress has been made.
- The task focuses on building an open-source database that covers the global fuel cell industry and others “interested in alternative fuels.” Wisely, the task began by acquiring an existing database (written under the direction of Robert Rose), which is a good endowment for building a useful tool that identifies industrial players. A new website has been initiated.
    - The task intends to seek and identify “gaps” and to promote meaningful cooperation between organizations, as a step in developing competitive U.S. industrial activity.
- The project has a good approach. The team is doing the website first. However, details are lacking on the information about the database other than the number of companies. It is not clear what exact information is in this database or what value it adds to original equipment manufacturers (OEMs) and suppliers. The website and database can greatly benefit from coordination with MN-012, Ohio Fuel Cell Coalition (OFCC), and MN-014, GLWN.

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 team has made excellent progress with the website. The team is meeting aggressive deadlines, with the website set for hard launch in July.
- The team is clearly showing progress toward objectives and completing tasks.
- The work is on track according to the work plan. The website coming online will be a major milestone.
- Many tasks are just now getting traction. This sort of activity, which is certainly an essential activity, is somewhat similar to planting seeds and hoping that important shoots will emerge and they will be apparent. However, both the appearance of the shoot and the place where the shoots appear are difficult to predict. Today it is far too early to understand exactly what has been accomplished. Certainly, the principal investigator appears keen, focused, and intelligent. His plan is formulated, and the team is on the field.

Question 3: Collaboration and coordination with other institutions

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

- The task is housed at a university and integrated with Virginia's statewide focus on clean cities, i.e., cities with significant technology deployed to mitigate air and water pollution. There is strength in that collaboration because it brings together a collection of many individuals and educational, governmental, and private industries.
    - A close tie to Birch Studios, which has people well versed in web-based products, is a strong asset.
    - Although there was no indication that fuel cell electric vehicles will be a primary focus, there was a clear indication that clean cars, such as fuel cell cars, need to be on the radar. However, other technologies can play equally well. It may prove difficult to break into a technology dominated by Asian automakers, companies that tend to have a well-organized and protected set of parts suppliers. Other markets might be more appropriate for initial thrusts into commercialization.
- There is cooperation between the state-funded Virginia Clean Cities, James Madison University, Birch Studio (an experienced web-based site developer), and the Breakthrough Technologies Institute. It will be necessary to incorporate industrial organizations, which should happen as the task progresses.
- The project is reaching out to the suppliers and manufacturers and seems to be connecting with the correct groups to be successful.
- Results indicate the team is effective at collaborating so far. The more important step of garnering collaboration from the broader community will be the real measure of success.
- Coordination with MN-012, OFCC, and MN-014, GLWN, needs to be improved. The presenter did excellent work in inviting the audience to help collaborate in making the website a 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 **3.5** for its relevance/potential impact.

- If the project is successful at garnering input from the hydrogen and fuel cell community, then this will be a successful and valuable project. To date, the team has laid the groundwork to begin receiving such input.
- The fuel cell industry needs this information, and the way it is being presented on this website should fill the void.
- The impact of the website can be high. However, care needs to be given to the type of information that goes into it and how it helps DOE meet its targets.
- DOE and others are fully cognizant that fuel cells are "happening" globally, and that they represent a transformational technology. There is keen interest in getting the United States and U.S. companies involved in these new products. This project is funded to make progress in developing a strong industrial base in fuel cell technology.
    - The potential impact could be significant. Even so, Virginia Clean Cities cannot be a primary actor, but rather a catalyst that accelerates the process.

Question 5: Proposed future work

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

- This is the first year of the project. The plans for the website seem reasonable, and methods to improve the website each year have been presented.
- The website portion is excellent. However, details are lacking with respect to the database.
- The project is just starting. There is an acceptable plan, and the tasks are described. Some progress is apparent.

- The principal investigator should create and include metrics of success. These metrics will help guide the project to make adjustments where needed to provide value to the community and contribute to the Hydrogen and Fuel Cells Program goals.

Project strengths:

- Collecting this information and combining it on a single website will be very useful to the fuel cell community.
    - There is a strong design team and plan for developing the website.
    - The matchmaking interface will be very useful.
- The project has executed on first-year tasks and appears to be moving forward. The team should continue with its current approach; it seems to be working.
- The web portal launch is excellent.
- The PI seems essential for project success. Much will depend on his skill and luck. To be sure, success will involve those with access to capital and markets.

Project weaknesses:

- It is not clear what metrics are being used to determine project success.
- OEM and supplier interactions and database content were not clear.
- The task involves overcoming some serious competition. The electrical utilities are not keen on facing price competition from alternative energy sources. The oil companies are not keen on seeing no market for their reserves, so those on this proposed task will be challenged. This task is large, and the budget is meager.
    - There is no clear initial focus and no description of what the first commercial targets are. There is no apparent effort to identify specific products (fuel cell system components) that might be early commercial markets. There is no apparent focus on building a story about clean air/water health benefits, etc.
    - The principal investigator did not mention James Madison University involvement or ways that he might tap into the enthusiasm of students to promote and address these new technologies.

Recommendations for additions/deletions to project scope:

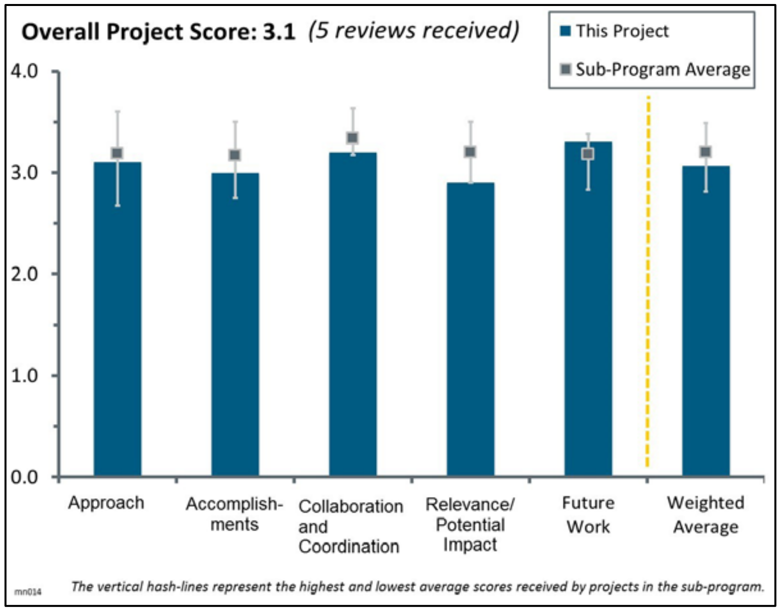
- Activities that can be done are started. A team is in place. This activity should be given some time to develop. The team should just stay on course, work hard, and seek partnerships as possible.
- The inclusion of data or charts showing the number of database entries and usage would be helpful to validate the acceptance of tools provided by the project. It would also help to ensure that information is reaching an appropriate audience and to guide the project in case adjustments are needed.
- The database needs more effort (than entering data from a questionnaire) and better coordination with MN-012, OFCC, and MN-014, GLWN.
- It is not clear how this website will be maintained after the DOE funding is complete.
    - It is not clear how the supplier information will be vetted and selected to be a part of the website.
    - It is unclear how suppliers will be monitored and removed if they are no longer supplying to the industry. These actions will all require time and effort that may be cost-prohibitive if no revenue stream is generated.

Project #MN-014: U.S. Clean Energy Hydrogen and Fuel Cell Technologies: A Competitiveness Analysis

Patrick Fullenkamp; GLWN – Westside Industrial Retention & Expansion Network

Brief Summary of Project:

This project falls under the Clean Energy Manufacturing Initiative (CEMI) mission to increase domestic manufacture of clean energy products and increase energy productivity. Competitiveness is driven largely by cost, so this project is examining current and projected costs, supply chain evolution, and global trade flows of clean energy hydrogen and fuel cell technologies. Project results will help CEMI identify strategic investments, identify technology areas for research and development (R&D) investment, and lay out a prospective future supply chain.



Question 1: Approach to performing the work

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

- The approach for this project is well structured and should materially help address identified barriers.
- The project is clearly well run, and the approach has been effective at moving the project toward its goals.
- Original equipment manufacturer (OEM) interviews are an excellent place to start. The assumptions in slides 9 and 10 may not be realistic. It is unclear whether OEMs have expressed interest in buying entire fuel cell systems and balance-of-plant (BOP) from Tier 1 suppliers, given the strong fuel cell research/patent portfolio and proprietary designs involved. This is akin to buying the car's engine from a Tier 1 supplier. It is not clear whether OEMs are speaking with one voice or how one would capture differing opinions and quantify them.
- This task is funded through CEMI, and the thrust is to address the supply chain necessary and appropriate for building and assembling products that flow from that emphasis on clean energy.
    - The thrust assumes competitiveness is driven largely by cost and therefore uses cost as a success metric. The task works to support the identification of high-value strategic investment, the design of a future supply chain, and technology areas for future technology investment.
    - There seems little focus on the "on-the-ground" current status. Major industrial firms in the global marketplace are delivering products, and the firms could accomplish that only if a "supply chain" exists for their manufacturing base. It seems useful to fully understand who is manufacturing what, and what current costs are.
    - The other missing step involves specifications. Certainly other manufacturers are building products to meet specifications. Because "fuel cell durability" is a serious concern, quality control of components needs to be a continued focus. Specifications are mentioned, indeed. However, the task of obtaining such information is not well described.
- The approach has not been clearly communicated in terms of generating a competitiveness analysis that is consistent in methodology with other competitiveness analyses done for the Office of Energy Efficiency and Renewable Energy (EERE),. It is not clear how the tasks described by the presenter contribute to an overall methodology.
    - It seems that the project so far has replicated data or analyses from other projects (e.g., the cost analysis by Strategic Analysis, Inc., [SA] and E4tech's commercial market analysis).

  - Stated outcomes are aiding DOE in identifying strategic investments and R&D investments, but it is not clear how this project will contribute to those outcomes beyond existing activities such as manufacturing cost analyses and market reports.

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.

- Overall progress toward goals has been good. The information gathered regarding manufacturing readiness is particularly interesting.
- The OEM surveys have gathered some insightful information, though there were some conflicts between the OEM and Tier 1 feedback that could have been explored further, e.g., the OEM "No" and Tier 1 "Yes" on membrane electrode assembly (MEA) #3.
  - There was agreement on the storage cost projections, shown on slide 12, attributed to the industry being more mature, yet on the survey, neither the OEMs nor the Tier 1 suppliers thought the technology or manufacturing was ready for production >1,000 units/year. This should be explored.
  - For the supply chain evolution, the Prius comparison seems useful, though there is no mention of the obvious difference between the Prius and fuel cell electric vehicles (FCEVs), namely infrastructure. It seems like the lack of support for infrastructure development could cause the evolution pathway for FCEVs to be significantly different.
  - The predicted evolution for MEAs, in which the OEMs will eventually have Tier 1 suppliers make the entire fuel cell power system, seems likely. It is not clear, however, how these evolution analyses contribute to the competitiveness analysis or what this means for U.S. MEA suppliers such as 3M or, potentially, Gore.
- The project is moving forward toward its goals. Progress toward DOE goals will be better known once the project reaches later milestones as data are rolled up with conclusions.
  - What the performance indicators are—how the project measures success—is not clear.
- There has been good progress on data collection. More information needs to be provided in slide 11. Just the ratio of the highest to lowest OEM estimate might be misleading. More robust statistical analysis is necessary to draw conclusions from these data.
- The principal investigator (PI) assumes that the "OEM" will build the fuel cell stack, an assignment similar to the current methodology of the internal combustion engine OEMs. This seems a questionable conclusion. The manufacturer of a flashlight is seldom the company that makes batteries. The clear example of a more likely market is the lift truck business, in which a fuel cell supplier builds a drop-in device to power lift truck vehicles. Options at this early stage need to be open.
  - Completed work focuses on projection of future fuel cell markets, with a clear bias toward the vehicular fuel cell market. Some interesting data about fuel cell stack projection were presented, along with cost projections from potential manufacturers. There were no surprises.
  - No list of actual or suspected current parts suppliers was mentioned.

Question 3: Collaboration and coordination with other institutions

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

- Collaboration appears to be effective to date. Future tasks, including garnering detailed cost data, may be more challenging. However, the PI looks to have a very good handle on this challenge. The PI should keep up the good work.
- The project has strong collaborators from the project execution side and from industry stakeholders.
- The project has very good partners in terms of experience in the industry, technical knowledge of fuel cell systems and manufacturing, cost analysis, and market analysis. The relationship with the National Renewable Energy Laboratory, "data collaboration," was not explained very well.
  - As much as the analysis seems to be relying on the SA cost analysis, it would be good to see, in future reviews, how the results of this competitiveness project have affected the analysis or methodology used by SA.

- Collaborations are with other organizations that are funded by this project. Although there is considerable competence in those team members, and one (SA) has built a credible bill-of-materials list, much of the actual present or future designs is not well understood. Certainly this information will flow eventually. The emphasis seems focused on FCEVs, even though the large global market today is combined heat and power (CHP). One interesting market possibility is to sell CHP systems as appliances and do the necessary market creation by working through permitting and safety evaluations that permit those systems to be deployed widely in the United States. Other potential markets need emphasis as well.
- More should be done to coordinate with the other Manufacturing R&D projects, including MN-012, Ohio Fuel Cell Coalition, and MN-013. Virginia Clean Cities.

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.

- There has to be a start in building the U.S. supply chain, and this work is laying that foundation. There is no bias shown for any specific U.S. location—the place where the businesses will be built—which seems most appropriate.
- There is potential for high impact if the web portal from MN-013, Virginia Clean Cities, is integrated with the all the data collected from this project. Maybe the project can be extended to include early markets, including forklifts, cargo, and buses, in addition to the automotive application considered in this project.
- The project will help identify supply chain gaps and potentially yet unrecognized barriers.
- The presentation does not make clear how this project will assist DOE, beyond currently supported cost and market analyses. Supply chain decisions ultimately rest with OEMs, not DOE.
    - It is not clear whether the project will output new technologies that need to be developed or parts or components that need to be redesigned to assist DOE.
    - It is also not clear whether this project will be linked with jobs assessments, such as at Argonne National Laboratory, nor whether there will be coordination with the other supply chain projects supported by DOE.

Question 5: Proposed future work

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

- The work for next year includes an estimate of global shipments of fuel cell and hydrogen storage components. That document could be very interesting and valuable. It would be useful to include plans that build an early technology utilization scheme, for instance, a residential development that is built as a test bed of distributed fuel cell power. There needs to be focus on market creation, and getting interested and competent people involved could be most useful.
- The project is well run, and the PI is aware of coming challenges. The PI has experience with this type of work and has overcome similar challenges in the past. The reviewer looks forward to seeing next year’s presentation.
- The proposed work for fiscal year (FY) 2017 is excellent.
- Future work is clearly identified. A Gantt chart (or something similar) would be helpful for understanding timing.
- Future work is a continuation and expansion of current work. It would be good to see more emphasis on ensuring consistency with current EERE competitiveness analyses.

Project strengths:

- The project is well run. The PI has run similar projects in other industries and has developed workable approaches to challenges.
- The PI brings decades of experience in part supplier relationships. His contacts are critical for project success.

- The project has good contributors, with long experience and understanding of fuel cell technology, industry, cost, and market.
- The project has a good foundation with the company questionnaire. The FY 2017 goals to identify specific advantages are also commendable.
- The project has the potential to yield very useful and actionable supply chain information.

Project weaknesses:

- The focus on vehicles seems critically dependent on existing OEMs, yet these organizations carry huge debts, money used to finance their existing technology manufacturing base. There should be greater emphasis on more—probably early—markets for fuel cell products.
    - There needs to be some concern for designs (including operation parameters) that result in necessary performance and durability.
    - There is no concern expressed for end-of-life considerations and the companies required to pull off those necessary tasks.
- Dissemination of results as a final report may not be adequate. It would be unfortunate if valuable results were not more readily available or accessible to the community. Perhaps there are other approaches in addition to a report that might help garner interest and highlight results.
- Overall, the methodology of the project to provide the Fuel Cell Technologies Office (FCTO) with competitiveness information regarding automotive fuel cell production has not been shown. It is not at all clear how this project will benefit DOE beyond current cost analysis and market analysis projects.
- The approach to get to FY 2017 objectives needs to be laid out better.

Recommendations for additions/deletions to project scope:

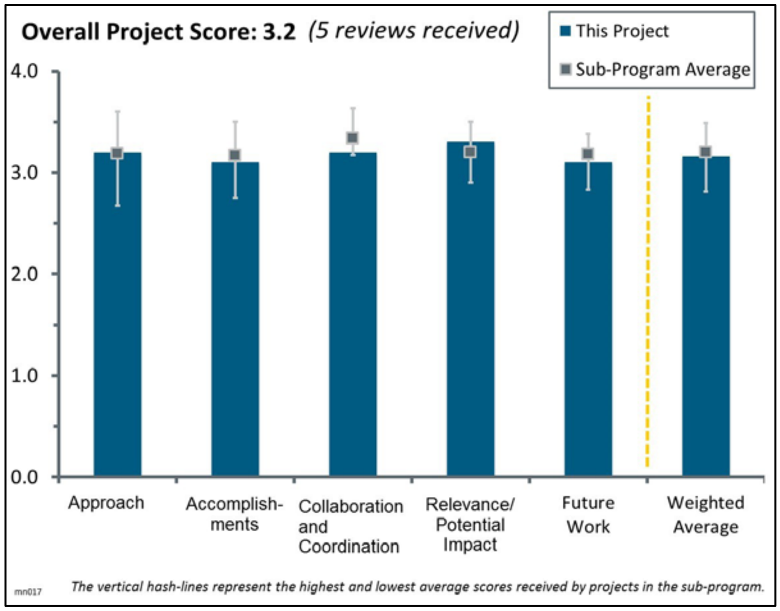
- It may be enlightening to explore further the instances of differing assessments of manufacturing readiness by OEMs and Tier 1s.
- It would be good to see a benchmarking of the project methodology against the standard EERE/CEMI methodology at the Clean Energy Manufacturing Analysis Center. It is not clear what purpose redesigning “5 key components” plays. The value of this activity is not clear.
    - Better coordination with the other FCTO supply chain projects would seem to be of value.
- Having vehicles as a focus is okay, but other markets should not be excluded.

Project #MN-017: Manufacturing Competitiveness Analysis for Hydrogen Refueling Stations

Margaret Mann; National Renewable Energy Laboratory

Brief Summary of Project:

This project contributes to manufacturing cost analysis for major hydrogen refueling station (HRS) systems. The project will work with the Fuel Cell Technologies Office to establish HRS manufacturing cost models and a manufacturing cost framework to study costs of HRS systems, including the compressor, storage tanks, chiller and heat exchanger, and dispenser. Investigators will assist in highlighting potential cost reductions in the manufacturing phase for future research and development projects in this field.



Question 1: Approach to performing the work

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

- The National Renewable Energy Laboratory (NREL) looks at capital costs necessary to install a HRS as part of the U.S. Department of Energy's (DOE's) Clean Energy Manufacturing Analysis Center. The organization and execution of the activity appears to be of the highest quality. As stressed, the data project current evaluations. Such a study requires a reference design and the results will be influenced by that design.
- Capturing the HRS cost and projections for growth is important work, and this project has made significant progress in capturing this information.
- The work to date appears to be thorough and detailed. The approach seems to be working.
- NREL should take into account new compression technologies such as electrochemical compression, which seems to be on the verge of commercialization. It is not clear whether the project is focusing on current technologies available or those that have the potential to reduce costs in the next five years.
- This project tries to study too many things in such detail and is making many assumptions that may not be justified. These details are probably best left to manufacturers and original equipment manufacturers (OEMs). The project needs to better focus on the big picture with a realistic set of assumptions, taking into account the experience gained with stations in California, Germany, and Japan.

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.

- Continuing the work as planned will provide useful information to help steer other programs.
- The project is 75% percent complete, with analysis completed on many of the aspects of the HRS.
- The project has made good progress for the funding. NREL reports many details on various sub-systems. However, very little is being done to use this information to identify gaps and guide DOE on future research needs, etc.

- Considerable information was presented on both the deployment of stations in the global community and costs for those existing stations. It might have been more useful to include hydrogen production costs, but that would have clouded the issue: how much money is required to build the HRS.
- It is not clear how the project addresses the current barriers.

Question 3: Collaboration and coordination with other institutions

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

- There are good partnerships between Sandia National Laboratories, Argonne National Laboratory, Pacific Northwest National Laboratory, and a number of overseas contributors.
- The study could have been possible only with cooperation of current industrial manufacturers. However, there is no indication of other collaborations.
    - A good collection of excellent collaborators was described. Actual roles among participants were not clear.
- There are good collaborations with various relevant institutions. Maybe more can be done with the HRS operators in California.
- Perhaps collaboration with new technology developers could be helpful: solid and organic storage, electrochemical compressor, commercial dispenser, and point-of-sale suppliers.
- While data have been gathered for cost analyses, there is no indication that the results have been shared with existing manufacturers for validation.

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 information is needed to further the growth of HRS installation and development. This research has dissected the HRS and broken down the costs into the components.
- The project gives a clear understanding of costs for an HRS. It assumes a hydrogen supply from either pressurized gas cylinders or from cryogenic liquid hydrogen. In many cases, it might make sense to include steam methane reforming or electrolysis in the station design. Those additional costs are understood.
    - The issue of finance was mentioned but not addressed. Certainly, some governmental and some private funding mix will usually be required.
- Results will be valuable to help guide the focus of future funding.
- It is not clear whether the models are used only for helping DOE assess status. Impact could be larger if these models are shared and used by the developers.

Question 5: Proposed future work

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

- The project is almost complete, and the plan to complete the final tasks is logical and reasonable.
- Future work should include additional companies that produce gasoline dispensers, new heat exchangers, electrolytic compressors, and novel storage that is nearly commercialized.
- The information given was that the project would terminate in December. One suspects this team will be given an additional assignment, one that may or may not be a follow-on to this study.
  The presentation was lacking in big picture details.
- It is not clear how the objectives stated in the “Relevance and Goals” slide are going to be met.

Project strengths:

- The project has been successful in meeting the goals and milestones. The cost analysis seems to be very thorough for each component.

- The engineering team is obviously first-rate. A very detailed manufacturing cost breakdown was given.
- The project represents a nice aggregation of cost data for current systems and components.
- The team has produced very detailed and comprehensive analyses to date.
- The analysis of various sub-systems is detailed.

Project weaknesses:

- The lack of choices from manufacturers limits some of the robustness of the cost calculations, but this is not the fault of the researchers.
- It would have been interesting if some effort was spent on durability and operating expenditures (OPEX). In some ways, the design was minimal. For example, there was no allowance for better utilization of the delivered hydrogen in the tube trailer. There was no mention of various compressor designs, such as the ionic liquid (Lurgi) design that operates with considerably higher efficiency. Again, that would be an OPEX consideration.
- It would be good to close the loop and garner comments from existing industry players as to the accuracy of results, i.e., "sanity check" the results and see whether they make sense.
- There is not enough focus on driving innovation and what areas would result in the largest benefits for cost reduction and reliability.
- The project is 75% complete, and it is not clear how thoroughly the objectives will be met by the end of the project.

Recommendations for additions/deletions to project scope:

- The study was done well. It seems likely that the next one will be of similar quality.
- NREL should discuss how the researchers will transfer this information to industry. It is not clear to which groups it will be disseminated and what methods will be used to transfer the information, i.e., whether it will be through the DOE website or another method. It is good information, but it needs to be disseminated.
- NREL should focus less on the details of the various sub-systems because they are already based on so many assumptions. These assumptions can be made at the sub-system level, and the model can focus on the main objectives of the project.
- The project should reach out to existing manufacturers and developers to review the results and the assumptions that drive the models.
- The project should bring in companies that are innovating away from the traditional and mature systems and components—technology readiness levels 6 and 7.

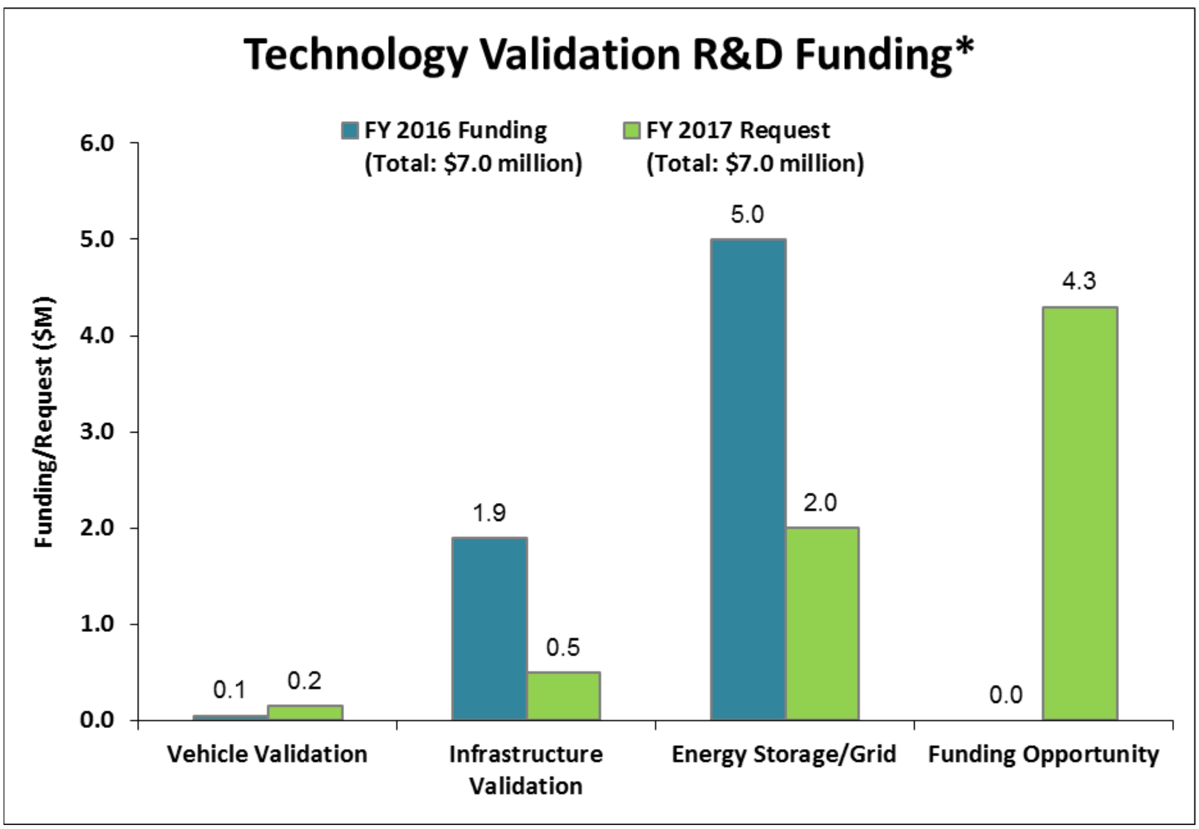
2016 — Technology Validation
Summary of Annual Merit Review of the Technology Validation Program

Summary of Reviewer Comments on the Technology Validation Program:

In general, reviewers commented that the Technology Validation program is well managed and follows an appropriate strategy, with an adequate balance of near- and long-term projects in its portfolio. They believed that effective collaboration with partners and successful management of large amounts of data furthered the program's goals and objectives. While reviewers stated that progress related to projects was adequately presented, they suggested that the issues faced by projects should also be highlighted, and recent progress should be compared to that of the previous year. Reviewers further recommended comparing data gathered from domestic stations against those from overseas; providing cost information on all of the projects, and developing more precise goals to coordinate topics for energy storage with both transportation and grid management.

Technology Validation Funding:

The Technology Validation 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- and heavy-duty trucks, and buses), hydrogen infrastructure activities (e.g., fueling stations, components, and tools), and grid integration/ hydrogen energy storage activities. In coordination with the Office of Electricity and other offices in the Office of Energy Efficiency and Renewable Energy, a key focus in fiscal year (FY) 2017 will be hydrogen-based energy storage and grid integration activities, including "H2@Scale," an approach to enable decarbonization among multiple sectors. The FY 2016 appropriation was $7 million. The FY 2017 request of $7 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 14 Technology Validation program projects had a maximum score of 3.80, a minimum score of 2.80, and an average score of 3.31. Key strengths identified by reviewers in all of the Technology Validation projects were the collaboration involving key partners and the potential for the projects to contribute valuable data to allow stakeholders to gain enhanced insights and successfully deploy hydrogen and fuel cell technologies.

Vehicles: Four projects relating to transportation applications were reviewed, with an average score of 3.3. The highest-ranked project in this grouping received a score of 3.7, while the lowest-ranked project scored a 2.8. The National Renewable Energy Laboratory's (NREL's) fuel cell electric vehicle (FCEV) and fuel cell electric bus (FCEB) evaluation projects were regarded as providing valuable real-world insight and were praised for the collaborations with industry stakeholders. Aging vehicles were the main concern. Reviewers thus suggested acquiring data from newer-generation FCEVs and normalizing FCEB data to account for aging buses and smaller sample size. Argonne National Laboratory's fuel cell electric truck (FCET) component sizing project and the Center for Transportation and the Environment's fuel cell hybrid electric delivery van project were both observed as having potentially promising impacts. It was recommended that modeling performed for FCETs be validated for real-world performance and that providing fueling for the delivery vans be a focus area earlier in the project.

Hydrogen Fueling Stations: Three projects focusing on hydrogen fueling stations were reviewed, with an average score of 3.1. The highest-ranked project in this category received a score of 3.3, while the lowest-ranked project scored a 3.0. Reviewers anticipated that the value of data collected and analyzed through NREL's hydrogen station data collection and analysis project would grow as more stations come online, but also recommended better context in presenting data analysis, while strongly suggesting that all retail stations fueling FCEVs report operational and cost data. The collaboration between GTI and Linde in the development and performance evaluation of delivered hydrogen fueling stations was commended by reviewers, and they suggested the collection and evaluation of additional data, such as fill variations and boil-off rates. While reviewers thought that there would be some useful learnings from the Brentwood (Washington, D.C.) hydrogen station case study performed by NREL, they were concerned about the limited nature of the applicability of learnings, as compared to investigating the implementation of hydrogen stations at retail sites.

Hydrogen Infrastructure Support: Six projects focusing on components, methods, and tools supporting hydrogen fueling infrastructure were reviewed, with an average score of 3.4. The highest-ranked project in this category received a score of 3.8, while the lowest ranked project scored a 3.1.

The hydrogen component validation project by NREL and the HyStEP (Hydrogen Station Equipment Performance) device project by Sandia National Laboratory were viewed as consistent with H_2USA priorities by providing crucial information for increasing hydrogen station reliability and accelerating station development. Increased collaboration with stakeholders through the H2Tools platform was recommended for the components project, while reviewers suggested feedback from potential users be obtained for the HyStEP device.

The advanced hydrogen tube trailers developed by Air Products and the cryogenic vessels and high-pressure liquid hydrogen pump fashioned by Lawrence Livermore National Laboratory (LLNL) were both seen to be of value to the development of hydrogen infrastructure. However, reviewers also cautioned that the LLNL project may be occupying a limited niche and strongly recommended collaboration with and input from more than one automaker. Reviewers further suggested that system cost goals and analyses be performed for both projects.

Reviewers believed that NREL's hydrogen meter benchmark testing could extract greater value by developing standards and methodologies that can be used across flowmeter manufacturers. Reviewers further suggested including station owners and operators with real-world experience in the effort, as well as reporting on how the meters are calibrated and the standard to which they will be calibrated, installation factors such as straight runs, orientation and vibration mounting, and other environmental factors relevant to the specific flow meter types.

The California Fuel Cell Partnership's Station Operational Status System project was praised for successful implementation in all California stations, and information provided by the system was regarded as vital to gaining customer acceptance. Reviewers suggested the addition of tank categories to accommodate vehicles with larger tanks, such as buses.

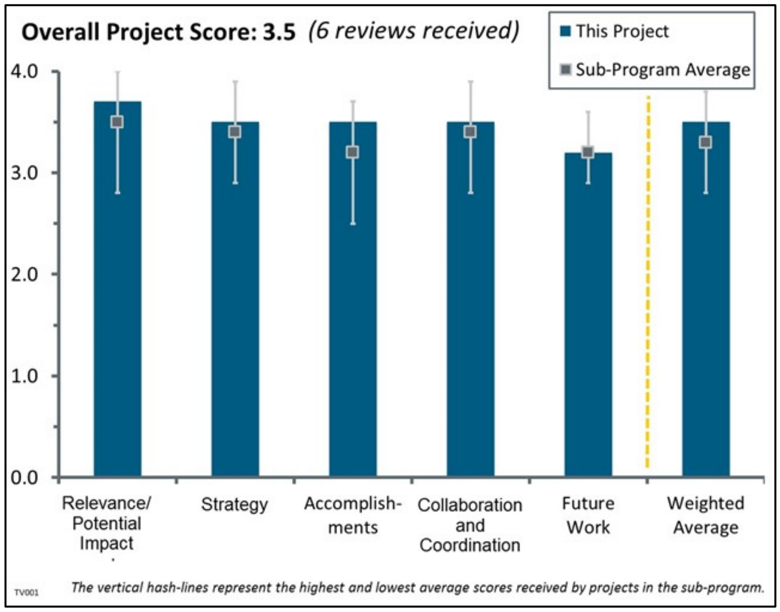
Hydrogen Energy Storage/Grid Integration: Idaho National Laboratory's project on dynamic modeling and validation of electrolyzers in real-time grid simulation received a score of 3.3 and was regarded by reviewers as important for understanding how electrolyzers may benefit the grid and penetration of renewables may be increased. Reviewers recommended investigating revenue streams in the case of future higher penetration of renewables, evaluating the impact sub-systems supporting the electrolyzer will have in terms of response times, and increasing testing time to between 4,000 and 8,000 hours.

Project #TV-001: Fuel Cell Electric Vehicle Evaluation

Jennifer Kurtz; National Renewable Energy Laboratory

Brief Summary of Project:

The objectives of this project are to validate hydrogen fuel cell electric vehicles (FCEVs) in real-world settings and to identify the current status and evolution of the technology. The analysis will objectively assess progress toward targets and market needs defined by the U.S. Department of Energy (DOE) and stakeholders, provide feedback to hydrogen research and development, and publish results for key stakeholder use and investment decisions. Fiscal year 2016 objectives focus on analysis and reporting of FCEV durability, range, fuel economy, fueling behavior, and reliability.



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 **3.7** for its relevance/potential impact.

- This project continues to measure the fuel cell stack and system efficiency to help meet the DOE target. Greenhouse gas (GHG) emission comparisons are now being evaluated. The FCEV durability, fuel economy, and driving range have now been documented over four years to demonstrate real-world conditions. The project continues to provide the six original equipment manufacturer (OEM) partners with essential data to improve the design and efficiencies of their FCEVs.
- This Technology Validation project has provided a wealth of information to support the development of FCEV technology that can meet commercialization goals. It is possible that the steadily declining number of participating vehicles and the age of the technology in them means that there is less benefit from this analysis going forward. The principal investigator’s comments that there will be valuable insights into certain metrics (such as durability) coming from the older vehicles are well taken. Nevertheless, the team’s goal of bringing some of the newer FCEVs into this data collection scheme will be important in addressing this concern.
- The project provides valuable real-world FCEV field data. It provides a clear and objective assessment of the current on-road status of automotive fuel cell technology progress. The real-world data are invaluable for suppliers, developers, investors, etc.
- This project focuses on FCEV evaluation, thus fully supporting the DOE Hydrogen and Fuel Cells Program.
- The project is to evaluate early FCEVs of high value to test durability, performance, fueling, and areas for improvement.

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.

- There is no substitute for driving the vehicles and filling them in a real-world situation. Collecting the data has been the right approach from day one. The National Fuel Cell Technology Evaluation Center is doing an excellent job with the data analysis.
- The project tracks and highlights progress on the key performance commercialization barriers. The key barriers have been identified, and the progress is being tracked properly.
- The project's methodology is sound, and it provides a good number of valuable data. The additional GHG estimates are valuable.
- The strategy—to assess early-market FCEVs and to allow the benchmarking of vehicles to improve performance and durability—is sound.
- These are excellent approaches based on data collection from vehicle operators.

Question 3: 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 analysis and publication of data are impressive and should continue. The public needs to be made aware of the significant progress these vehicles have made over the past four years. With the newer-generation vehicles coming on line and the old vehicles being retired, the new data collected continue to be useful and important as feedback for efficiency and improved specifications for OEMs. Analysis of fuel cell degradation over time is impressive and essential for establishing a metric. There has been a measurable improvement in the fuel cell stack degradation. The stack and system efficiency has also improved. The hydrogen production shows that onsite renewable production is the most efficient. Maintenance and reliability were analyzed per vehicle. The slide showing causes and effects for stack maintenance was helpful. It showed there are some undetermined areas. The 16,000 fills is also impressive.
- Good benchmarking data have been collected from earlier vehicles and have helped with commercialization of new vehicles.
- The project is sharply focused on critical barriers (DOE targets).
- The project has done very well in measuring and demonstrating progress toward DOE goals. The main question is whether newer-generation fuel cell technology should be included to assess the impact of projects like this on the development of the technology.
- It is not clear what the progress has been since last year's review.

Question 4: Collaboration and coordination with other institutions

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

- This project has amazing collaboration from six OEM partners. These partners are solid, and this project could not have asked for a better collaboration.
- The project seems to be well integrated with other organizations examining FCEV technology. It would be useful to get a better sense of how the OEMs are using these data and the value to the OEMs, although that may be difficult for confidentiality reasons.
- There is excellent collaboration with vehicle manufacturers/operators.
- There is good interaction with FCEV OEMs for data use, benchmarking, and analysis.
- It appears that the automotive partners are appreciating the data crunching and reporting.

Question 5: Proposed future work

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

- The future work is clearly outlined.
- In the future, work with newer vehicles and with a driver–refueling interface would be of value.
- Bringing in newer vehicles would add to the project's value quite a bit.
- The customer feedback needs to be matched with the technical data.
- The relevance of the proposed activity "identify new opportunities to document fuel cell and hydrogen progress publicly" is unclear.

Project strengths:

- Overall, this is a well-designed project that has provided much valuable insight into FCEV performance. The composite data products are well thought out, and the team has been good at developing new products over time to show data in different ways.
- Six OEMs continue to be engaged. The data published are transparent and well organized. The public needs to be made aware of the significant progress.
- The collection and analysis of data and trends on FCEV operation are strong.
- The objective data collection and FCEV benchmarking is strong.

Project weaknesses:

- There are no significant weaknesses.
- The main weakness is in trying to make the connection between what is seen here and what is happening in the private sector's continued development of FCEV technology. It is to be hoped that adding in some new FCEVs will help demonstrate the progress (or lack of progress) made in FCEV development.
- The data from FCEVs are not direct from the transponder or users, but through OEMs. Additional information is needed for the driver–refueling interface. Data from older FCEVs should be normalized for comparison with new FCEVs.

Recommendations for additions/deletions to project scope:

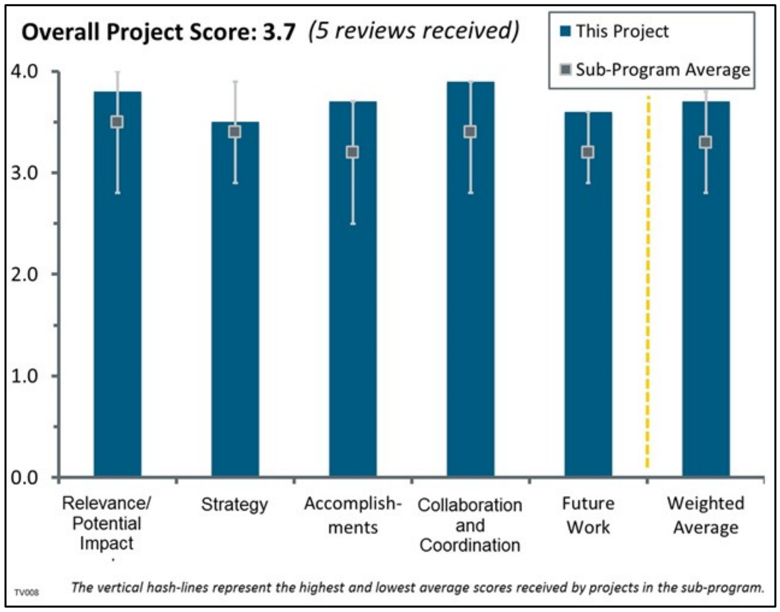
- It is recommended that data also be obtained directly from the transponder, if possible. Data should be collected for the driver–refueling interface. A new FCEV benchmarking is recommended with normalized or separated data of older FCEVs.
- It is recommended that the project make a publication and/or presentation—more public showings that customers can use hydrogen in a vehicle safely.

Project #TV-008: Fuel Cell Bus Evaluations

Leslie Eudy; National Renewable Energy Laboratory

Brief Summary of Project:

The objectives of this project are to 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 conventional technologies and to document progress and lessons learned on implementing fuel cell systems in transit operations to address barriers to market acceptance. Annual FCEB status reports will compare results reported from transit partners and assess progress and needs for successful implementation of FCEBs.



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.

- As FCEBs are critical to the advancement of fuel cells, the technology validation of FCEBs is an absolute requirement.
- With increasing attention on fuel cell technology application in trucks, this work becomes even more valuable—as it currently is for transit stakeholders.
- This continues to be an excellent project for assessing the progress of FCEBs toward the DOE/DOT targets for the current year and the ultimate targets.
- This project is necessary for FCEB commercialization.
- The correlation of DOE along with the 2016 targets and the ultimate target were explained very clearly.

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 works closely with bus fleet companies. The project provides industry data back to the industry on validation assessment and optimization.
- The strategy to objectively evaluate U.S. FCEB operations is sound.
- The project is well designed to track the performance targets set by DOE/DOT and to ensure that there is an apples-to-apples comparison with the buses. The weakness with this is that, with so few FCEBs in operation, there is limited ability to compare to other technologies such as pure electric against which fuel cells will be competing. It would be interesting to see more about the fueling part of the equation, since it is a little unclear how the infrastructure barriers specific to FCEBs is being addressed (for example, fueling times and time between refueling). It would be interesting to see the bus fueling data t side by side with the light-duty vehicle data.
- On slide 9 of the presentation (Availability Summary: 2015 Data), data from the top pie chart indicate that fuel cell system problems reduced total availability by 13% and accounted for more than half of the non-availability period. To improve the availability reported, vice actually improving availability as well as the

quality of the report, the National Renewable Energy Laboratory (NREL) removed the data for the two troublesome buses to get a better number. To their credit, NREL researchers are clear about what they did and did not try to lie, but they did fudge the numbers to provide a number that was not true, i.e., the second, lower pie chart reports only on good buses rather than all buses. Slide 13 provides comparison data for FCEBs vs. diesel and compressed natural gas (CNG) buses. It would have been nice to see a similar comparison of the data provided on slide 14 (Maintenance Cost per Mile by System), maybe a separate slide comparing a composite of *all* FCEB maintenance costs against similar costs for diesel and CNG buses. The chart would probably not have been favorable to fuel cells, but it would demonstrate just how much more improvement needs to be achieved.

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.

- The buses run for five to six years, and their total hours of operation is most impressive at over 10,000 hours for each bus. The average bus availability has improved to 73%. The fuel economy has improved. Reliability continues to increase and has surpassed ultimate targets. Preemptive maintenance has resulted in many of the improvements.
- Given the task assigned, the project team appears to be clearly and sharply focused on the project goals and barriers.
- The project is well targeted to assess progress against key performance barriers.
- There is clear delineation of reported results compared to the previous year's results.
- Progress is good, but the value is being challenged by the overall small number of buses and older bus performance.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.9** for its collaboration and coordination.

- The transit agencies are providing data, and they also review the reports before they are made public. The transit agencies have been cooperative and helpful. NREL has a good relation with the transit agencies.
- Collaboration is very good, as always. The international collaboration is especially interesting, even though the project cannot make direct comparisons against the international deployments because of the difference in operating environments. It would be interesting to see how the international deployments stack up in terms of progressing toward their own local goals compared to how the U.S. programs are progressing.
- There is broad collaboration with both industry and government stakeholders. The project should work with the DOE Office of Energy Efficiency and Renewable Energy Vehicle Technologies Office (VTO) on transfer of knowledge to increase durability and reliability of the fuel cell, battery system, and other components, especially where these components are used in other vehicle applications (such as VTO-funded hybrid- and battery-electric truck projects).
- Collaboration is excellent with transit districts, FCEB original equipment manufacturers, and other organizations. The challenge would be to secure data from other counties where FCEBs are operating.
- The NREL team, and especially the presenter, stated that a great deal of involvement was performed by a number of partner organizations.

Question 5: Proposed future work

This project was rated **3.6** for its proposed future work.

- Given the future necessity for FCEBs and the need to measure their performance, there is a requirement to continue with future work.
- The main interest is in seeing how the FCEBs will compare to a hybrid or battery bus, so it is good to see that NREL will be analyzing battery bus fleets shortly.

- Older bus data may affect analysis of new (next-generation) buses being introduced. Proposed work with parts, maintenance, and support providers is appropriate and valuable, especially for older bus support.
- Strong plan for data analysis/reporting on future bus projects.
- Data collection remains the number one need. Better tools and training are needed. Lessons learned should be documented.

Project strengths:

- A broad array of detailed data is reported. The number of vehicles is expanding. Most vehicles included in data reporting are in full daily revenue service, not demonstrator buses used for educational purposes.
- Objectivity; comparative analysis with FCEBs, diesel buses, and CNG buses; and detailed information on operational costs are all project strengths.
- The methodology is well established and sound, making this a credible, objective assessment of FCEB progress.
- The project is analyzing successful fleet operations and a total of 17 buses.
- This was a good, clear presentation, and the presenter was refreshingly direct and clear.

Project weaknesses:

- The biggest concerns are more to do with whether FCEBs are making sufficient progress toward the ultimate targets than with this project, which is very effective at doing what it is supposed to do. If certain targets appear to be too difficult to reach or other technologies, such as battery buses, will meet those targets first, then it seems we should start to question whether it makes sense to keep tracking FCEB technology in demonstrations.
- Project weaknesses include the low number of vehicles, performance as buses get older, older bus maintenance costs, and lack of collaboration with other bus users outside of the United States.
- A weakness is the need for a good relationship with transit agencies to obtain operational data. If this is part of the contractual obligation of transit agencies funded to operate FCEBs, this should not be an issue.
- It is recommended that NREL present all the data and facts as they are rather than as how NREL would like them to be.

Recommendations for additions/deletions to project scope:

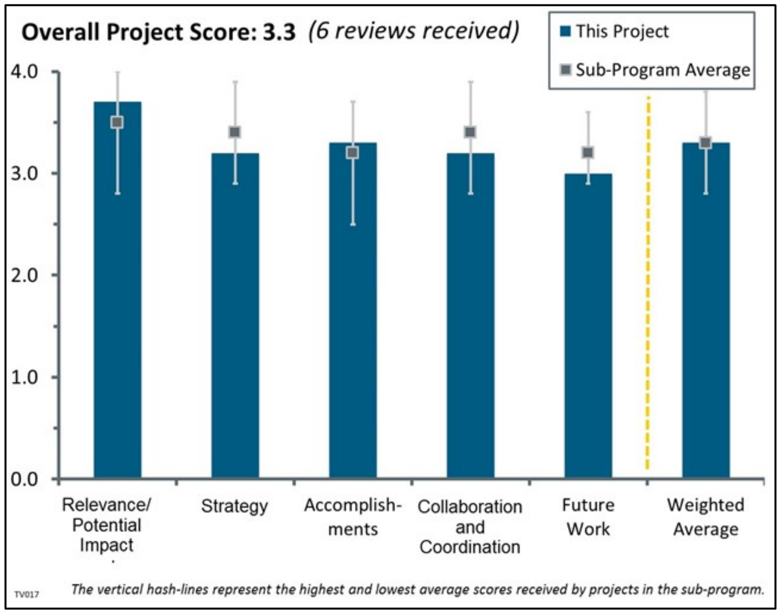
- It was asked in the question-and-answer session whether it would be possible to compare the FCEBs to electric buses. The difficulty of doing an apples-to-apples comparison is very clear, but it seems like this is an important goal to try to reach in some way, especially as electric buses are being deployed in moderately higher numbers over the next year or so. It is not clear whether the project has evaluated noise reductions from the buses. Perhaps this is self-evident, but it seems like it is another potential selling point for FCEBs. It is also a little unclear whether this project is tracking the powertrain cost or the overall bus cost. In addition, perhaps it would be possible to do a GHG assessment, similar to the one done for the light-duty vehicle technology validation project.
- The project should add potential assessment of similar drive systems for trucks (as drive systems are identified as a challenge affecting operational costs/miles). FCTO should include the FCEB supply chain for components and parts in the Manufacturing Research and Development program’s efforts. There should be development of a neutral indicator of knowledge/expertise levels of maintenance staff at transit agencies in maintaining FCEBs (electrical systems, electrified components, electric drive/power train, gaseous fuel storage systems [CNG and hydrogen], and automated operational data collection).
- The project should continue with the intent to normalize data to account for older bus performance, increase the number of buses in the project with collection of bus data from operations outside of the United States, and assess costs attributable to high maintenance due to unique low-volume manufacturing.

Project #TV-017: Hydrogen Station Data Collection and Analysis

Sam Sprik; National Renewable Energy Laboratory

Brief Summary of Project:

This project will evaluate hydrogen infrastructure performance, cost, utilization, maintenance, and safety. Data analysis will support validation of hydrogen infrastructure, identify status and technological improvements, provide feedback to hydrogen research, and provide results of analysis for stakeholder use.



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 **3.7** for its relevance/potential impact.

- This project is critical to the Fuel Cell Technologies Office (FCTO). Collecting and analyzing data are extremely important in assessing technical performance and cost over time, as well as informing FCTO's future direction. Simply put, what gets measured gets managed.
- The project metrics are reasonable and well defined. The U.S. Department of Energy (DOE) is getting good value from this project, as it addresses the critical areas that need data collection and analysis prior to commercialization.
- The project is an important way to address the hydrogen infrastructure challenge. The project's value will grow considerably as more stations come on line.
- The project is clearly very relevant since data on station performance are essential as hydrogen infrastructure grows.
- The project aligns with DOE research, development, and demonstration goals, but serious consideration needs to be given to how the National Renewable Energy Laboratory (NREL) is proceeding, as NREL's method may not be supporting and advancing progress.
- It is not clear until what point in the future these data have to be collected. At some point, issues are known, and hydrogen stations operate in a more or less unsupported commercial environment. Before this point arrives, the DOE/NREL focus for hydrogen station data collection/reporting should shift to bus/medium- and heavy-duty vehicle fueling stations.

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.

- The project defines what kind of retail outlet is wanted in the future. The project works closely with the California Fuel Cell Partnership, among others, and continues to make improvements. Every six months, the project publishes results. All the results are very transparent and are published on the project website.
- This analysis is a good foundation for assessing hydrogen stations and the targets for fuel cell electric vehicle (FCEV) and infrastructure viability. The project's value will grow considerably as more stations come on line, especially retail ones. It is a bit difficult to judge how some of the older stations reflect what

will happen in a retail environment. It would be helpful for the team to explain which site appears to have significantly more hydrogen dispensed than the others (on slide 9). One station seems to comprise a very significant amount of hydrogen dispensed. Also, on slide 18 it would be good to explain why the reformer failure rate is not considered worse than that of the compressors and what the significance of the reformer failure rate is.

- The strategy for gathering data from U.S.-based stations is adequate, but it would be even better if the strategy involved more stations. It is understood that, for confidentiality reasons, some of the stations do not provide data to the National Fuel Cell Technology Evaluation Center. However, the team could reach out to international partners in places such as Japan and Germany to gather additional information. Another potential idea is to gather component data from non-hydrogen refueling stations to compare their performance. It is problematic that the level of detail obtained from each partner varies. Potential approaches to resolve these issues include a streamlined data template, a help hotline to answer any questions related to data gathering and reporting, and a training program for new data reporters.
- As could be discerned from the question-and-answer session (Q&A), there was great confusion regarding the number of stations in the project. The presenter stated that there were 59 stations, but the project collected data from only 11. The status of the other 48 stations is unclear, as is how many are still in operation and, if they are operating, why the project did not collect their data, etc.
- The project should make sure *all* stations refueling FCEVs provide operational data into the DOE/NREL database to get even better insight into the challenges and status of hydrogen station technology. It is not clear how long (until what point in the future) these data have to be collected.
- The presentation advises that the barrier is the lack of current hydrogen refueling infrastructure performance and availability data, and while the presentation provides numerous charts, there is little if any discussion about overcoming the barrier listed.

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 many stations working and reporting data. The volumes of fuel dispensed have increased, e.g., the average fill is 2.62 kg, and dispensing has become more efficient. Amount, time, and rate are all measured for each station and can be averaged. The utilization rate shows more cars can be filled. The project has identified some of the maintenance issues, failure rates, or kinks being worked out, e.g., compressor dispensers' entire safety storage reformer thermal management. There are compressor issues at all stations. One item was vandalism. There has been excellent progress made on this project over the past several years, and the project must continue to collect data and make improvements before commercialization. Monthly compressor maintenance is conducted. Maintenance cost over time shows $12,400 per station per quarter. Hydrogen quality is also measured. Safety reports by quarter are reported out, and there have not been many incidents except some minor leaks. Electrolyzer energy use per kilogram of hydrogen is tracked. Compressor energy and cost per kilogram of hydrogen are also tracked. This helps with identifying startup time efficiency. The project is very organized, showing major areas of how the project budget is broken down. The team looks at all the critical areas, such as utilization rates, fueling, reliability, cost, and permitting time.
- The team has been consistently delivering consolidated reports every six months and making them available to the public. This is important and ensures widespread data availability. For the general public, it is particularly important to share information on safety records.
- In the future, making a distinction between unique stations and same-design stations may be beneficial for determining whether the same issues continue to plague hydrogen stations when they are rolled out in larger numbers (and new designs). The project should include differences between small and large (capacity) compressor-based stations, as well as an assessment of differences between reported maintenance cost and true (possibly internal to companies) maintenance cost. The project should consider collecting the number of operational hours for station compressors and correlating this with the number of compressor issues reported.

- The project seems to be making good progress toward DOE goals. However, the newer stations will help make this project more relevant to DOE goals, since the newer stations are not intended to be demonstration stations but to show how retail hydrogen stations could operate.
- The presentation provided different answers on the number of hydrogen stations. Slide 5 noted 23 stations, while another stated there were 59. During the Q&A and after the briefing ended, NREL tried to explain that there are different definitions of what constitutes a single “station,” but if the presentation is theirs, NREL should choose the best definition, get consistent numbers, and explain outliers as necessary. The briefing is titled Hydrogen Station Data Collection and Analysis, yet five years into the project, NREL advises that a barrier is a lack of hydrogen infrastructure performance and availability data. If there is a lack of performance and availability data, it is not clear how they generated about 40 pages of data for their briefing. Slide 7 (Hydrogen Infrastructure Composite Data Products) is interesting/disappointing. The chart advises that there are 61 composite data products (CDPs) in 9 categories—43 updated and 18 new—but there is no discussion about what the CDPs are or why they are important. The only clarification was from the oral and written statement that “a subset of the infrastructure CDPs [is] presented here.” There is no explanation of the importance of the subset or the nature of the other CDPs. There is also no subset as offered. Slide 9 (Hydrogen Dispensed by Quarter) makes no sense; slide 10 makes less sense. When asked about the purpose and accuracy of the data points, the presenter remarked that a station could have opened in California this morning. The point of that response is unclear. By slide 11, it was tough to figure out the purpose of the charts being presented. It almost seemed like some charts were added solely to increase the page count so that the presentation was more of a book report rather than a management presentation. Slide 28 and 29 provide previous-year reviewers’ comments. The first comment states that, despite templates being produced, the level of details and harmony of those data have impacts on conclusions from the NREL analysis. A reviewer commented that, despite templates being produced, the analysis is tough to follow. Yet NREL’s response does not focus on addressing the confusion. The result is that the same problem exists this year as last. Another comment from the previous year states that there is no indication that NREL can manage data and analysis for 40+ stations. NREL’s response did not provide details, just assurances. This comment is still valid, and NREL’s ability to manage data and analysis for 40+ stations remains in question.

Question 4: Collaboration and coordination with other institutions

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

- Collaborations are excellent. The partners providing data are very impressive. The memorandum of understanding with the California Energy Commission (CEC) is very important. The California Air Resources Board (CARB) is also a partner in this project.
- Collaboration with international partners is critical to improve data collection from a wide set of technologies and operating conditions. The project could benefit from information exchange beyond safety. This would help benchmark performance and operation data, which could help accelerate the introduction of the most effective technologies and best practices.
- Collaboration is forced in this project because data collection requires inputs from the various station owners.
- The project needs to make sure that all stations fueling FCEVs in a retail manner report operational data for inclusion in the database and assessment.
- NREL provided a list of partners providing data on slide 31 and lists all collaborators on slide 30. Beyond slide 31, there was no discussion on who did what, if anything.

Question 5: Proposed future work

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

- The project must continue to collect data on cost versus performance. The CEC builds the stations, and the project simply collects the data. Although the number of stations in the future is somewhat unknown at this time, at least the team is identifying areas where stations should be located and raising the funds to build them.

- The reviewer is looking forward to the new stations and the separation of retail vs. demonstration.
- Thinking about future work could be broad and/or out of the box.
- NREL may be collecting a lot of data, but without considerable management overhaul, the data could be irrelevant, if not just plain wrong.
- It would have been good to see within the proposed future work an effort to continue to reach out to domestic and international stations that do not currently report data. Also, the team should communicate and exchange lessons learned from international organizations that collect similar data for stations located outside of the United States.
- It is not clear what defines “retail” vs. “demonstration” stations.

Project strengths:

- This is an important area of focus since it represents a key potential barrier to the market and one that really needs to be addressed through a collaborative effort, with public and private sector input. Therefore, this seems like an ideal project for DOE.
- Partners such as CEC and CARB are a huge plus in this project. The publishing of data every six months is excellent and should continue.
- The slides comparing the flow rates and fill times for -20°C vs. -40°C filling are very useful, justifying the additional cost for hydrogen pre-cooling to -40°C.
- Project strengths include the experience from years of data collection, analysis capabilities, and frequent data reports.
- Strengths include the large numbers of data, with more to come, and the ability to handle more.

Project weaknesses:

- One weakness is really a weakness in the hydrogen fueling market. It seems there is a very limited pool of suppliers for some of the key technologies here, making it hard to see if the problems with certain stations are likely to be fundamental problems with the station configuration and technology choice or if the problems are specific to one company’s products.
- International collaboration is lacking. The number of stations within the project is a weakness, as is the types of technologies under review. There is a need for data on combined heat, hydrogen, and power (CHHP) and cryo-compressed hydrogen.
- The inclusion of previous/old-generation hydrogen station data is a weakness. Differences between data fields completed/submitted by station operators give an incomplete picture of what the real issues are.
- One trivial comment is that the word “data” is plural. In the responses to past comments, the presentation includes statements such as “data is reported.”

Recommendations for additions/deletions to project scope:

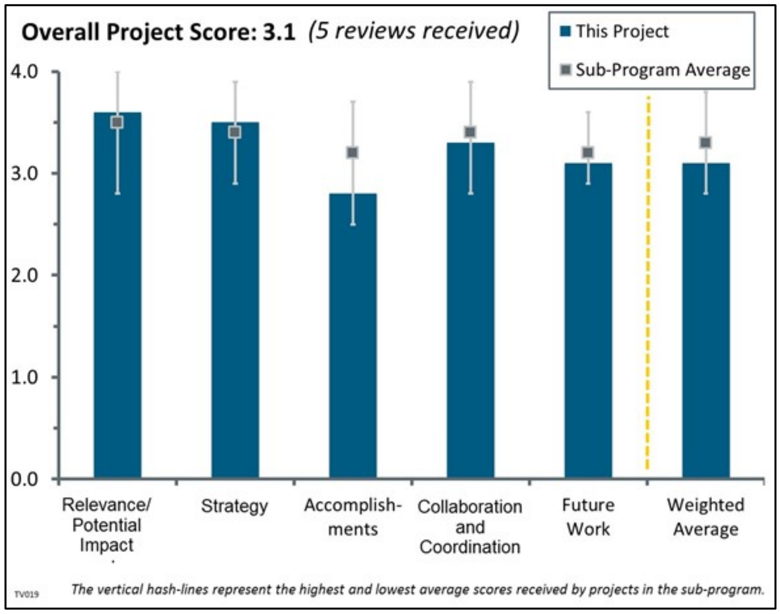
- The project should consider sharing results with the International Partnership for Hydrogen and Fuel Cells in the Economy.
- It could be interesting to assess how a station would ramp up to more hydrogen fueling events per day and how it would supply the hydrogen as the market grows.
- International collaboration for data collection and analysis should be strengthened. The number of stations within the project should be increased, as should the types of technologies under review, for instance, CHHP and cryo-compressed hydrogen stations.
- Under safety events, it would be useful to understand what “near misses” are. The project defines “near misses” as including hydrogen leaks without ignition. It would be good to know whether there were many hydrogen leaks, and, if so, what the estimated leak rates were.
- There should be a serious look at how NREL is managing the project and NREL’s response to this year’s shortcomings.

Project #TV-019: Hydrogen Component Validation

Daniel Terlip; National Renewable Energy Laboratory

Brief Summary of Project:

The objectives of this project are to (1) reduce fuel contamination introduced by forecourt station components, (2) improve station reliability and uptime, and (3) increase the publicly available energy and performance data of major station components. The project will focus its efforts on a contaminant library, station reliability and maintenance, and station power and energy demand.



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 clearly advances and supports progress toward understanding and improving the reliability (and feasibility) of hydrogen dispensing stations. Ideally, this work would be done by industry. However, in the absences of industry work, this project serves a very useful role.
- A project of this type offering failure analysis provides critical information necessary for improvements that increase component and station reliability.
- The project is targeted at identifying key areas of failure, collaboration for common solutions, and commercial advancement of industry for hydrogen fueling.
- It is important to conduct hydrogen component validation.
- Based on the presentation, the objectives and the areas of focus are consistent with H_2USA priorities. However, it is not clear how the objectives and areas of focus address barriers identified by the U.S. Department of Energy (DOE). The presentation should show the barriers being addressed.

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 general approach of working with users to log and investigate equipment failures is sound. The survey of equipment vendor concerns is quite valuable. The National Renewable Energy Laboratory (NREL) is uniquely suited to collect and analyze the data from a variety of sources. The approach of installing and testing equipment is a key component in obtaining and understanding the data.
- Collaborating with industry to identify areas of common failure at compressors, dispensers, and chillers is a sound strategy. The project should share data as appropriate and undertake outreach to identify solutions.
- Leveraging the existing projects is a positive approach for this project. The inability to find a customer base is a weakness. If the customers are sponsored in part by DOE, then they should make space available if this is important to them and to DOE. It is not clear whether the forecourt station operators are DOE-sponsored. There are no specifications of sampling techniques and anticipated contaminants. There are purity specifications for hydrogen, but it is not clear that these will be used in the study. The project will benefit

from the National Fuel Cell Technology Evaluation Center (NFCTEC). Several points are not clear: whether monthly station reports will be published, how H2Tools will benefit the project, how the Station Operational Status System will benefit the project, or how NREL composite data products will benefit the project.
- The project should focus on compressor operation and reliability, as this component has had the highest downtime and maintenance.
- Both organization and collaborations should ensure adequate information is available for failure analysis, part modification, and retest.

Question 3: 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.

- Progress is good in terms of examining common problems associated with contaminants, valves, and seals. Thought has gone into metering to isolate problems. Posting data on H2Tools should be of value for industry collaboration.
- Contaminant library data, energy cost data, and reliability data are necessary data collection points if the industry is to improve uptime.
- The project reports initial data/samples collected and reported. The analyses have been initiated. On slide 9, it is not clear what "function group contaminant studies performed at NREL highlight" means—whether the three chemicals/compounds shown (amides, sulfur compounds, and aromatics) are present there or to be looked for. It is also not clear what "compressors and dispensers remain major maintenance burdens," means on slide 10, since this was already known. The pie chart on side 10 indicates "electrolyzer" has the most maintenance events, but this was not discussed for reasons that are unclear. "Progress – Maintenance Reduction" may not be the proper title for slide 11, since what is presented there are more like actions initiated as a result of data analyses. Progress is not "Communicating with equipment manufacturers." On slide 14, it is not clear whether the 900 bar compressor installation generates the data. It is not clear what the source of these data is since slide 14 comes two slides after the installation slide (slide 12), with chiller and dispenser data in slide 13.
- Progress since last year is adequate; however, at this stage, high-level recommendations on the compressor (design changes/modifications, system changes, etc.) should be given.
- The amount of data analysis and number of conclusions are disappointing approximately four years into the project. Greater understanding and more success stories were expected. For example, the monthly logging and accumulation of data on thousands of failures are very good. However, little or no analysis of the root causes of the failures is presented. This may be due to an absence of findings or merely from deficiencies in the presentation. Photographs of metal flakes and elastomer are shown, but no description of their sources or how to avoid their occurrence is included. Under "Maintenance Reduction," it is not clear how or whether maintenance was actually reduced as a result of the project. The graphs selected for compressor performance did not illustrate conclusions from the project. For instance, presumably performance is measured to compare to expectations and/or compare estimates. Merely showing results does neither. Furthermore, the graphics raised questions as to why power decreases, why the kilogram–hydrogen curve is so jagged, and why a graphic over such a limited range of pressure increase was selected.

Question 4: Collaboration and coordination with other institutions

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

- The project seems to have a robust selection of relevant industry participants.
- The project is partnering with HydroPac and others to identify problems and solutions, which is good. More collaboration is needed, possibly through H2Tools.
- There is a good list of collaborators, although the project should consider adding Air Products and Chemicals, Inc.
- There is collaboration with five different organizations.

Question 5: Proposed future work

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

- The Venn diagram of activities and issues is an effective way of illustrating and thinking about future work.
- The future work of value is to continue with the investigation of common problems, collaboration with industry, and identification of common solutions.
- The proposed future work is too high-level. A list of activities is recommended. The drawing is not well defined. It would be unfortunate if the project work is only the area defined by the overlap of all three circles, although this is not clear.
- The project should solicit seal and gasket suppliers to join the collaboration effort.
- More details should be given to clarify the objective/focus of the proposed future work.

Project strengths:

- The project is fulfilling a need not currently being conducted by industry.
- Strengths include a streamlined process to identify common problems, collect and store data, and collaborate with industry for solutions.
- A high number of critical infrastructure suppliers are engaged collaborators.
- Strengths include the focus on validating operation and reliability of the key components affecting system downtime and maintenance. There needs to be an identification of failure modes and root causes.
- NFCTEC has well-established data collection processes.

Project weaknesses:

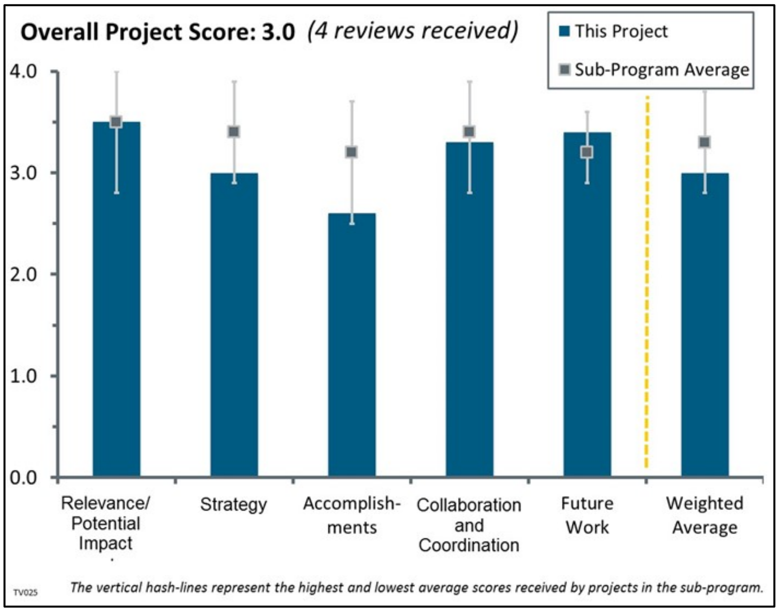
- The project does not identify critical contaminants that have been specified through previous DOE efforts. It is not clear how the project at Hawaii Natural Energy Institute, the project at the University of Connecticut, or codes and standards efforts contribute to this project.
- The testing location is at elevation (5,800 feet). Real assets deployed in the field are essentially at sea level. Corrections for altitude should be included. Future work should prioritize areas of research based on feedback from actual infrastructure installations.
- The project seems to have limited accomplishments, given its four-year lifetime. The viewgraphs as presented do not adequately reveal conclusions from the project; they merely cite activities.
- Lack of project completion targets is a weakness.
- There is limited interaction with industry to identify problems and solutions.

Recommendations for additions/deletions to project scope:

- There should be increased dissemination of aggregated data to industry, possibly promoted through H2Tools, to increase interest in identification of common problems and solutions.
- There should be more emphasis on discovering the cause of the failures and working with the manufacturers to fix them.
- The project would benefit from Air Products and Air Liquide as collaborators.

Project #TV-025: Performance Evaluation of Delivered Hydrogen Fueling Stations

Ted Barnes; Gas Technology Institute (GTI)



Brief Summary of Project:

The objectives of this project are to (1) install 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 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 **3.5** for its relevance/potential impact.

- As the number of fueling stations increases in the coming years, it is very important to get real-world data on the performance of delivered hydrogen fueling stations. The project aligns well with U.S. Department of Energy (DOE) research, development, and deployment objectives.
- Real-time, remote data acquisition provides an easier and faster means to determine the reliability of various station components.
- This project aligns well with DOE objectives and will provide good data from multiple sites to help validate station performance.
- It is unclear whether all the Linde Group stations are based on liquid hydrogen or whether their designs are significantly different. Therefore, monitoring five stations by the same operator may not have as much impact as having different hydrogen delivery technologies or different operators.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.0** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The project has a good plan with appropriate decision points.
- It would be helpful if the project could identify the specific technologies and performances being validated at each station, beyond just reporting number of fills per month.
- The project appears to be stuck because of permitting issues with remaining stations. Perhaps the investigator can switch to stations that are in active duty already.

Question 3: 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.

- This project has made good progress with the first stations and expects to have more than the two years of planned data. Delays on the newer stations will result in less than two years of data. This is not unexpected because of issues with getting stations commissioned at this stage of development. Having more than two years on specific stations could provide added value by showing how the station performs over time.
- The project appears to be stuck because of permitting issues with remaining stations. Perhaps the investigator can switch to stations that are in active duty already.
- Certainly, the delay in permits has contributed to the delay in data from the three non-commissioned stations. However, the quality of data collected from the first two stations could be improved beyond just number of fills and dispensed amount. Perhaps the project should include boil-off rates, daily fill variations, etc.

Question 4: Collaboration and coordination with other institutions

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

- The team has excellent collaborations with industry and multiple station locations.
- The Gas Technology Institute (GTI) and the Linde Group appear to be strong partners.
- It is possible that there may have been practical reasons not to include other stations from different operators, but this project might as well be titled "Performance Evaluation of Linde H2 Stations by GTI." Also, there is no need to plug an unrelated company sales pitch at the Annual Merit Review (slide 15).

Question 5: Proposed future work

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

- The future work will increase overall station data.
- Commissioning of the remaining stations should be at the top of the list. If delays continue, additional time may be needed to generate meaningful data from the new stations.

Project strengths:

- The project will provide much needed data on station performance at multiple sites. Having data beyond two years on some stations could provide insight on station durability.
- Dealing with a single operator should make communication and execution easier.

Project weaknesses:

- The permitting process continues to cause delays for station commissioning. This emphasizes the need to engage code officials early in the process and to educate those with less familiarity of hydrogen.
- Dealing with a single operator can make the data quality biased.

Recommendations for additions/deletions to project scope:

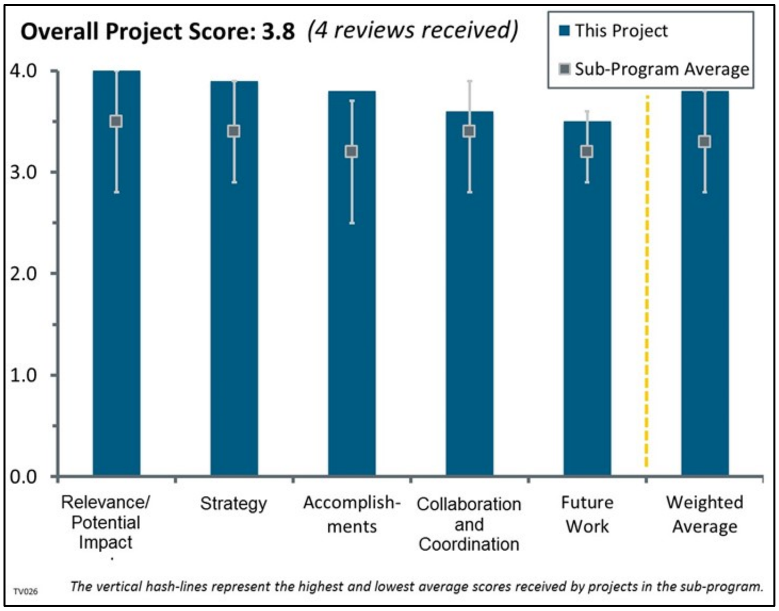
- Perhaps the investigator can switch to stations that are in active duty already in order to collect data.
- The project team should focus on securing permits for the remaining three stations and catch up to the schedule. The team should also consider getting input from other delivered hydrogen vendors.

Project #TV-026: Development of the Hydrogen Station Equipment Performance (HyStEP) Device

Terry Johnson; Sandia National Laboratories

Brief Summary of Project:

The main objective of this project is to accelerate commercial hydrogen station acceptance by developing and validating a prototype device to measure hydrogen dispenser performance. Fill safety is a common goal of vehicle manufacturers, consumers, station operators, and state stakeholders. The Hydrogen Station Equipment Performance (HyStEP) device can shorten the lengthy station acceptance process.



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 **4.0** for its relevance/potential impact.

- This project develops a device to test hydrogen stations without the need to use multiple original equipment manufacturer vehicles. This is of vital importance to the industry and will speed up the process of commissioning stations.
- Based on past experience with the 350 bar station testing apparatus (STA) of the California Fuel Cell Partnership (CaFCP), the development of a unit that can test and validate a station for 700 bar cold fills will be very useful in speeding up station commissioning. However, for adding to commissioning efficiency for commercial purposes, it would also be helpful to have an adapter added to the unit that can collect hydrogen samples for quality testing. Beyond commissioning, the testing unit can also be used for periodic gauging of station performance.
- The HyStEP device is likely to contribute in accelerating station acceptance process, both in time and cost, while helping standardize hydrogen dispensing safety and performance.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.9** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- The only thing this reviewer would change is doing tests in the winter in conditions similar to those found in upstate New York to determine reliability under harsh conditions.
- The project has an excellent approach to developing and testing the device.
- The project appears to be well designed and well managed, both from a schedule and cost perspective.

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 fact that the project was successfully deployed in about a year and half is very impressive. The project management and execution should be held as an example for other projects.
- The project has come in on target for scope, costs, and schedule. The management of this project has been great.
- This project has made significant progress. The design is good for ease of movement between sites. Testing included fault detection to determine how well the stations respond to specific out of bound conditions. The device was validated at several stations.

Question 4: Collaboration and coordination with other institutions

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

- There was good collaboration with other laboratories, industry, and station providers.
- It is recommended that this project understand previous work, such as the CaFCP STA, so that it can learn from others.
- In Phase II it will be crucial to begin investigating who this device will be handed off to for future work and how that will happen. Otherwise, the investment and work to date could fall flat and not reach the stated goal.
- Overall, there is excellent engagement with partners. However, Powertech was missing from the partner and collaborator list although a chunk of the construction, performance, and safety tests was done at Powertech. Also, it is not clear why U.S. automakers were not part of this effort.

Question 5: Proposed future work

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

- The project has a good plan for follow-on work. Most future work is for other partners to put the device to work.
- This should evolve into more of a turnkey operation for station commissioning that includes hydrogen quality sampling for purity certifications and eventually flow meter certification. A company that could do all three would really simplify commissioning.
- The project needs to enlist feedback and participation of possible future users of the device (or one like it) in order to ensure (as best possible) this device will be readily picked up and used for future station testing activities. Merely developing and confirming the device will not be sufficient; project partners need to "lean into" the third party use of this device.
- The stated future work is sound. However, if the project is considered complete and the device deployed, there are questions about who will do the follow-up work and how it should be performed.

Project strengths:

- Project strengths include a robust design that can be easily moved from site to site and open source code to ensure all in the industry can benefit from the technology. The fact that an audience member during the presentation asked where he/she could acquire the device speaks volumes to the value of the project.
- The project team including Powertech with all of its experience helped to keep the project on scope and on time. Further use is guaranteed by California Air Resources Board (CARB).
- The project has a clear objective as well as excellent project design and execution.

Project weaknesses:

- *[No responses entered.]*

Recommendations for additions/deletions to project scope:

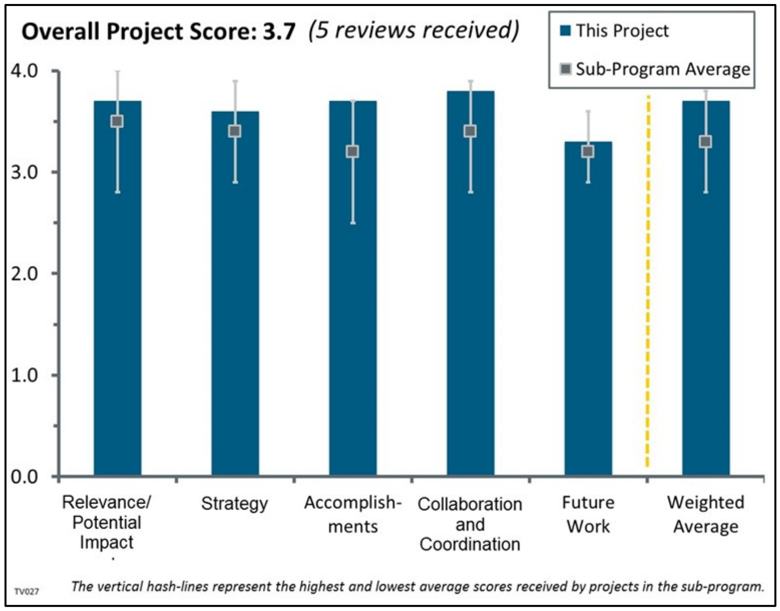
- Although it is likely out of scope, an added benefit would be to investigate the possibility of adding a hydrogen sampling device to test quality.
- It is not clear what the commercialization or deployment plan is for the HyStEP device. It is not clear who should be responsible for validating station performance: regulators (e.g., CARB), station owners, or automakers. This is outside the scope of this project, but if these questions are not addressed, the value of the device may be diminished.

Project #TV-027: Station Operational Status System (SOSS) 3.0 Implementation, SOSS 3.1 Upgrade, and Station Map Upgrade Project

Ben Xiong; California Fuel Cell Partnership

Brief Summary of Project:

The objectives of this project are to (1) consistently and reliably report hydrogen station operational status information to fuel cell electric vehicle (FCEV) customers to increase customer satisfaction and station demand and (2) provide the most recent, realistic, and accurate hydrogen station information to FCEV customers, station implementers, and authorities having jurisdiction to address stakeholder station information needs.



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 **3.7** for its relevance/potential impact.

- This project has provided the U.S. Department of Energy (DOE) and project partners with valuable data and has established a customer-based information system for hydrogen station availability and liability so that an FCEV user can know if a station is available. This information system answers the question of whether the station is online or offline, a needed capability with so few stations available.
- The project strongly supports early-stage rollout of FCEVs. It would be difficult to proceed with early commercialization if a station locator tool such as this did not exist.
- This project is vital to commercialization as it promotes customer acceptance. Customers need this information during the early stage of implementation of hydrogen stations when availability is limited.
- The objectives of this project are highly relevant in advancing hydrogen FCEV deployment. One of the frustrations for owners and early market manufacturers of FCEVs remains the availability of reliable fueling stations.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.6** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- Phase I and Phase II are completed and it is an outstanding information system. Researchers are now improving the Station Operational Status System (SOSS) website so the customer can use the information easier. Data collection is fairly simple, so the customer can determine whether a station is available. Red, yellow, and green indicate the status of a station.
- The project benefitted from an excellent plan from its initial phase through continual updates to improve functionality.
- The project's approach implementing the SOSS upgrade is sound and achievable.
- Because this tool will likely be needed for the next 10 years of early market introduction, adding someone with a bit more information technology (IT) knowledge and experience is strongly encouraged to ensure

that all features needed for future growth and security are taken into consideration. For instance, IT subject matter experts from the University of California, Los Angeles; original equipment manufacturers (OEMs); Air Products and Chemicals, Inc. (APCI); or IBM Solutions can be consulted on this project to make sure that it is done right from the start as opposed to a stopgap, Band-Aid approach that leaves the tool vulnerable and inefficient. For instance, it was unclear why Amazon Cloud was selected over Microsoft Cloud, which may offer a lot more features and flexibility and has a proven track record with industry. Also, if Microsoft Cloud is adopted there is no longer any need to support a separate server, which will become obsolete in a couple of years. You can save cost on hardware and software, have the latest security features, and backup is taken care of by Microsoft. Finally, if the tool will be used for both electric and compressed natural gas (CNG) vehicles, it is imperative that all of hardware and software features/requirements be fully vetted by experts to make them as secure, cost effective, seamless, and intuitive as possible.

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.

- Researchers have successfully upgraded their data to a mobile-friendly access: http://m.cafcp.org. They are developing a backup system or server in case one of the systems crashes. They are also developing a new application called implementation station map upgrade to advise of new stations coming online and their status.
- The number of stations participating in the project is commendable. DOE, the California Air Resources Board, and the California Fuel Cell Partnership (CaFCP) deserve kudos.
- There have been outstanding accomplishments over the last year, with added stations and better data collection. The project is moving to automated data collection, which can speed up the process and avoid human error. Development of a disaster recovery plan is a good addition to the project scope.
- The project has succeeded in bringing all California stations onboard its system and made good progress on the disaster recovery plan.

Question 4: Collaboration and coordination with other institutions

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

- Overall, collaboration is impressive. The project has seven car companies providing valuable input as well as station operators.
- Collaborations could not be improved; the project is working with all known stations in the state. New funding calls from state organizations require that all new projects participate and will ensure complete coverage is maintained.
- There is excellent collaboration with the right stakeholders.
- Key stakeholders are all participating, which is further validation that the project is necessary.

Question 5: Proposed future work

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

- Adding new data fields will greatly increase the functionality of the application.
- The project is supporting the development of additional stations in California and elsewhere through Air Liquide. At the moment, the database can only process four pieces of data. The team needs to develop a capability to add data.
- The goal of collecting cumulative customer and detailed station data, although extremely helpful to industry, may not be welcome by OEMs or station owners, such as APCI. Therefore, it will be another feather in CaFCP’s cap if this can be pulled off with full buy-in from the stakeholders. Also, although station locators for electric and CNG vehicles are available, having all of this information on a single platform will be helpful.

- Besides real-time reporting, the project team may also consider engaging legal or social media experts regarding sensitivities surrounding privacy and customer data collection and use.

Project strengths:

- Strengths include 100% participation in California of operating stations. The project also has good partners such as the CaFCP.
- The project provides vital data to customers, which could increase acceptance of hydrogen as a vehicle fuel. New projects funded by the state will require participation.
- The project strengths are its relevance, timing, and strong partnership with vendors.
- It provides a much needed solution for three alternative vehicle areas.

Project weaknesses:

- Because this tool may be used broadly for many years, the project is encouraged to leverage multi-OEMs and station provider IT expertise, especially using those that have a lot of remote data collection experience and lessons learned. There is no need to constantly be reinventing the wheel from scratch.
- It would be helpful if the project could make a clear boundary between its effort and those of the OEMs.

Recommendations for additions/deletions to project scope:

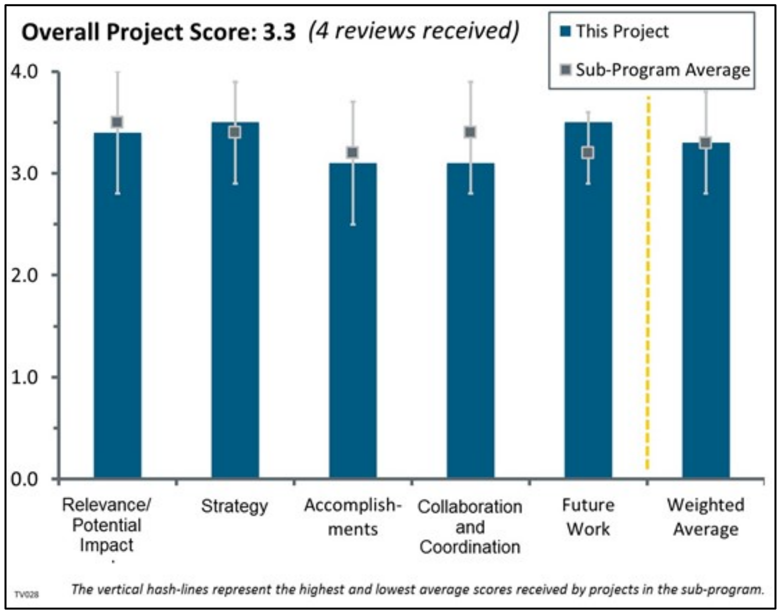
- The team should claim victory on this project and publish the results for all to see and use.
- The project should continue work to increase data fields that can be used for analysis.
- It looks like the SOSS station status criteria considered only one tank size (which appears to be for light duty passenger cars). Therefore, the relative unit of 95% state of charge (SOC) can potentially send the wrong information for vehicles with larger tanks, such as buses. Therefore, the project team should consider adding one or more tank-size categories and calculate the 95% SOC accordingly.

Project #TV-028: Advanced Hydrogen Fueling Station Supply: Tube Trailers

John Aliquo; Air Products and Chemicals, Inc.

Brief Summary of Project:

This project supports the U.S. Department of Energy (DOE) objective of developing and validating advanced hydrogen tube trailers for the hydrogen and fuel cell market. Investigators will design, procure, construct, and demonstrate a new composite over-wrapped pressure vessel to increase the pressure capability of tube trailers. Increasing hydrogen delivery pressure to >586 bar (8,500 psig) can raise hydrogen delivery capacity, reduce the need for compression at hydrogen fueling stations, and lower overall hydrogen delivery cost.



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 very relevant. The number of hydrogen stations is on the rise, and the majority utilize delivered gaseous hydrogen.
- The project will develop hydrogen tube trailers at higher pressures. This could be a big benefit to the industry by reducing the need for compressors. At this point in development, compressors are responsible for the majority of station issues.
- There is a clear need for high-pressure tube trailers, and this project addresses the need directly. There is no fundamental technology barrier to conducting this design and fabrication project. Air Products and Chemicals, Inc., (Air Products) could do the entire effort on its own without U.S. Department of Energy involvement. However, considering that there is a national need for such a product and the economics alone have not led Air Products to pursue the project, this is an appropriate use of research and development dollars (especially considering it is almost 50% cost-shared).
- The project has the potential to minimize some of the expense associated with station compression if the U.S. Department of Transportation (DOT) allows these high-pressure trailers to move on major highways and pass through tunnels.

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.

- Air Product's successful design, construction, and implementation of the 520 bar tube trailer provide a lot of confidence in its project plan for the 586 bar composite tube trailer.
- The approach is a good plan with adequate decision points for moving forward.
- The approach is logical, straightforward, and appropriate for the largely engineering task proposed.

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.

- This is a new project, so it is not expected to have accomplished much at this point.
- The project has just begun; as would be expected, there have been no substantive accomplishments to date.

Question 4: Collaboration and coordination with other institutions

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

- The project involves good partners that are committed to the work.
- The proposed collaboration is appropriate. It is important that the project engage a vessel manufacturer.

Question 5: Proposed future work

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

- The majority of the work plan is included in the future work. This is expected of a new project.
- The future work plan is appropriate for the project.

Project strengths:

- The development of higher-pressure tanks that can be delivered to sites could be a benefit to the entire industry. Reducing the need for compression could lower cost and reduce downtime due to compressor issues.
- A project strength is the straightforward, direct work plan proposed, i.e., design, build, and test a high-pressure tube trailer truck.

Project weaknesses:

- For the project to be of most benefit, the results need to be widely distributed as opposed to being tied to patents specific to one company.
- The goals of the project are not adequately specific: the team does not have a cost goal at all, and its pressure goal is >586 bar (although it implies wanting >700 bar). Its Task 1 go/no-go is also vague, stating only that it will “prove the technical and cost viability of >586 bar,” but the project does not say how such viability will be determined.

Recommendations for additions/deletions to project scope:

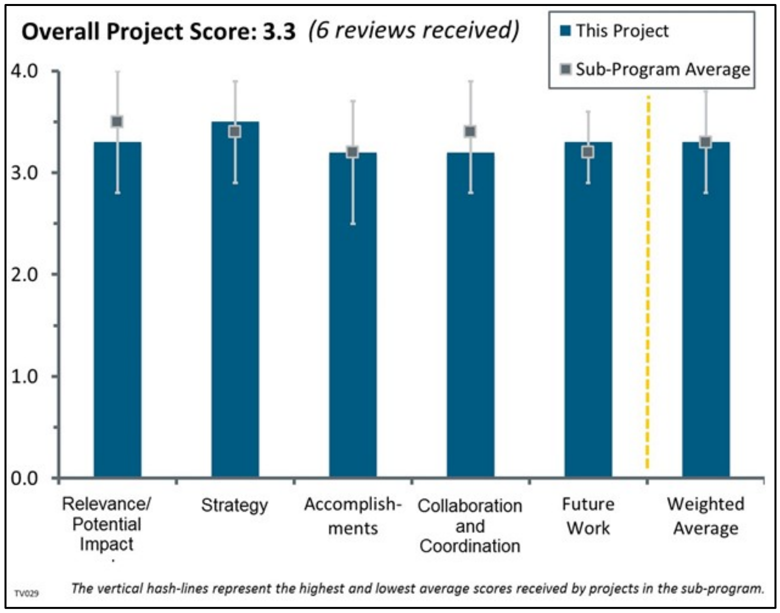
- Getting DOT approval for moving the tube trailers on the road is critical to the success of the project. The team should engage DOT officials as soon as possible to ensure this process does not stall the work and result in excessive delays.
- A specific system cost goal needs to be added.

Project #TV-029: Performance and Durability Testing of Volumetrically Efficient Cryogenic Vessels and High-Pressure Liquid Hydrogen Pump

Salvador Aceves; Lawrence Livermore National Laboratory

Brief Summary of Project:

The objective of this project is to demonstrate performance limits for cryo-compressed storage and delivery technology in the most adverse conditions. The project will manufacture small, durable vessels with thin insulation refuelable to high density with a liquid hydrogen (LH2) pump. Cryogenic pressurized hydrogen storage and delivery provides safety, cost, and weight advantages over alternative approaches to long-range (500+ km) zero-emissions transportation.



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.

- Cryogenic pressure vessel technology has been worked on for many years with noticeable progress, so extending it to 700 bar refueling makes a lot of sense. What is learned from the thermomechanical lifecycle and strength testing adds to the knowledge needed to improve compact vessel design, safety, and durability. If successful, this project could lead to substantially increasing hydrogen density storage and vehicle range.
- Given that fueling systems are not yet fully standardized, even for type of fuel, a discussion about LH2 storage vessels and pumps is justified and necessary.
- LH2 offers unique benefits compared to gaseous hydrogen (GH2): the commercial hydrogen industry uses LH2 for long-distance transport, the suggested technology has merits to make good progress toward U.S. Department of Energy goals, and there are very good technical parameters for storage. The use of the vacuum jacket concept is unique, but there needs to be manufacturability validation. The composite vessel design with LH2 is good.
- This technology evaluation is of high value to increase energy density storage and reduce cost for fuel cell electric vehicle (FCEV) commercialization.
- The description of barriers should be focused on those such as storage cost and not the generic barriers of storage and infrastructure.
- There does not appear to be much interest in onboard LH2 storage by automakers beyond BMW. To stay relevant, the project team should make a convincing techno-economic feasibility case for LH2 over other options.

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.

- Based on its presentation, Lawrence Livermore National Laboratory (LLNL) is clearly and sharply focused on barriers, and it would be difficult to offer suggestions for improvement.

- The test facility at LLNL appears impressive. It would be helpful to tie explicitly the capabilities and novel/unique features of this site with the goals and objectives of the project.
- Safety is the main concern with this project, and the principal investigator, in collaboration with the Linde Group (Linde), did an excellent job in completing the hazard and operability study (HAZOP) and managing the risk, as demonstrated by the safety features designed into the hydrogen test facility.
- The project has a good technical team, and Spencer Composites and Linde make the team stronger. The recycling of GH2 at pressure and the re-liquefy strategy is unique. It can benefit more from a solid-state electrochemical compressor compared to mechanical; the 100 kg/h rate is a good and achievable goal. The finite element analysis (FEA) model approach is sound. The 1,500 bar test and data analysis approach is satisfactory.
- There is ongoing work to evaluate stress of vessel, life-cycle calculations, safety, and the cost of high value necessary for FCEV evolution.
- The technical approach is sound and logical. However, it would be beneficial to the project to get input from other automakers. The team should also assess the refueling and infrastructure cost of the technology compared to the conventional compressed gas option.

Question 3: 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.

- Using 350 to 700 bar to get 80 g/L hydrogen seems achievable. Fatigue analysis data show fiber and metal synergistic behavior that can be economically advantageous. BMW support in FEA simulation is beneficially utilized. Plastic fatigue potential to improve design is a good finding. The team has done a good job in designing, building, and testing multiple vessels and improving them further. The hydrogen test facility and its use for safety evaluation are valuable.
- Work to extend from 350 bar to 700 bar continues with stress and life-cycle testing of the vessel and liner. The hydrogen test facility is of high value for future work associated with testing for leakage, durability, permeation, and cycling. This work occupies a limited niche but is of high value for ultimate evolution of FCEV commercialization.
- The technical accomplishment is good, especially the establishment of the test facility and the cycle testing of multiple cryogenic vessels.
- While the presenter and the presentation were clear, direct, and very well organized, the presentation only glanced upon the manufacturability of the systems discussed. The subject was brought up during the presentation, but there was no discussion or follow-up, and the question was reinforced as an issue with the discussion on slide 9 about failures due to productivity. On slide 7, there was a brief discussion of testing cryogenic storage vessels and their design for 6,400 cycles (fill/refill cycles) to provide a 4x safety factor for 1,600 cycles. Not until during or just after the question-and-answer session was it explained why 1,600 cycles was chosen (it is believed to be the target necessary to achieve 500,000 miles lifetime per vehicle). That said, the numbers presented are questionable. Given the numbers presented (a target of 500,000 miles per vehicle and a design of 1,600 fill cycles), LLNL is assuming about 312 miles per tankful, a target number consistent with a typical gasoline internal combustion engine fill-up. However, as someone with extensive FCEV driving experience, the reviewer believes that range anxiety is such that an FCEV driver will fill up the FCEV before the fuel tank is less than a quarter full, and at times a driver will want to fill up the FCEV with a tank that is half full or even fuller. Therefore, filling up at a quarter of a tank may indicate that 1,600 fills will only allow an FCEV to be designed with a 375,000-mile normal driving range, and filling up at the half mark drops the range to 250,000 miles. Laboratory testing is one thing; the real world can be a little different.
- Five cryogenic vessels have been tested with noticeable improvements in cycle life.
- The update would benefit from a table of goals and timelines vs. progress toward those goals and schedule. It is not clear whether the project is on track or behind. From slide 9, it is not clear whether the second quarter/fiscal year 2016 milestone, "Complete 1,000 accelerated thermomechanical cycles on two 65 L 700 bar, 80+% volume ratio vessels," is completed. It appears that it is not. Slide 18 showing goals/ milestones in 2016 should go earlier, prior to Accomplishments.

Question 4: Collaboration and coordination with other institutions

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

- The team is composed of strategic partners, each bringing a unique set of skills essential to the success of the project.
- The project has a small but tight team; in this case, adding additional members would probably be counterproductive.
- The project makes good use of Linde's capability in HAZOP for the hydrogen test facility. There is a need to show benefits more quantitatively during storage, dispensing, and use.
- Work with partners, including BMW, is of high value, but additional partners are needed.
- The fact that storage and delivery technical teams are co-funding the project is positive. However, broader collaboration and buy-in from multiple automakers beyond BMW are needed for any chance of commercializing LH2 storage.
- It is good to have BMW's participation, but the work is more fundamental than just near-term automotive support.

Question 5: Proposed future work

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

- LLNL is performing a needed task and appears to be doing a good job.
- The project is of high value for testing, evaluating, and commercializing cryogenic storage. Goals are relevant for industry use. Additional work for comparison with gaseous storage is needed.
- The proposed work is sound, especially testing of the thin liner vacuum jacket. However, the quantitative value of carrying out the 1,300 bar test is not clear.
- It is not clear what the thermodynamic benefits of LH2 are relative to GH2. It will be helpful to show feedback on manufacturing input comments and BMW input on future potential, capital cost, and maintenance costs.
- Slide 15 is not really describing challenges and barriers but rather tasks and goals. More informative would be a table of goals with a column of explicit challenges and barriers for each goal. For example, it is not clear whether the key challenge is the thin liner or high fiber fraction and why that is true. It is not clear whether the failure mechanism(s) is known.

Project strengths:

- Strengths are that the project is relevant, the approach seems reasonable, and the test facility looks promising.
- A project strength is the fundamental testing of cryogenic storage for commercial use with increased energy density. BMW as a partner is a significant strength, but additional partners are needed. Development of a test facility for additional testing of applications is of value.
- A strength is the strong team, which includes a vessel manufacturer, an end user, and a pump supplier.
- The good team is a strength. LH2 may offer new solutions for FCEVs.
- Having its own LH2 test facility provides an excellent capability for hands-on learning.
- The team clearly has a strong understanding of the issues.

Project weaknesses:

- Not having other automakers on board on LH2 storage could be viewed as less attractive for large-scale deployment.
- The project needs to show more data and analysis of why LH2 was selected.
- This project may not lead to a commercially acceptable vessel design after many years of expensive research and development.
- The project needs more commercial partners for analysis and demonstration. Other weaknesses include complex technical issues regarding durability of the pressure vessel and thin liners that will require

additional time and funding to address. Also, there is a need for a detailed cost analysis and comparative analysis with gaseous storage. There is also a need for robust and repeated testing of pump, pressure vessel, and thin liners to increase confidence for commercial applications.

- The status, strategies, progress, or linkages of the various tasks are not very clear. For example, slide 7 shows “durable design modeling with safety factor of 4.” Slide 9 shows a vessel overview. It is not clear which vessel corresponds to the safety design with a safety factor of 4. It is not clear what the approach is to address the tank performance issues. It is not clear whether it is possible to correlate failures with the modeling. For the goal of “Test degradation by pumping 24 tonnes LH2,” it is not clear what the key approach/barrier is or why the team is not showing any data on this. It is not clear whether the HAZOP review was a requirement before conducting this work.

Recommendations for additions/deletions to project scope:

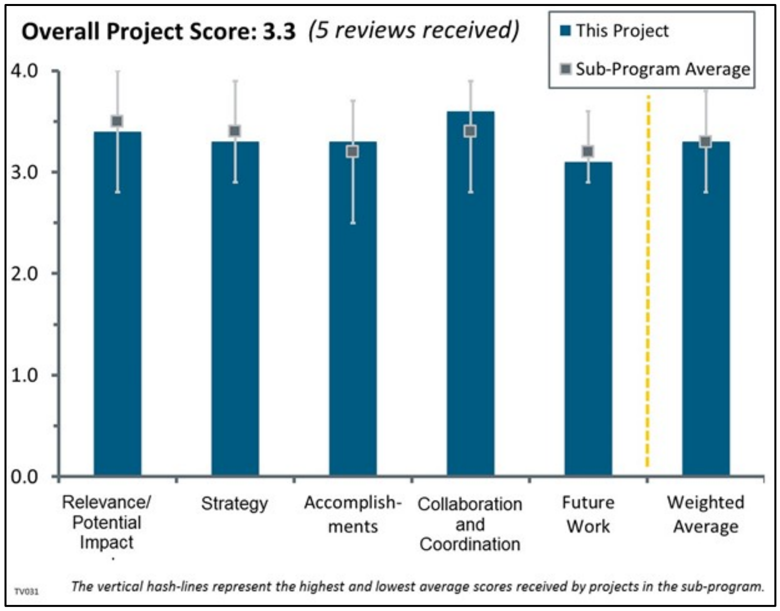
- It would be very helpful to see a more organized/structured presentation approach. Other improvements include the following: (1) specific technical goals/timeline mapped to specific progress, (2) a description of key learning and troubleshooting, and (3) a strategy and approach to move forward to address key failure modes/risk areas, plus some dispositioning of observations (high risk vs. low risk, confidence level to address issues, etc.).
- A cost analysis for commercialization and a comparative analysis with gaseous storage would both be of value. Other recommendations include repeated testing to increase confidence and increased collaboration with industry partners.
- The project should carry out infrastructure cost analysis. The project should seek out input from other automakers.
- The project should have a stakeholder workshop to share results and the deployment strategy.

Project #TV-031: Dynamic Modeling and Validation of Electrolyzers in Real-Time Grid Simulation

Robert Hovsapian; Idaho National Laboratory

Brief Summary of Project:

This project is conducting a business case analysis of electrolyzer-based hydrogen fueling stations. The purpose is to validate the use of hydrogen-producing electrolyzers as beneficial to full-scale fuel cell electric vehicle (FCEV) deployment. Researchers will use first-of-a-kind, distributed real-time simulation with hardware-in-the-loop (HIL) model validation to examine electrolyzer participation under dynamic grid conditions.



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

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 validates electrolyzer response time data for analysis to determine the best fit of an electrolyzer in a support role to the grid.
- The relevance and potential impact of this project is resolving the application of electrolyzers for grid services and hydrogen generation.
- This project aligns well with the Hydrogen and Fuel Cells Program.
- This project addresses a key question for hydrogen generation and storage. The potential benefits for grid support are not clear since, in the future, greater amounts of renewable power may need to be curtailed.
- There appears to be a great deal of measuring occurring, but the presentation and briefing were not clear on why the measurements were necessary and/or beneficial. One example is on the chart titled “Accomplishment, Demand Response and Electrolyzer Performance.” That chart provides six different graphs, but a briefing was provided for only one graph, and the graphs’ importance was not provided for any of the graphs.

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.

> It seems that a key focus should be on future potential revenue streams with much higher levels of renewable penetration. If the economic analysis at 2015 utility rates and the 2015 California renewable profile for PV and wind is not that favorable, then it seems that this analysis is incomplete and does not tell the full story. This “future” scenario is of course uncertain in its offerings of grid support markets—perhaps some learnings from Germany could be utilized. However, one can see the value and learnings that can emerge from both a hardware demonstration and economic analysis, and the overall integration and execution approach appears well-thought-out.

- The grid modeling approach is not associated with any specific grid system. It seems most likely that not all local grids are the same. It is not clear how the grid modeling will take into account interaction with different grid systems, i.e., how the grid for California will interact with the grid for Colorado. Perhaps the project assumes one giant grid for North America will be modeled. Better definition is needed. Regarding the electrolyzer interface, it is not clear whether the project is discussing an electrolyzer or an electrolysis cell; one has balance-of-plant (BOP) hardware and controls (electrolyzer), and the other is just an electrochemical cell. They will have different response times and different vulnerability to surge currents. It is not clear what a "Utility/Aggregator" is. The presentation should at least attempt to educate the reviewer. A 500-hour demonstration does not provide reliability data, decay data, or efficiency data other than for the beginning of life. The project should consider 4,000 hours to 8,000 hours (~one year).
- The key barrier/risk (front-end controller) in this project has been identified.
- The project needs to look more at the BOP spin-up times as the electrolyzer scales to higher power output.
- Project evaluation is troubling as the use of metrics is not well explained nor was an explanation provided as to how or why results mattered or what the metrics goals should be.

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 category is excellent to outstanding. Good progress is reported on all fronts. It is requested that the project spell out/define RTDS next year.
- The data are very promising.
- There may be a good deal of good, if not great, work being performed under this project, but neither the oral presentation nor the written slides support that position well. The what, the why, and even the how were not well detailed. Numerous data were provided but not the managerial sense behind the data. The lack of clarity cannot support a higher grade nor does it justify a lower score.
- Progress is adequate. It is not clear how Milestone 2 will be accomplished on September 2016 with only 20% complete to date (June 2016).
- The accomplishment for the electrolyzer handling grid transients or rapid ramp-up was not for an electrolyzer but for an electrolytic cell; there is a big difference. It is concerning that the National Renewable Energy Laboratory does not understand the difference and would report information that could easily be misinterpreted.

Question 4: Collaboration and coordination with other institutions

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

- Including the California Air Resources Board (CARB) and the Pacific Gas and Electric Company (PG&E) is a very good. EnerNOC's interest in how this technology fits into the company's grid program future validates the need for this work.
- Given the nature of the project, there is outstanding collaboration between the two laboratories, the utilities, the universities, and CARB.
- It is excellent that the project is getting real-world and market information from utilities.
- There is collaboration with relevant industry.
- It is not clear why there is no electrolyzer company as a collaborator. It is suggested that Proton Onsite or some other electrolyzer company be considered as a collaborator.

Question 5: Proposed future work

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

- Proposed future work is well planned and addresses the key and critical areas.
- The development of a front-end controller is an acceptable future activity. There are two or three milestones on slides 9 and 10 that still need to be completed. Proposed work is ambitious.

- Perhaps there is a way to explore possible revenue streams with future, much higher fractions of renewable power. The following is from an earlier slide: “Objective: Validate the benefits of hydrogen electrolyzers through grid services and hydrogen sale to fuel cell vehicles for full-scale deployment.” There was not much discussion of the latter (hydrogen sale for FCEVs); the focus was mostly on grid support. Perhaps this objective should be dropped. It needs some further discussion.
- Obstacles or issues were recognized and stated. At the next review, feedback should be added from collaborators on the work described on slide 23.
- The oral presentation and the reviewing of the written documents generate a question as to how all the data tie together, i.e., whether the data is truly beneficial and required or is being gathered for the sake of gathering data. The presentation seems to indicate the latter.

Project strengths:

- Strengths include the demonstration, HIL-style approach, quantification of responsiveness, and calculation of economic benefits.
- This is a well-planned project with a focus on key technology barriers.
- The larger budget for national laboratories is a strength.

Project weaknesses:

- The project does not accurately assess the operation of the electrochemical cell (rather than electrolyzer) in relation to transients. It is not clear how the current controllers respond to transients—or perhaps the current controllers have been bypassed. If the latter, perhaps there are safety concerns. It is not clear how the hydrogen compression pumps respond to the different rates of hydrogen production—whether they run faster or slower and how they know to run faster or slower. If these questions cannot be answered with a high level of confidence, the project managers and DOE should re-evaluate the project.
- There is no real sense of what is trying to be presented. The project should figure out what message is desired and stick to it. Then more data should be shifted from the primary slide section to the back-up slides section.
- The weakness is in current vs. future markets; the focus on current markets is understandable but less interesting and important than future ones.
- Coordination and inputs from collaborators are project weaknesses.

Recommendations for additions/deletions to project scope:

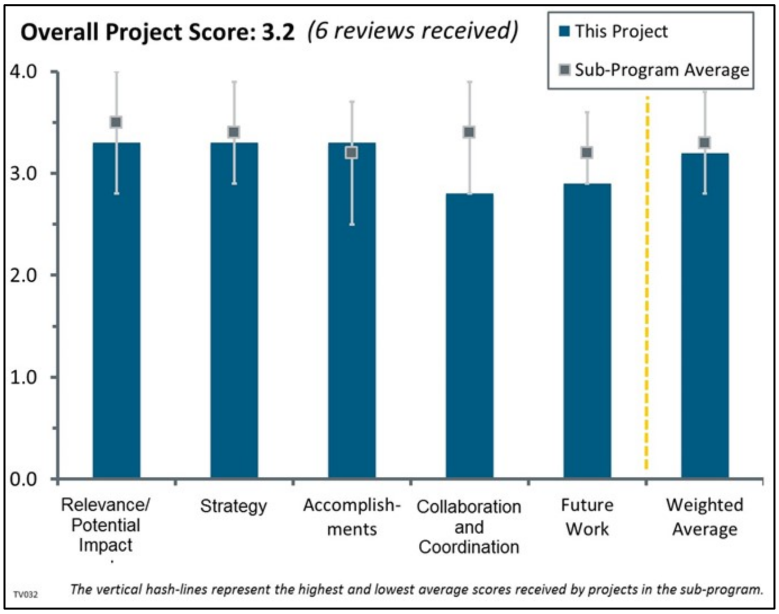
- The following is on slide 10: “Develop suitable PG&E distribution network model…” This activity does not seem trivial. Perhaps there are existing models that can be used or leveraged. More information here on approach/strategies would be good. If the electrolyzer resource would be bidding into an independent system operator (ISO) market, perhaps information from prior ISO markets can be used rather than relying on a new distribution network model.
- The project should consider evaluating the impact that the subsystems supporting the electrolyzer will have on response times. The project should determine whether the pumps, blowers, and valves spin up fast enough to match the stack response time. For example, perhaps there is an optimal size for the electrolyzer and associated BOP, i.e., banks of specific-sized electrolyzers or simply one very large electrolyzer.
- The project does not need additions; it needs focus.
- The DOE should assess the probable success of this project.

Project #TV-032: Fuel Cell Electric Truck Component Sizing

Ram Vijayagopal; Argonne National Laboratory

Brief Summary of Project:

This project aims to develop design concepts for fuel cell electric trucks (FCETs) that are functionally equivalent to conventional diesel trucks in multiple classes and functions. These prototypes will use fuel cells as the primary source for propulsion power. Onboard hydrogen storage will provide the entire energy requirements for the drive, and the battery will be used in a charge-sustaining manner. Investigators will determine fuel cell and battery power requirements, the stored hydrogen mass and total mass of the fuel cell system, and fuel economy and range. The project will conduct analysis to verify whether the concept designs meet real-world requirements.



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.

- The range of truck sizes and applications is quite broad. This project uses automated vehicle analysis to examine a wide range of vehicles and applications. It provides a cost-effective analysis of a broad application space that has previously only been explored in piecemeal fashion.
- The project involved modeling to estimate the sizing for fuel cells and other components for various heavy-duty truck platforms. This is a good start for any original equipment manufacturer (OEM) wanting to develop FCETs for different market segments.
- Establishing a methodology for sizing truck components while balancing the fuel cell with the battery is critical to a successful integration effort.
- This could be the next big fuel cell market after materials-handling equipment. The project is therefore very relevant.
- This project aligns well with the Hydrogen and Fuel Cells Program.
- Relevance addresses objectives and/or questions to be resolved. Relevance does not address barriers identified by the U.S. Department of Energy.

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 project has a good plan for accomplishing the work with available data on market segments. The approach of using the worst possible fuel economy to size the tanks for a 200-mile range is good because real-world efficiency is often lower than advertised.
- The effort uses the existing strengths of the Autonomie vehicle model in a transparent manner that focuses on the key design aspects of truck power system modeling. The effort captures all key aspects.
- The project has excellent approaches in selecting vehicles and developing sizing methodology.

- There is good analysis based on computer simulations of FCET performance.
- It is not clear whether the system design takes into account an inner city driving cycle with frequent stops and starts. It is not clear whether there is enough time to recharge the batteries in such a cycle.
- The four-step process given in slide 5 is at the very top level and does not identify details of how decisions on truck designs will be made. Retrofitting a fuel cell and electric power train into a vehicle is an ambitious approach. The approach is reasonable, and the execution of the approach will be very difficult and will require fuel cell design knowledge (a drop-in fuel cell would be very difficult to find or accomplish). Changes in weight distribution will need to be addressed, as will safety issues, ruggedness of the system components, fuel storage issues, and operator acceptability. It is not clear all of these aspects (and others) are considered in this project.

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 short project is focused and delivers good value and considerable accomplishments.
- The project was completed with all the objectives accomplished.
- Use of the Autonomie model to validate fuel economy and hydrogen needs was sufficient. Data provided detailed base weight and performance information.
- The project lays a good foundation for pursuing FCET design and building of prototype FCETs.
- The project was completed within the time period. Modeling is a great start for designing a new vehicle. The next step is to get an OEM involved in building and demonstrating an actual vehicle.
- Slide 11 identifies the targets and performance requirements as accomplishments; a164 kW fuel cell was identified for applications. It is unclear how this compares to a fuel cell bus requirement. Whether this project can learn from the fuel cell electric bus (FCEB) activities is unknown. Motor sizing is reported; it is unclear how this compares to FCEB projects. The project reports that hydrogen storage capability can be achieved for a 200-mile run. It is unclear whether this is consistent with the FedEx project.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- The project team has good collaboration with other laboratories.
- Collaboration is good considering the limited scope and the short period of performance of the project.
- Collaboration with other groups (other than the National Renewable Energy Laboratory [NREL]) is not apparent, but it is not obvious that additional collaboration is necessary.
- Collaboration partners noted were from the technical community. Collaborators that operate real-world truck fleets should be added.
- It is unclear why no truck companies were involved.

Question 5: Proposed future work

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

- There is no proposed future work, as the project was completed.
- Adding cost data would be valuable. However, it could be a challenge to get users to provide these data. The team should consider how data could be collected from multiple users in a way that protects anything considered sensitive.
- Taking the analysis one step further by conducting a life-cycle cost analysis that incorporates cost and durability would be of value.
- The project should add comparative greenhouse gas reduction results for the load profile and truck types evaluated.
- It is unclear whether there are any plans to build and test prototype FCETs.

- The project does not address how to prove the analyses were correct. The project moved to cost analyses assuming technical analyses were correct.

Project strengths:

- Selecting representative truck applications is quite a challenge. The team did a great job of narrowing it down to a selection that covers a wide variety of platforms. This information on what is required to meet users' needs could be valuable to the industry. The truck market is large and could help drive volume production of components.
- The project seems to have been conducted in a very professional and direct manner. The application of the Autonomie model platform is an enabling technology that covers the breadth of cases examined. Results of the project are summarized in the "FC kW vs. H2 kg Stored" graph, which shows that a ~160–200 kW fuel cell power system would have wide applicability for trucks.
- The project is well-planned and has excellent approaches in developing sizing methodologies.
- There are good computer simulations between Argonne National Laboratory and NREL.
- Analyses were performed.

Project weaknesses:

- Consideration is not given to the ambient temperature effect on the energy storage characteristics of the battery portion of the system.
- It is unclear why there was no truck OEM participation.
- Modeling is only as good as the assumptions. Real-world service will be needed to fully validate performance.
- There is no confirmation the analyses were valid.

Recommendations for additions/deletions to project scope:

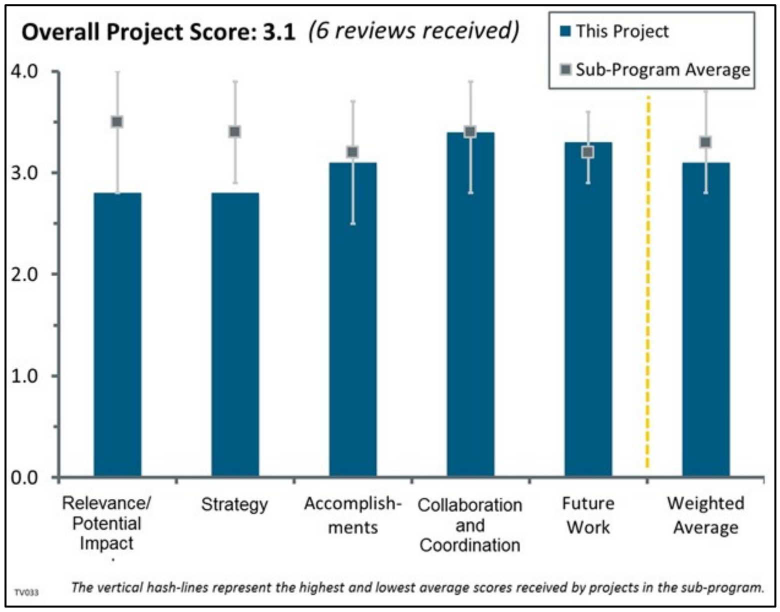
- It appears that the masses of the vehicles (due to fuel cell and storage subsystem weights) were incorporated into the analysis. A sensitivity analysis should be conducted to see how alternate mass and volume scaling would affect system projections (due to weight compounding).
- Research should be added on the impact capturing regenerative energy would have on fuel consumption. The project should investigate the trade-off of the increased weight that would result from providing a slightly larger battery.
- If funds are available, the project should initiate the building of prototype FCETs.
- This project does not appear to have any capability to cross-check the analyses with real-world data. The "relevance" statement in the presentation says, "Verify whether the concept designs meet real-world requirements." However, the team did not identify verification actions in either the "approach" or the "accomplishments" discussion of the presentation. Maybe this project should be linked up with the FedEx project.

Project #TV-033: Brentwood Case Study

Carl Rivkin; National Renewable Energy Laboratory

Brief Summary of Project:

This project has undertaken permitting and construction of a hydrogen fueling station. Investigators will document lessons learned to reduce the time and costs associated with deploying hydrogen fueling technology. This work will address key barriers including lack of knowledge regarding project siting, inadequate installation expertise, and high permitting costs.



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.

- This is a great project, less to provide direct research but more to support the need to provide hydrogen fuel in Washington, DC, a crucial goal.
- This project certainly aligns with the U.S. Department of Energy's goals insofar as it can help clarify issues surrounding hydrogen station deployment. It is somewhat limited in its broader applicability because of its location at a National Park Service (NPS) site, as opposed to a retail station site similar to what the industry is looking to build. That said, it is worth evaluating whether this experience suggests that building stations at government-owned or other non-retail locations could be one pathway for deploying fuel cell fleets.
- Inadequate installation expertise of contractors was mainly a barrier because of the choice of contractors without experience implementing fueling stations overall (conventional fuels and natural gas). Lessons learned from this project are mainly useful to accelerate station implementation of behind-the-fence non-retail stations.
- Many of the lessons learned related to permitting in the Brentwood Case were part of many station reports in 2004–2008. This project probably would have been a stronger presentation if the objective were to strengthen collaborations with other agencies and put a substantial load on the Proton Onsite fueling system to fully test out the technology.
- It is difficult to see how this project, being the production of a case study report in particular about the station development, will be applicable beyond the scope of the Brentwood station installation. While the physical station is useful, the location essentially self-selected to one that does not provide the opportunity to obtain substantial lessons learned outside of this particular installation. Perhaps there will be opportunities to gain lessons learned during station operation, but that does not seem to be the scope of the project. A redirection may be wise at this point. The final deliverable could still include some information about the installation of the station but would ultimately provide significantly more information about the operations.
- The project has little impact on advancing commercial hydrogen stations or even addressing the stated barriers. Part of the reason for this may be that the site selection process was pre-determined for reasons unrelated to project design. The NPS location was too ideal and simple to generate lessons for future stations. There would have been much greater relevance if the case study had been carried out for a commercial station in a busy public intersection with public hearings attended by multiple stakeholders.

Question 2: Strategy for technology validation and/or deployment

This project was rated **2.8** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- Given the nature of the challenge, barriers were identified and met with effectiveness and efficiency.
- The project seems well designed as an “add-on” to the desired goal of building a fueling station in Washington, DC, which has been lacking one. Although applicability to the broader effort to deploy hydrogen stations may be limited, the project seems to have been well designed to illuminate the issues in this environment. It makes sense as a way to provide some added value to that main goal. However, it should be made clear that was its purpose; it was not designed to highlight the most likely issues facing those building hydrogen stations in retail sites. It would be valuable to reach out to other organizations focused on building hydrogen stations to see how the Brentwood project compares to their experiences and perhaps make sure there is a cohesive message getting out about station-building. The lessons learned about the challenges of moving a modular station seem quite useful. It is a bit surprising to learn how difficult even this model is.
- The usability of lessons learned from this project can be expected to be low because of significant differences between working with a federal government agency for implementation of a station on federally controlled property and implementation of hydrogen stations at retail station sites (the presenter made reference to the National Association of Convenience Stores’ (NACS’s) being a target audience for the lessons learned report).
- The station’s location seems to inherently limit the breadth of lessons learned that can be generated for the report. In particular, the strategy seems to be weak because it seems that there is very little guarantee that the station installation process encountered many, if not most, of the challenges that are actually encountered by developers active today and for the foreseeable future. In the end, the strategy seems like it will limit the value of the end product.
- There was no technology to validate, just a permitting process to describe.
- It might have been helpful to this project if the team had read some of the NREL (Wipke) reports related to permitting, building, and deploying stations.

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 did a good job in identifying and documenting the lessons learned with respect to the various permitting agencies that can potentially be used for potential sites on park service lands.
- Given the nature of and need for this project, achievement and progress are outstanding.
- The overall fueling station project will help to better educate the U.S. Department of the Interior on fuel cell technology and potentially provide an opportunity for education and outreach with Congress.
- The project seems generally well aligned with DOE goals. The one weakness is that there are intensive efforts to create hydrogen stations in California (and elsewhere), and this one is slightly out of alignment with the goal of supporting stations that help create a viable retail market. However, it makes sense as a relatively low-cost add-on to a project intended to provide much-needed hydrogen fueling in Washington, DC.
- It was difficult from the presentation to determine just how much of the report work has been completed. It seems that there could have been some discussion at least of the material collected to date and perhaps some status on report writing (even if just at the outline, or similar, stage).
- The report should make it very clear that this was not a retail station implementation and how this station differs from other fueling station installations. The report should include a section about what unexpected items were to be considered by readers of this report (and which unexpected issues did not occur that could have occurred).

Question 4: Collaboration and coordination with other institutions

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

- The right stakeholders and partners were engaged for the chosen location.
- The collaboration so far seems to be appropriate and well-coordinated. The only comment is that, at the presentation, it was revealed during discussion that the subcontractor for installation was not an entity regularly in the business of installing fueling stations. This is not really a weakness, but it does seem like there was a missed opportunity for learning how hydrogen stations compare to stations that provide other fuels in terms of installation, as may have been revealed if the subcontractor had more prior experience with fueling installations.
- From the presentation, it appears that DOE and the National Renewable Energy Laboratory (NREL) coordinated well with each other. They hired Werken and Anderson–Burton, not so much to partner but instead to perform straight construction work. It appeared the NPS role was limited to landlord services and that the DOE–NREL partnership collaborated with Proton OnSite. To some degree, because of the nature of this project, the opportunity for collaboration might be limited.
- Collaboration seemed to be good. The project should consider coordinating messaging and public dissemination of reports on this effort with others, such as the Fuel Cell & Hydrogen Energy Association and California Fuel Cell Partnership.
- It is not clear whether interviews were done with project partners on benchmark information.
- Collaboration with the U.S. Department of the Interior, since the station is located at an NPS facility.

Question 5: Proposed future work

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

- The future plans seem good. Using this as a springboard to other NPS sites is a good follow-on as long as it is determined there is a real opportunity for those stations to be used and support a fleet of hydrogen vehicles.
- Support and planning for future requirements are outstanding.
- The proposed future work seems appropriate for the scope of the existing project and includes relatively low-risk plans.
- After completion of the lessons learned report, the team should consider expanding the scope to real-world commercial stations rather than looking for another NPS location.
- It will confuse NACS membership if lessons learned from this project are portrayed as applicable to retail gasoline station sites or that the project results could be used to benchmark for those sites. Future work presented on slides does not include outreach to the commercial fueling industry, which was identified as a barrier and a challenge. Work with NACS should be added.

Project strengths:

- The key strengths are that this is a fairly low-cost way to provide added value to an effort to build a station. It makes sense to use that opportunity to provide lessons learned. The coordination among participants seemed effective.
- Strengths include fueling in the Washington, DC, area; lessons learned about construction contractors; and lessons learned about overlapping jurisdictions.
- The simplicity of a project meeting a need is a strength.
- Some important milestones have been accomplished by the station itself on which the project is centered, including bringing a station to the Washington, DC, area and making use of a containerized (possibly even modular) station design.
- The project has engaged the right stakeholders to obtain the lessons learned.

Project weaknesses:

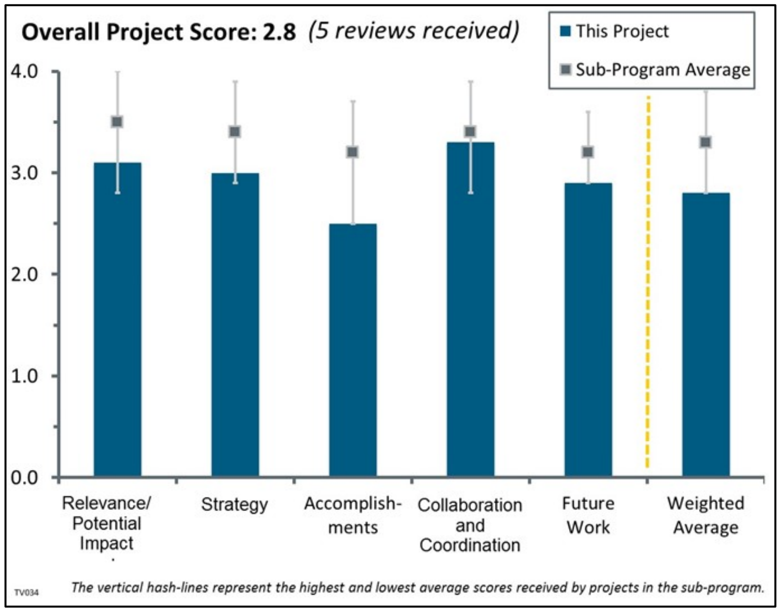
- The key weakness is seeing how this matches up with other experiences in building hydrogen stations. Some coordination with other entities around messaging is needed. It would also be good to make sure that the report does not become a "dead" document on a web portal but perhaps instead can be revised as further experience is gained through other hydrogen station deployments such as this.
- The non-retail and behind-the-fence location of the hydrogen station is a weakness.
- Poor site selection has limited the project impact.
- The final product report does not have broad applicability, and that is a definite weakness.

Recommendations for additions/deletions to project scope:

- An aspect of lessons that can be learned from operations should be added, even if this extends the project budget and timeline. This will potentially have a greater impact than the lessons learned from the installation of the station.
- The project would have been more suited to the Codes and Standards program rather than the Technology Validation program. There was no technology to validate under this project, just permitting.

Project #TV-034: Fuel Cell Hybrid Electric Delivery Van Project

Jason Hanlin; Center for Transportation and the Environment



Brief Summary of Project:

This project aims to increase substantially the zero-emissions driving range and commercial viability of electric drive medium-duty trucks. Investigators will develop and validate a demonstration vehicle to prove its viability and then build and deploy up to 16 vehicles, which will perform at least 5,000 hours of in-service operation. The project will also develop an economic and market opportunity assessment of medium-duty fuel cell hybrid electric trucks.

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 **3.1** for its relevance/potential impact.

- Goals to develop, demonstrate, and deploy longer-range medium-duty zero-emissions-vehicle trucks are important, and the tasks seem extremely relevant for the U.S. Department of Energy (DOE) to support since this has been studied less than light-duty vehicle decarbonization and because biofuels are contentious and may be better applied in other areas, such as aviation. States such as California are very interested, and it is encouraging to see the California Energy Commission (CEC) as a partner.
- The potential impact of this project is outstanding. Phase 1 vehicle development and deployment needs to happen. A go/no-go decision is important, and while Phase 1 is important, if Phase 2 goes forward, this project would be amazing. The potential partners could operate multiple vehicles and collect operational and market data. This would help DOE demonstrate the marketability of these fuel cell vehicles for fleet applications. The project will also be demonstrating hydrogen fueling infrastructure.
- Extending the range and durability of fuel cell electric delivery vans will help with market transformation via advancing potential commercialization in the delivery van sector.
- This project will design, build, and test a fuel cell electric truck that could be adopted by multiple companies in the market segment. The success of this project could help reduce costs for all fuel cell applications by increasing the volume of production.
- The value of the project is good for widespread deployment of a low- or zero-emissions vehicle by a well-established commercial delivery company.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.0** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- DOE covers only the fuel cell part of the project, which totals $3 million. The project partners will raise nearly $8.5 million. This is a good deal for DOE. The overall project will cost around $13 million. Transparency of objectives is outstanding. The range will be 125 miles. Zero-emissions delivery vans are the objective. The project will use one vehicle to demonstrate at multiple sites.

- Converting existing vehicles will show how the new technology compares to the baseline, with changes only to the power train. Full commercialization will require commitment by original equipment manufacturers (OEMs) to achieve higher production volumes.
- Partner contributions from the two California agencies and the United Parcel Service, Inc. (UPS) strongly support project execution. Contractor selection and progress are of significant concern.
- It is not clear why additional funding is needed for Phase 2 (16 additional vehicles). The project could just reduce the number of vehicles and renegotiate terms to avoid delaying another one to two years. The presentation should show the new vehicle configuration that is mentioned. On slide 6 (Milestones), it is very hard to evaluate progress since only $188,000 of $2.982 million of DOE funds was reportedly spent, but it is unclear how much was spent by partners. A mapping slide showing the key project roles and project owners would be helpful since it is a large group of funders and partners.
- If project managers had more experience and were more familiar with the earlier reports related to similar validation efforts with delivery vans, for example the Sprint vans, they might have anticipated some of the issues with service providers and had a better plan to mitigate risks. For instance, identifying back-up service providers for vehicle after-market modifications during the first request for proposals might have saved a lot of time. Also, the installation of fueling stations should be completed well ahead of deploying the delivery vans. Otherwise, the project will potentially incur yet another serious bottleneck.

Question 3: 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.

- UPS has developed a design for an existing walk-in van to be retrofitted with a fuel cell system and storage system. They have completed a lot of due diligence on the size of the fuel cell and storage system as well as the fueling infrastructure. They put out a procurement request and found a replacement partner for the fuel cell. They have done a lot of analytical work this year, but the presentation does not show this. The South Coast Air Quality Management District (SCAQMD) is a partner and has committed $980,000 to building vehicles. The project still needs more funding to complete the building of the fleet. Center for Transportation and the Environment (CTE) activity resumed in February 2016 after Unique Electrical Solutions (USL) agreed to be the subcontractor for equipment design. Complete Coach Works (CCW) out of California will be the subcontractor to USL and will modify the first vehicle. The design for the vehicle looks good.
- The project team is making progress despite early issues with a project partner pulling out. The delay in timing is unfortunate, although understandable, considering the situation. Choosing the new partner through a competitive bidding process ensures the best value and that the partner is committed.
- Progress is slow because of subcontractor changes, initial technology selection, and the need to complete funding.
- It is hard to evaluate this project. Very little to minimum technical progress was reported. The partnership with SCAQMD, as well as with UPS, is cited. It is unclear why UPS is not mentioned in the partner co-funding amounts. Given the delays, the statement “Continually search and identify additional funding sources” seems problematic. This objective does not seem to be a good use of time and funding. On slide 8, it is not clear that the amount of work is commensurate with the DOE charges for the past year. This is something DOE should assess. On slide 9, “evaluate” is used as the verb, which makes this hard to assess as a reviewer. It is unclear what the bottom line is. It is unclear how close the team is to locking in a final design and final design specifications. It is unclear what has changed from last year.

Question 4: Collaboration and coordination with other institutions

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

- The project has excellent partners (as of the addition of the new team member). The principal investigator (PI) has successfully gained additional funding from California agencies that will allow at least half of the planned trucks to be built.
- The project has high value with UPS and the California agencies as partners.

- The partners have already committed funds for this project. Having UPS as a partner is outstanding. USL is fully on board.
- This is a nice group of project sponsors, especially CEC and SCAQMD. These sponsors provide the ability for the project to access the hydrogen fueling station projects in various parts of California, which is vital to the success of the project.
- Slide 15 seems incomplete: "Identified and evaluated additional upfitter/refurb contractors to support project; will partner with CCW" and "Collaborating with DOE and Argonne National Laboratory (ANL) to validate vehicle modeling and simulation results—ANL focused on vehicle configuration, component sizes, and operating profile of potential deployment routes." These do not seem to be shown on slide 15. A task flow showing key tasks, deliverables, and dates would add clarity, especially for design and safety standard reviews.

Question 5: Proposed future work

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

- The project has taken responsibility for getting funding in advance and developed a realistic design and solid partners in order to build the vehicle. Codes and standards are going to meet the safety standards, and this will be part of any redesign. The project has already started to determine how to finalize the retrofit of the vehicle to demonstrate it. This project needs to go forward.
- The plan is sufficient to complete the objectives. Having a go/no-go decision point for acquiring the additional funding is good.
- It would be good to see the station contracts completed.
- The goal for a 125-mile range with a hydrogen cylinder is of value, but subcontractor issues for technical build-out, lack of progress, and financial controls are concerns.
- This comment is about the following: Task 1 –Vehicle Build: Complete design and hold final design review [2Q 2016]; Order long-lead components [2Q 2016]; Build vehicle and validate battery-only operation [3Q –4Q 2016]; Task 4 –Project Management: Update and Review System Safety Plan and Hazard Analysis [2Q 2016]. It is unclear whether these tasks have been completed or if there is some other status. Without understanding the current status better, it is hard to comment on future work.

Project strengths:

- The PI has experience in putting teams together to complete the work and meet project objectives. His excellent skill in managing the unforeseen changes with partners is an asset to the team. Having a committed demonstration partner is a plus. The demonstration period of two years will allow a good data set to be collected for analysis. The trade study is of high value to inform the industry. Good variability of routes for the demonstration will help test the vehicle under many conditions.
- There are excellent project partners and advanced funding. The vehicle design has progressed and is ready to go. There is a realistic timeline for implementation of this project. The team includes fuel cell integration experience.
- Team members, including UPS and the California agencies, will make valuable contributions to judgments about project acceptance.
- Relevance is high. The two-stage process seems sensible, and the team elements seem to be there.

Project weaknesses:

- The project has certainly had its share of setbacks, which is to be expected from high-risk pre-commercial research projects. Probably having partners with greater fuel cell vehicle validation experience would have been helpful in avoiding some of the pitfalls, but the project team contributed little money while learning valuable lessons.
- The predicted range of the truck seems low, although the presenter stated it would satisfy 95% of route requirements. It would be good to know whether the use of auxiliary loads (air conditioning) will lower this range.

- Weaknesses include completion of funding, change in subcontractors, technical issues for hydrogen storage on the vehicle, and lack of progress.
- The team has not done a good job of conveying the current project status, or overall progress (not just DOE work), or progress toward goals for the overall project. Information about the progress shown by DOE's partners could be conveyed in one or two slides and would add tremendous insight into the project. It is unclear whether the project is on track to meet the stated timeline. Stating in the project slides that seeking more funding is a key objective does not build confidence and puts the project at further schedule risk.

Recommendations for additions/deletions to project scope:

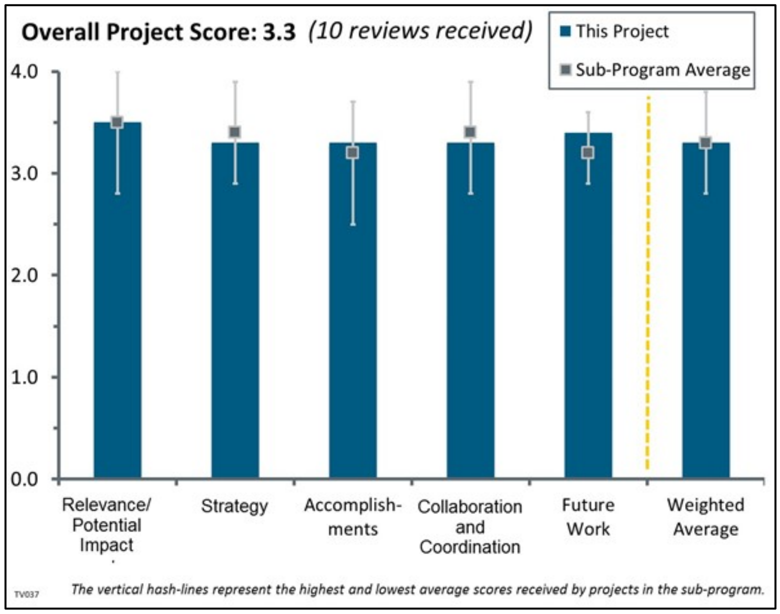
- Acquiring hydrogen tanks of the right shape and size to increase range is a challenge because of the high cost of designing and testing new tank configurations. The team might explore the possibility of teaming with a tank manufacturer that could provide cost share on the development work. This new tank design would need to be applicable to multiple vehicles to make the development worthwhile to the tank OEM.
- The project would be well served with additional project management.
- The project should not seek additional funding but perhaps just go with six trucks instead of sixteen. In addition, CTE should not seek additional funding to allow for investment into custom 700 bar tank development for Phase 2 vehicles. That objective would be nice to have but is not required. The project needs to focus its time and attention on delivering a working, safe, reliable truck with extended range. It is not clear how close the project is to this objective, and the team should not be distracted from this goal.
- Someone should inquire as to whether FedEx and DHL Express are interested in buying into this project.

Project #TV-037: Hydrogen Meter Benchmark Testing

Michael Peters; National Renewable Energy Laboratory

Brief Summary of Project:

Hydrogen flow meters are struggling to meet the 1.5% accuracy requirement for motor vehicle fuels, impeding the sale of hydrogen by the kilogram. This project will design and build a laboratory-grade gravimetric hydrogen standard; conduct high-pressure hydrogen testing of commercially available flow meters, replicating conditions specified in the SAE International J2601 fueling protocol; and report on flow meter performance against National Institute of Standards and Technology (NIST) Handbook 44 (HB44) requirements and California Code of Regulations accuracy classes.



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.

- Accurate metering is one of the fundamental enablers of hydrogen fueling infrastructure deployment.
- This project is critical to meet SAE J2601 standards for refueling using commercial meters. It will directly address a barrier to technology commercialization, as it is important to know whether there is a technology that meets the 1.5% accuracy requirement.
- If done properly, this project can be very useful in progressing hydrogen fueling toward mass point-of-sale commercialization.
- The project has two overarching objectives: (1) to inform the U.S. Department of Energy (DOE) of the status of the present metering technology, and (2) to act as a kind of “innovation laboratory” to encourage development of new solutions. In the present status of fuel cell and hydrogen technologies, it will be hard to have consolidated market players to invest in this development. In other words, established companies in the field do not see accurate hydrogen metering as a valid business case. To be successful as catalyzer of innovative solutions for this technology, some plan will be needed.
- This is a critical element in enabling the deployment of a hydrogen infrastructure. It is crucial to have accurate meter technologies to be able to sell hydrogen to the consumer. The presentation says that the resolution of the scale has been enhanced to +/- 0.2 g, which would be okay if it were clear whether the accuracy is +/- 0.2 g. Enhancing the resolution says nothing about the accuracy (which is different from precision and different from the resolution of the device). The meaning of “1% relative” is that the meter must measure 1% at every flow rate; the lowest mass injected is 20 g, so the device must measure 0.2 g. This requires the system to be accurate to 0.067 g (using the factor of three or better, as stated by the principal investigator [PI]). This system will likely not deliver on what is eventually going to be needed. However, as an initial testing device to get an “idea” of where meter technology is, this will do.
- The project is addressing important needed work in metering for station validation, but the strategy could use some slight improvement: (1) it was not quite as clear as it should have been how this work will directly translate to improvements in meters in hydrogen stations, as opposed to metering methods for station validation, and (2) it presupposes that any existing inaccuracy in validation metering may be greater

than the inaccuracy of dispenser meters themselves. This may be the case, but evidence for this was not presented.

- It is important to advance an accurate and reliable way of measuring hydrogen at stations. However, the goals of this project do not seem to be well defined. It is not clear whether the goal is to improve accuracy of existing flow meters or to compare/validate the performance of commercial meters or to develop a protocol for testing the accuracy of hydrogen flow meters. Also, some of the stated barriers do not appear to be addressed by this project, at least not at this stage.
- While the low tolerances are certainly a technical challenge, it would be going too far to say they are “impeding” the sale of hydrogen, as there are (temporary) countermeasures to address this issue in the near term. There are not clear reasons to look at meters that are not in practice/use at stations. It seems that it would be more relevant to use actual meters that are in service and help improve those. A separate project can be initiated to look at potential metering technology.
- While attaining a better understanding of flow meter accuracy is a reasonable step, the limited number of flow meters being tested and the likely proprietary nature of results will result in limited project impact. If the project were geared toward developing a test “standard” or methodology to evaluate any flow meter and included some industry agreed-upon acceptance criteria for the testing, it would be of considerably higher value.
- There is still much practical work to do.

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 hydrogen infrastructure testing and research facility at the National Renewable Energy Laboratory (NREL) is an appropriate testing site for the project. The team conducted an adequate preliminary assessment of flow meter technologies to down-select the Coriolis meter. The test plan, which includes the three types of tests, is adequate to test the conditions that would be experienced at hydrogen refueling stations. The system design and the pre-test are well designed to ensure that measurements are correct and can be replicated.
- The strategy appears to be well developed. In particular, the broad range of operating conditions and the inclusion of pre-testing will help ensure future confidence in the work’s final determinations. The strategy is thus comprehensive and provides added value to the project.
- The approach seems to be well conceived to address the barriers.
- The approach at this stage of this work is good. This work needs to eventually get to the point at which dynamic measurements and meter validation are made under a true SAE J2601 fill profile, including initial pressure pulse (20 g) and National Fire Protection Association Hydrogen Technologies Code 2 (NFPA2) intermediate stops for leak checks, not just measurements under similar conditions. The flow meter needs to be accurate to 1.0% (acceptance) and 1.5% (maintenance) (under HB44 and Recommendation 139 [R 139] of the International Organization of Legal Metrology [also known as Organisation Internationale de Métrologie Légale – OIML]). California has temporarily relaxed this, generating additional accuracy classes, so for the immediate term, the accuracy of the validation device can be relaxed a bit to be more in line with the relaxed California accuracy classes. Operators need to be able to start and stop the fill and maintain this accuracy at every point in the fill, not just at the end, so a time-resolved real-time measurement and validation at these accuracies needs to be made; during a SAE J2601 fill, the mass flow rate—not just the end states—needs to be accurate to HB44. What is needed is a very accurate mass flow rate measurement. Arguably, the measurement of the flow rate meter validation system should be ~10 times more accurate or 0.1% relative. During the question-and-answer session (Q&A), the PI indicated he was targeting 3%. It is a start, but a factor of 10 should be a goal for this work. Also, the investigators are attempting to take mass as a function of time (mass flow rate measurement). This is a real challenge and is noted here because focus should be maintained on what is really needed. The presentation says that the resolution of the scale has been enhanced to +/- 0.2 g, which would be okay if it were clear that the accuracy is +/- 0.2 g. Enhancing the resolution says nothing about the accuracy (which is different from precision and different from the resolution of the device). The meaning of 1% relative is that the meter must measure 1% at every flow rate. The lowest mass injected is 20 g, so the device must measure 0.2 g.

This requires the system to be accurate to 0.067 g (using the factor of three or better, as stated by the PI). This system will likely not deliver on what is eventually going to be needed. However, as an initial testing device to get an “idea” of where meter technology is, this will do.

- The project is answering well to the present needs related to metering technology. Independent assessment is required. During the presentation, it was not possible to check how far work in this field that is already available has been considered and used. Probably it is too early now, but when experimental results are available for evaluation, the already available work achieved in Europe and Japan will have to be used as benchmarking.
- This is a good project, but it needs careful balance so that scientific laboratory and vehicle original equipment manufacturer perspective will not get in the way of realistic meter operation in the field (laboratory-grade gravimetric hydrogen standard vs. replicated conditions specified in SAE J2601 fueling protocol vs. testing designed to span the range of hydrogen gas conditions that would be experienced at current light-duty hydrogen vehicle fueling stations vs. noted “no pre-chilled hydrogen/testing at ambient conditions” vs. meter location in system). It is not clear how many hydrogen tanks and kilograms of capacity are in the system.
- This project does address some of the stated barriers, assuming the objective is to develop a protocol for testing commercial flow meters for a hydrogen station environment. However, the technology deployment pathway is not clear. It is not clear who the beneficiary will be: flow meter flow manufacturers, regulatory agencies, or research laboratories.
- It is great that the team is consulting with NIST, but for commercial systems, the team is encouraged to add Coriolis if the company is willing to participate so that a prime suppler can share strategic information and learn from others on the team. Ideally, such an approach will lead to timely adoption of information and improvements by industry. This may already be happening, but the discussions around manufacturer participation seemed a bit vague.
- For a flow meter to provide the most accurate measurements, several performance aspects need to be quantified:
    - Calibration. It is not clear how the meters will be calibrated (presumably simply using factory calibration curves). It is not clear against what standard they will be calibrated, but perhaps the scale system could be used for this. Along these lines, flow meter settings need to be documented.
    - Installation. Flow meters can be very sensitive to installation, including straight runs, orientation, vibration, mounting, etc. This needs to be verified with the manufacturer and reported. Station designers will likely deviate from this, and the effects are unknown.
    - Environment. Depending upon the flow meter type, environmental factors can have a significant effect on accuracy. For Coriolis meters, mounting and vibration can be a factor. For turbine meters, flow conditioning and de-swirl of inlets can have a big effect and need to be addressed.

  From a station designer’s perspective, the greatest interest is in the effect of operating conditions on the ability to provide an accurate fill. For example, station designers would like to know the cumulative errors during a tank fill rather than uncertainty in measured flow rate. A simulated fill protocol with different fill rates, transients, etc., which is then reported in error over a tank fill, would be most valuable.
- It seems that one of the issues with meters is the extreme temperature cycling. It is not clear whether there are plans to do precooled hydrogen (preferably to -40°C). Presumably this has a significant impact on the final outcome(s). When asked about this topic, the presenter stated that the meters are not in the path of precooled hydrogen, and to add that hardware would have added extensive cost and time. While the SAE J2601 standard says that the startup hydrogen mass shall be less than 200 g, it seems that some providers are right up to that limit (as close to it as possible)—this may be useful information for the device-under-test pressure pulse plan. It is not clear whether the project is going to send a temperature signal to the stations (or how to fake out the station on temperatures that do not occur in Colorado).

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 project will provide information to domestic and international hydrogen fuel providers, helping them to understand how commercial meters perform and what corrections need to be made on the control side to improve accuracy. This will directly help accelerate the introduction of hydrogen refueling stations and improve the customer experience.

- If the project maintains its expected schedule, the completion of the project will be very well timed to current and ongoing developments in California and the Northeast. This will help maximize the potential impact of the project. To date, progress appears to be quite rapid.
- Planning and engineering work are good, considering the very tight development timespan. Some work is still necessary to evaluate quantitatively the measuring accuracy of the facility. During the Q&A, it was stated that the facility aims at an accuracy three times better than the required maximal accuracy of the metering device (1.5%). This should be demonstrated as soon as possible because it is essential for project's success.
- This project is just getting started; progress is good. Because of delays in acquisition and permitting, the laboratory has taken longer to construct than anticipated, but delays in this area are actually commonplace.
- System design and the rationale behind it are well aligned. There is one caveat: flow measurements in selected positions 1 and 2 may not work for a dispensing facility in which a cascade storage bank branches off to more than one dispenser.
- Meter selection, system design, and system pre-testing are good accomplishments. Now actual testing has to be done and completed to make data observations.
- System design and pre-testing accomplishments are sound. However, there may not be enough time left for the planned actual test before the September end date.
- The project has an aggressive schedule (12 months), including procurement and testing. It is not surprising that storage vessels and flow meter lead times have put the project behind schedule.
- Because of the long lead time for Type 3 vessels, the project is a bit behind but appears to have some slack in the schedule to catch up during testing.
- It is not clear what the reason is for location 3 of the meter if it is out of scope and causes problems as the meter becomes a heat sink (between heat exchanger and breakaway). When asked during the presentation, the presenter said that the project will focus on positions 1 and 2 initially. Input from industry stakeholders is that there are now efforts to move meters closer to the nozzle. Pressure, volume, temperature (PVT) compensations will inevitably be an inadequate method of improving overall accuracy (90%–95% accurate at best when making a correction for 20–80 g, and that is not good enough). Therefore, station developers will continue to push to locate the flow meter closer to the point of transfer, and flow meter manufacturers will improve the accuracy of the meter with multiple (inlet + outlet) temperature compensation (accounting for temperature change inside the meter) and calibration parameters that are more highly customized and tuned for the hydrogen fueling application, instead of generic steady-state flow applications for the manufacturers' core clientele (the chemical and petrochemical industry). This returns us to the question of the reason for the choice of meter that is not in use (based on cost, per a comment from the presenter).

Question 4: Collaboration and coordination with other institutions

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

- The project appears to be coordinating appropriately with many partners. In particular, the use of a testing facility already within DOE's portfolio of capabilities was a good decision for this project. Additionally, the outreach to the international community involved in station development and testing will be particularly valuable.
- This is a good set of collaborators, but it would be good to see a dispenser manufacturer and a fuel provider as part of the team. Sandia National Laboratories (SNL) is part of the team, but no SNL materials expert is listed as a participant. It would be good to see that SNL materials expertise is at least available for occasional consultation. During the Q&A, this issue was posed to the PI, who indicated that he consults with SNL frequently, which is good. This is particularly important since some of these meters (Coriolis in particular) really require a high-strength material to enable thinner wall material, and unless it is high in Ni content, the material will have increased sensitivity to embrittlement. The investigators recognize that materials issues are an area of concern, so having access to SNL materials expertise is important.
- Established collaborations are invaluable for this project.

- While the current collaborations are good, and certainly there is much that can be learned from the experiences of the California Division of Measurement Standards, there needs to be some actual station developers in there, too—those in the field with real-world experience, dealing with this in a retail setting.
- The collaboration with NIST and researchers from national laboratories is excellent. It would be beneficial for the project team to engage the meter manufacturers more.
- There is a good mix of national and international project partners. However, adding a station operator from industry with field experience outside a national laboratory station may strengthen this project significantly.
- It would be helpful to have inputs from the station owners/designers who will need to implement these flow meters. A standard test procedure and test platform that could be used to evaluate flow meter suppliers would be a helpful outcome. In particular, flow meter manufacturers could use this test stand and test procedure to validate performance for direct comparison to other manufacturers.
- The project shows collaboration with NIST and in Europe with the Joint Research Centre. However, other players have already gained considerable experience (the Clean Energy Partnership in Germany), and how far the project has considered these achievements is not apparent.
- The only suggested addition to the list of collaborators is hydrogen producers currently operating refueling stations such as Air Products, Air Liquide, Linde, or other similar companies in the industry.
- It would be helpful if the project would more clearly identify the flow meter suppliers with whom the team is actually working.

Question 5: Proposed future work

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

- It seems this should be a continuous/longer-term project that can constantly keep up with industry and assist with progress. Industry can use this as a tool, a leg up; the project can be a real incentive for industry to keep working on the technology. It is suggested that funding be continued (at some useful level) to keep driving this progress.
- The future work is appropriate for the scope of the project. Coordination with final decision making in HB44 could potentially have a great impact on accelerating station development.
- The future work in the two-year frame of the project is excellent. It is, however, not clear whether the laboratory can be further used as a tester/enabler of developers (in the sense that there may not be very many). Perhaps the development toward a fully certified instrumentation for mass measurement could be considered.
- It is important that the team complete the stated project closeout work.
- It would be good to see a realistic SAE J2601 table or MC Method fill. It is agreed that since the meter needs to be upstream of the chiller, cooling is not necessary. However, a realistic fill without the chiller should be performed—which might not be possible while keeping the tank from over-temperature, over-pressure, or over-state-of-charge.
- Proposed future work is adequate. The only suggested improvement is to receive feedback from companies currently operating hydrogen refueling stations before testing begins.
- Proposed future scope seems adequate to meet the project objectives.
- It appears that the project is primarily focused on milder California weather conditions, hence diminishing its overall usefulness. It is recommended that the project seek additional funding to set up the system for multiple ambient weather extremes, as well as varying vibration conditions since the meters will be on working forecourts. Also, it is not clear how the flow meter components will be stressed to understand flow meter drift over time.
- It is not clear what "Provide validation testing of proposed SAE J2601 slow fill protocol for home fueling and roadside assistance" has to do with proposed future work for metering. This appears squeezed in by SAE J2601 proponents and unfit for work done in this project. This should be eliminated from this project because *if* there is metering for home fueling and roadside assistance, there is no business transaction taking place that requires accuracy applicable to retail fueling.

Project strengths:

- This project represents a first-of-a-kind instrument in the public domain and is trying to tackle a critically important problem: qualifying a mass flow rate meter to HB44 under an SAE J2601 fill. As a first attempt, this project is good.
- The project's strength is that it is helping to answer a major unknown currently being faced by today's station developers. The immediate impact and value potential are very large for this project.
- The project is developing a laboratory-grade meter benchmark and assessing viable (but to-be-conclusively-proven) hydrogen meters.
- The overall direction/intention is positive and will create an accurate benchmark/baseline meter accuracy—in industry, there is a lack of determining accuracy in a controlled environment (although it is done in the field).
- Strengths include use of the hydrogen infrastructure testing and research facility, skilled researchers, appropriate design of experiment, and the inclusion of pretesting.
- The partnership with NIST brings considerable expertise, plus some previous experience with compressed natural gas (although the reviewer may not have heard the presenter correctly about the last point).
- The main strengths are strong technical competence and an expert team, availability of an up-to-date refueling station for testing, and correct timing.
- Strengths include a strong experimental base at NREL and very strong international collaborations.
- The project has a good team and an aggressive schedule.
- The project design and the system pre-testing are well done.

Project weaknesses:

- There are no particular weaknesses in this project.
- There are no significant weaknesses. One potential weakness is that the project is looking only at the case in which each dispenser is served by an independent storage bank.
- The proposed location of the meter (location 1 or 2) is a project weakness. Even though location 3 could act as a heat sink, from a practical/realistic perspective, the meter should be as close to the nozzle as possible. The heat sink concerns will be addressed based on in-field experience by industry. However, locations 1 and 2 may be a good solution for in-laboratory benchmark testing of meters—which does not take away the technical requirements for meters installed in retail stations in location 3.
- The qualification of mass flow rate meters operating in the required pre-testing time specified by an SAE J2601-compliant fill and to accuracy of HB44 is a very challenging problem. However, validation techniques and hardware must be developed to accomplish that task. This project falls short of achieving that in accuracy over the entire range of fill parameters and in its ability to actually execute an SAE J2601-compliant fill (there is no precooling).
- Station developers/industry have moved far beyond this challenge and onto bigger ones (flow meter placement, flow meter internal temperature compensation)—the pace is fast. What is known about the metered accuracy now was not even tested six months ago. Industry may not be as thorough as the laboratories, but industry is solving the problems at free market pace; there needs to be a way for laboratories/DOE to keep up *and* do the needed research and development (R&D) for future applications (i.e., future technologies).
- The stated challenges with project permitting, logistics, and schedule may be detrimental to the project's success.
- A project weakness is the lack of a stakeholder community able to drive and optimally profit from the facility.
- There is a lack of involvement or inputs from the station designers. If this work is to be implemented, their input is needed to coordinate the deliverables to be usable in station design.
- There is a lack of detailed input from industry experts.
- Only one type of meter can meet that 1.5% accuracy requirement.

Recommendations for additions/deletions to project scope:

- The project scope appears appropriate and well designed. There are no additions or deletions recommended at this time.
- This is a first step in establishing a better understanding of flow meter performance. The project could be strengthened by the establishment of test standards or protocols that could be used across flow meter manufacturers, providing station designers with a way to do “apples-to-apples” comparisons of flow meters. Eventually, this could be expanded to create a test standard by which a flow meter could be “qualified” for the application, along with guidance on installation, operation, setup, calibration, etc.
- The objective “Provide validation testing of proposed SAE J2601 slow fill protocol for home fueling and roadside assistance” should be deleted from the project. The proposed location of system design of the developed system will be made publicly available. The project should assess at what range or number of meter units a meter could be commercially profitable. This will be valuable information to add to this benchmarking effort because the information will provide an understanding of what is needed to motivate meter manufacturers to invest in R&D to develop new or more accurate meters.
- A beneficial outcome of this or future research would be correlation factors that account for errors in traditional PVT/ideal gas or equation of state calculations *and* errors introduced by the nature of the gas temperature’s changing as it settles in the heat exchangers (often metal thermal storage, which causes dramatic temperature gradients and transients).
- This system (or project) needs to develop a mass flow rate meter validation system capable of validating an SAE J2601-compliant fill and one that is accurate per HB44. It would be good to see a system 10 times more accurate for validating the meter, but a factor of 3, as the PI has indicated, will be satisfactory. The current system does not have this capability. The investigators are working toward this but are not there yet; this objective needs to be kept in sight.
- The project should consider a scenario in which a storage system serves more than one dispenser through the same main line and priority fill panel, then branches off to individual dispensers that can be fueled simultaneously.
- The project should invite companies involved in the design and operation of hydrogen refueling stations to provide feedback.
- The project should reach out to the broader area of the metering industry, well beyond hydrogen, and organize international benchmarking exercises with similar facilities.
- The likely technology deployment pathway should be clarified.

2016 — Safety, Codes and Standards
Summary of Annual Merit Review of the Safety, Codes and Standards Program

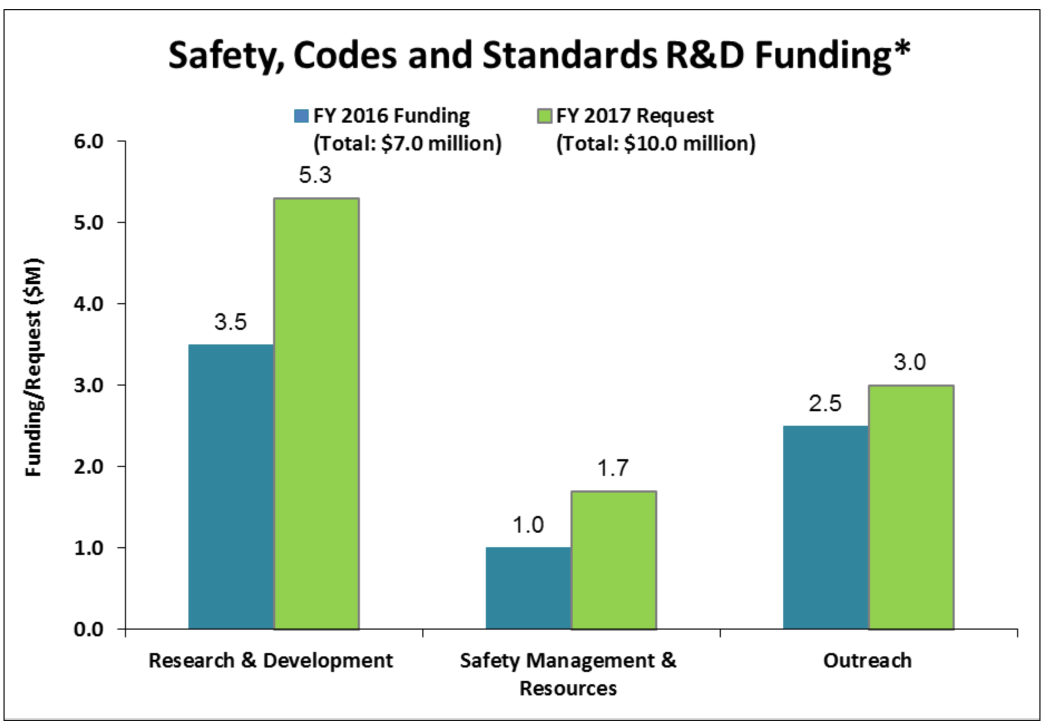
Summary of Reviewer Comments on the Safety, Codes and Standards Program:

The Safety, Codes and Standards (SCS) 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 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 commended the progress made since the previous year and noted that the work of the SCS program enables the accomplishment of the broader goals of DOE and the Fuel Cell Technologies Office. They particularly applauded the balance of near-term and long-term activities in the program portfolio, mentioning progress in such areas as hydrogen behavior, separation distances, materials compatibility, and fuel quality. Reviewers were impressed with the science-based approach and the feedback provided to code development organizations (CDOs) and standard development organizations (SDOs). program engagement of relevant stakeholders such as CDOs and SDOs, both domestically and internationally, was praised. Reviewers felt that more progress could be shown in non-R&D activities. In particular, they stated that the outreach efforts have been "well-maintained" and that continued focus in that area is essential, and they recommended expansion beyond stakeholders to include outreach to the public. Key recommendations for R&D focus included medium- and heavy-duty fuel cell electric vehicles and fueling protocols.

Safety, Codes and Standards Funding:

The program's fiscal year (FY) 2016 appropriation was $7 million. FY 2016 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 2017 request of $10 million will allow the program to broaden its existing R&D efforts and expand its focus on infrastructure-related activities.



* 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 2016, 10 SCS program projects were reviewed, with a majority of the projects receiving positive feedback and strong scores. Reviewers' average scores ranged from 3.2 to 3.7, with an overall program average score of 3.4.

Research and Development: Seven R&D projects were reviewed, earning an average score of 3.45. The highest scoring project in this category received a score of 3.7 and was also the highest scoring project for the SCS program. The R&D category is divided into three sub-categories: Sensors and Component R&D; Hydrogen Behavior, Risk Assessment, and Materials Compatibility; and Hydrogen Quality. The summaries of reviewer comments for R&D are provided below for each sub-category.

Sensors and Component R&D: Reviewer comments for this category were generally positive. Reviewers were particularly supportive of the collaborative efforts and stakeholder engagement for projects relating to sensors and component R&D. The approach to component R&D was praised as being thorough, while the sensor effort was commended for its comprehensive validation plan, though some modifications were suggested. Reviewers recommended that the results of these projects be published to provide guidance on the application of the respective components.

Hydrogen Behavior, Risk Assessment, and Materials Compatibility: The science-based approach to codes and standards through hydrogen behavior and risk assessment related R&D was applauded by reviewers, who noted the value of these projects both domestically and internationally. The software and publication outputs of the risk assessment efforts were praised as being highly beneficial to stakeholders. Materials compatibility projects were praised for their relevance and for their efforts to enable stakeholders to utilize the data acquired during the course of the work. All of the projects received praise for collaboration and stakeholder engagement. Reviewer recommendations included adding clarification and focus to the future plans of the projects.

Hydrogen Quality: The expansion of scope from previous years was praised for efforts related to fuel quality. Reviewers also applauded the progress in developing a prototype hydrogen contaminant detector. They recommended that the standards-related portion of the work move forward at a more aggressive pace, but they felt that the pace of the R&D portion was commendable.

Safety Management and Resources: One safety management and resources project was reviewed, receiving an average score of 3.5. Reviewers applauded the expanded impact of the Hydrogen Safety Panel to include non-DOE work and noted the success of the panel in terms of the increased number of reviews from the previous year. They also praised the international collaboration for first responder training and the continued domestic outreach and training efforts. Reviewers raised concerns about having sufficient resources to update items developed elsewhere and hosted on H2Tools.org and whether the efforts to transfer external resources to the site might be duplicative.

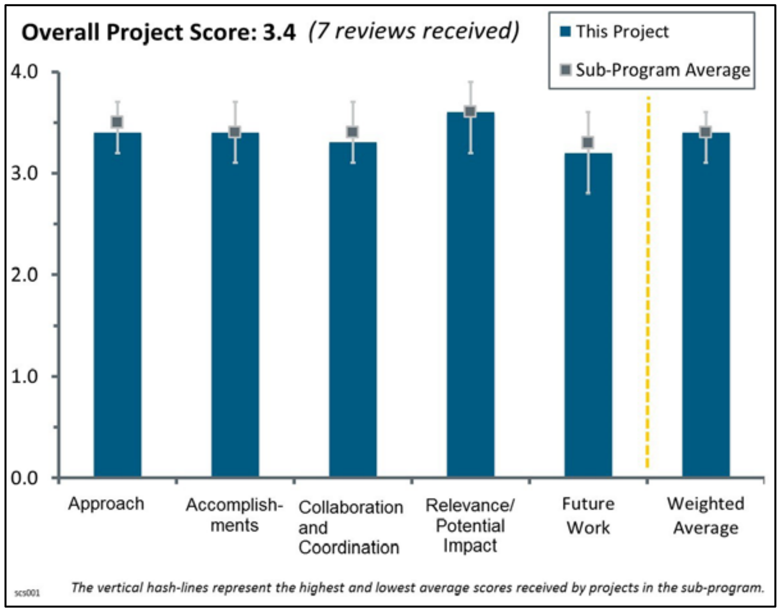
Outreach: Two outreach projects were reviewed and received an average score of 3.3. Reviewers praised the outreach activities for their engagement of a diverse set of relevant stakeholders. They commended both projects for their efforts to serve codes and standards activity coordination, which is a critical area of need. Reviewers encouraged even further development of outreach on a regional level and recommended that both projects seek clarification in areas in which there is perceived overlap.

Project #SCS-001: National Codes and Standards Deployment and Outreach

Carl Rivkin; National Renewable Energy Laboratory

Brief Summary of Project:

The objective of this project is to further the deployment of hydrogen fuel cell technologies with particular focus on the infrastructure required to support fuel cell electric vehicles. This outreach and training project supports technology deployment by providing codes and standards (C&S) information to project developers and code officials, making project permitting smoother and faster.



Question 1: Approach to performing the work

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

- This project consists of a multiannual effort aiming at continuous improvements and developments of C&S. It contributes to many overarching national activities and therefore integrates and aligns with the overarching goals. It is well designed and able to respond properly to new needs.
- The principal investigator does a good job at identifying the needs and doing the needed outreach. This project has really grown in collaborations and teaming compared to previous reviews. The Continuous Codes and Standards Improvement (CCSI) process is good, and having a national laboratory helps drive that positive process. Chairing the National Fire Protection Association (NFPA) 2 is appropriate as long as the national laboratory does not write code language. Code language is written by the technical committee, not by the chair, so there should not be an issue, but attention needs to be paid to make sure this stays "clean."
- The project is on track. The approach is sound, feasible, and integrated with other efforts.
- The approach is effective. It contributes to overcoming most barriers.
- This approach is working well; however, the team should keep improving the communication about the project, available resources, and potential activities. "Getting the word out" to all stakeholders is critical and should be done through multiple means. H2 Tools is excellent in the sense that it is a "one-stop shop" and fairly user-friendly, but even it is not well publicized. Additionally, social media outlets are useful tools. Many stakeholder groups, such as the International Association of Fire Chiefs and International Association of Fire Fighters, have Twitter handles and/or Facebook pages. This seems like something H_2USA should be working to promote because there are relevant topics for all audiences: those who are in the midst of commercialization (California), those who are preparing (the Northeast), and those who will begin preparing in the near future (the rest of the country).
- The outreach and training was a good approach, but it is not clear whether this is a role for a national laboratory. It may be better handled by an industry association. With CCSI, it is not clear if there is an overlap with the work with the Fuel Cell and Hydrogen Energy Association (FCHEA). If it is coordinating activities, FCHEA may be able to do additional work on continuous improvement. The National Renewable Energy Laboratory (NREL) connection to the FCHEA work was unclear, and the design is a bit amorphous. It is not clear whether it can be more focused year-to-year depending on needs. It is not clear whether anyone is looking across the U.S. Department of Energy (DOE) and national laboratories to push new science to technical C&S committees.
- It would be helpful to clearly state the needs and prioritize the specific barriers of concern with a little more detail at the beginning (i.e., examining what problems the project is trying to solve and why). While the

barriers can be understood from the context of the project, they might not be clear to an audience not closely involved in the topic. Also, a summary statement invites check-in and discussion from partners/collaborators to ensure that everyone remains on the same page regarding the work scope. That being said, the project does address the primary challenges in the topical area.

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 completion of the video, the publication of the *Guide to Permitting Hydrogen Motor Vehicle Fuel Dispensing Facilities*, the availability of the H2 Tools portal, and the Northeast training sessions are major, noteworthy accomplishments in the outreach portion of the project. The formation of the Hydrogen Storage Task Group and its work is also noteworthy in that area. It would be good to have a greater understanding of the steps needed to drive the change into the NFPA 2 revision.
- The reviewer would give this a 3.25 if possible. The project is better than good, but it could still use some improvement. Outreach to the current and future users is essential to address all levels of concerns and questions, and to receive input. The project should ensure that messaging is consistent across the board with regard to properties of hydrogen, the reasons for this technology, the rollout of fuel cells, etc. There are some regional distinctions, but New York, for example, is a zero-emission-vehicle (ZEV) state (as is California), so much of the same dialogue/messaging can be used. Plenty of notice should be given on the *Stationary Fuel Cell Guides* document when it is published.
- The output for this project is very good, considering a $300,000 budget. There were six publications, some of which are NREL reports, but others are in proceedings of refereed conferences and are appropriate publications for this work. The important issue here is that this work is in the public domain, and there are places where it can be found and referenced.
- The project delivers a consistent high quality. The two important guides published this year are the result of previous years' efforts.
- The project is effective and contributes to overcoming most barriers.
- The degree to which progress has been made and measured against performance indicators is satisfactory, and the progress toward the goals is appropriate.
- There were some good accomplishments and progress noted. However, it is not clear whether national laboratories should be developing permitting tools and guidelines or whether this a role for industry associations or standards development organizations (SDOs)/code development organizations (CDOs)? It is also not clear whether a national laboratory should be working on state/regional regulatory issues. It is a good idea to link to the most recent version of C&S. The project should consider how this can be aligned with the Hydrogen Fuel Cell Standards website[1].

Question 3: Collaboration and coordination with other institutions

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

- The project appears to involve all of the right areas of expertise and stakeholders in producing and delivering consistent material to inform C&S development and streamlined permitting of hydrogen facilities. As discussed in the question and answer (Q&A) session, the area of removing restrictions (such as the Maryland tunnel restriction) remains challenging because of the different levels of authority and different authorities having jurisdiction (AHJs) at the state/local level. A future opportunity for the project may be to try to develop a framework for identifying such restrictions and providing a consistent approach to get restrictions removed.
- The collaborations/teaming with this project in the current year are better than it has been in the past. Particularly notable is the Hydrogen Code Improvement (HCI) Team activities collaborating/teaming with the FCHEA Transportation Working Group (TWG) Joint Task Force on Regulations, Codes, and Standards (RCS). That team is led by Jennifer Hamilton from the California Fuel Cell Partnership, which has two

[1] www.fuelcellstandards.com or http://www.hydrogenandfuelcellsafety.info/

special task forces: one for strategic thinking and one for writing code. The HCI is providing significant collaborative participation in that effort.
- The collaboration and coordination seem to be good and well managed with a high degree of involvement from different stakeholders.
- The number of coordinated interfaces is impressive.
- Collaboration exists, and partners are fairly well coordinated.
- The project should get the U.S. Department of Transportation (and its associated agencies: the National Highway Traffic Safety Administration and Pipeline and Hazardous Materials Safety Administration) more involved. The collaborations should not just be side conversations but should provide regular input so the work is not being done in silos and the different departments understand what the others are doing. This is critical to national rollout and coordinated efforts.
- It is not clear whether there is a better metric to show engagement with SDOs/CDOs, such as drafts reviewed, C&S proposals, accepted proposals, and leadership positions on committees/working groups. It is not clear whether there is enough engagement with industry associations. Much of this work should be coordinated by them.

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.

- Concise codes and broad AHJ understanding of applying codes are critical to the success of hydrogen infrastructure, and the project addresses both of those needs very well. Needed changes to codes (such as separation distances) will continue to evolve through the various standard-setting bodies, so work will need to continue both to educate the standard-setting bodies as new information informs changes to C&S and to inform/update those applying the C&S of the changes that have been made and why they are good.
- The relevance of this effort is very good. Without harmonized intelligent C&S, the deployment of safe hydrogen technologies would not happen. This project goes a long way to make sure the RCS domestic community is ahead of RCS needs.
- The project aligns well with the Hydrogen and Fuel Cells Program and DOE research, development, and demonstration (RD&D) objectives. It also has the potential to advance progress toward DOE RD&D goals and objectives.
- The project is certainly relevant. It may be difficult to gauge the immediate impact, but the project will certainly see the effects as stations are built/deployed in the coming years.
- The project has the potential of providing a high impact, but to maximize it, it is essential to keep the commitment of the different stakeholders.
- Part of the impact is evident as stand-alone products. Other impact is integrated or merged in a wider effort.
- This work aligns with DOE objectives. However, there may be some overlap with other activities (SCS-019 and SCS-022). It is unclear how this is truly coordinated.

Question 5: Proposed future work

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

- The future work proposed is good and necessary. It continues in line with the previous efforts.
- The proposed future work grows from the existing effort, which is fine. It would be good to see increased effort in the collaboration/teaming area.
- On the topic of “codifying the mitigation measures to reduce setbacks,” codifying is already happening with stations being built (and opened), so it is crucial to get stakeholder input.
- There is some overlap with other projects. Some tasks did not fully describe how the project will drive results. There is no mention of the International Organization for Standardization. These documents will be published in future years. It is not clear how this project will support adoption/acceptance and improvement.

- With the exception of the engineering-based liquid hydrogen setback distances, the future work proposals have insufficient detail regarding who will do what and when. For a funding request to be made, a project plan with more detail about planned activities for the next year must exist. It would be helpful to see in particular what the measurable deliverables are and how they are prioritized.
- The project should continue to work on overcoming project barriers/challenges.

Project strengths:

- The project addresses a critical need in streamlining the safe deployment of fueling infrastructure for hydrogen vehicles and for other installations (e.g., stationary fuel cell applications). The emphasis on communication to stakeholders by developing and making available information that has been developed in the hydrogen industry is to be commended.
- There is a great deal of knowledge behind the work and many good and timely activities happening in parallel.
- The principal investigator is clearly very knowledgeable about code language, the code writing processes, and how AHJs work and think. The principal investigator is a very good asset.
- The project relies on highly qualified and broad competences.
- Continuous improvement of C&S is critical.
- The project is generally effective.

Project weaknesses:

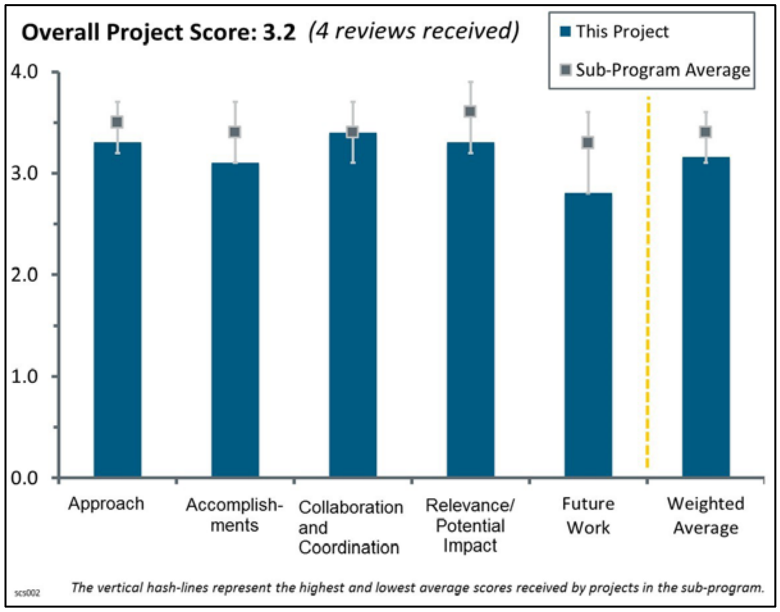
- While not a weakness with this project, per se, communication to the “outside world” is a global challenge. Most of the time we are all talking to each other, but it is important to consider how to get the word out to the people who need to know it is there and use it. There is some of that through workshops, for example, but that is still very focused/targeted. Another challenge is dealing with commercialization in real time; stations are opening on a weekly basis, and there is lag with research and development (R&D), making it difficult to keep up with the pace. It is not clear that this can be fully resolved, but it should be recognized, and there should be efforts to stay “ahead of the curve.” A good example of this is the reality of the code cycles, which lag behind the pace of industry but work closely with industry to get the most up-to-date information to help inform the code committees.
- While the teaming and collaboration is better, the project can still be improved. When presented, it looked like the HCI project was just at NREL, but when questioned, the principal investigator did articulate that it was part of the FCHEA TWG special RCS task force. It would have been good to have the principal investigator note that and embrace that activity during the presentation rather than clarify it during a Q&A.
- Because the area of interest is so large, there is a risk of tackling too many items with not enough resources for each one and of losing focus. It would be beneficial to see prioritization of the planned initiatives to justify/communicate a focus on the top five (or top three, six, or whatever is appropriate) with more specific deliverables for each one.
- From the presentation, it appeared that some of the achievements are based on a “one-man team.” If they are the product of the team at NREL, it should be made clearer next year.
- It is not clear how this project has improved C&S or is driving clear movement.

Recommendations for additions/deletions to project scope:

- It would be good to see a topic regarding development of a plan or template for how to tackle the issue of use restrictions that vary with location/authority (e.g., tunnel restrictions). This is a challenging topic, to be sure—the deliverable may not be a specific step-by-step list of actions to take, but rather a list of who to talk to and what questions to ask to establish the path of action for each individual situation.
- The project should maintain a balance between R&D (for future implementation) and the current activities and deployment.
- The project should consider reviewing overlap with other projects.

Project #SCS-002: Hydrogen Component Research and Development

Robert Burgess; National Renewable Energy Laboratory



Brief Summary of Project:

The objective of this project is to conduct research on pressure relief device (PRD) failures with the goal of gaining an improved basic understanding of high-pressure hydrogen operational safety and risk. Results are provided to manufacturers and system suppliers for enhanced design, operation, and quality control of PRDs for use on high-pressure hydrogen systems.

Question 1: Approach to performing the work

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

- The project provides learning that is potentially critical to many areas of hydrogen use and deployment—the work will inform design practices, component reliability research and development (R&D), materials assessment, risk analysis efforts, etc. It is good to see the literature search combined with the specific testing and test method development to help advance knowledge and raise awareness.
- The approach is good for the level of funding provided. The modeling of collaborations based on success with the sensor laboratory is an effective approach to managing reluctant participants from disclosing proprietary data. The main reservation with the approach is that the project analysis seems to be ending prematurely.
- It is unclear why this task was attempted. The approach, if it was to replicate a field failure, is appropriate.
- It is unclear how these results would be applied.

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 so far is beginning to scratch the surface of creating knowledge that may have far-reaching impacts on the safety of hydrogen systems. Understanding PRD failure mechanisms is important to designing safe systems.
- The team has developed good root cause analysis and forensic review skills and tools.
- The project has laid good groundwork by uncovering/summarizing some specific problems/concerns. Data development is still needed; at this point the testing is not on a statistically significant sample size. Nonetheless, it can, one hopes, inform some of the work on risk analysis.
- The field failure was duplicated.

Question 3: Collaboration and coordination with other institutions

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

- There is good work with the Compressed Gas Association (CGA) and gas suppliers. The industrial gas companies in their large installations have been able to apply engineering and administrative controls in

their installations/processes to mitigate risks associated with PRD valve reliability, but this may not work well with retail/public facilities—it is to be hoped that spreading awareness of this issue will elevate efforts to get systems to eliminate the hazard.
- CGA and the industrial gas suppliers in the hydrogen energy market are engaged.
- The collaborators appear to be suitable, but the lack of a valve manufacturer is notable.

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 likely will strongly influence codes and standards (C&S), in that it may provide sufficient rationale to determine whether PRDs enhance safety and should therefore be required or represent a failure mode and should therefore not be required.
- The project/results directly apply to key objectives/barriers in deploying hydrogen infrastructure and providing data to inform C&S development.
- It is difficult to see why a well-known failure mode was replicated.

Question 5: Proposed future work

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

- Future work, at least as expressed in the slides, is somewhat broad/diffuse. It would be good to see more specifics on the development of statistically significant accelerated testing protocols to highlight maintenance interval needs and possibly materials selection issues.
- Much of the proposed future work is unrelated to this specific effort. Rather than using the limited data developed so far to support efforts to remove requirements for PRDs on hydrogen pressure vessels, it would be beneficial to see an expansion of the effort to better understand failure mechanisms of PRDs, thermal PRDs, etc. As the project could not replicate a key failure and identified some interesting information on some failure mechanisms, the objectives of this project as described on slide 6 have not yet been fully met. Some interesting information has been obtained that can readily lead to further investigation, which may result in better manufacturing techniques or designs of PRDs with improved reliability. The project results obtained so far do not convincingly demonstrate that removing the requirement for PRDs will result in a higher safety factor than exploring failure mechanisms sufficiently to facilitate more reliable PRDs.
- The project is complete.

Project strengths:

- The project deals with a very real problem and is working with real hardware samples. Also, the development of the test equipment and procedure are project strengths.
- This project provides useful information on failure mechanisms for PRDs.
- The expertise and resources of the National Renewable Energy Laboratory (NREL) are project strengths.

Project weaknesses:

- Recommendations for future work/direction need to be clearer.
- This was a short-duration project with a low level of funding. The effort does not explore leads identified from the project so far. It has not explored the subject sufficiently to conclude that PRDs should not be required in regulation, yet supporting the effort to loosen this requirement is one of the future work items.
- The applicability of the project is a weakness.

Recommendations for additions/deletions to project scope:

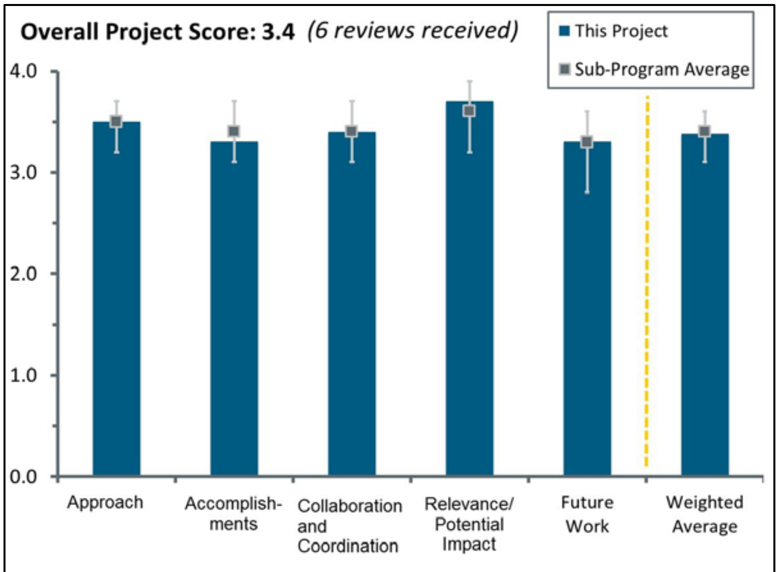
- While there is a need to start work on other components, this project has uncovered some interesting information that warrants further investigation. Recommendations for additions to the project include the following: (1) develop a plan for exploring more failure mechanisms, (2) expand the effort to various types of safety relief devices to make an informed recommendation on the feasibility of improving safety through use of such devices compared to a baseline of utilizing no such device, and (3) consider feeding lessons learned into safety models to help system developers make informed decisions regarding use of such devices.
- It is not clear whether additional samples (it appears that NREL has six valves in service) can be tested to develop at least preliminary statistics around valve failure modes.

Project #SCS-005: Research and Development for Safety, Codes and Standards: Material and Component Compatibility

Chris San Marchi; Sandia National Laboratories

Brief Summary of Project:

The main goal of this project is to enable technology deployment by providing science-based resources for standards and hydrogen component development and to participate directly in formulating standards. The project will (1) develop and maintain a materials property database and identify materials property data gaps, (2) develop more efficient and reliable materials test methods in standards, (3) develop design and safety qualification standards for components and materials testing standards, and (4) execute materials testing to address targeted data gaps in standards and critical technology development.



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 **3.5** for its approach.

- The approach of this project aligns with the tasks required to overcome the barriers associated with developing a hydrogen embrittlement standard. The project has an excellent strategy to ensure the effort is valued and useful through coordination with expert organizations globally. The pursuit of a refined database is also a good approach to assist users in accessing and dissecting the various data.
- The project has clear deliverables/status for each barrier and is focused on a manageable number of topics (three). The topics selected provide key support for stakeholders working to develop performance-based standards.
- This is a sharply focused project. It has great graphics to depict the issue and accompany the data.
- The approach is sound and value-added. It is focusing on issues for higher-pressure hydrogen systems. More integration with the American Society of Mechanical Engineers (ASME) is warranted.

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 excellent data that are clearly making it to codes and standards (C&S) committees and are usable by industry.
- Launching the database is a significant accomplishment, and it is commendable that the principal investigator resisted developing an in-house database program. The use of Granta not only should provide the maintainability and support benefits discussed in the session but also should make it familiar to outside stakeholders such as industrial users. The development of the low-temperature fatigue testing methods is also a significant contribution. It is not clear what will be required to move this methodology into the realm of performance-based standards and to ensure acceptance by stakeholders.
- The accomplishments to date are appropriate. Outreach through ASME, the American Society for Testing and Materials (ASTM), and SAE International is appropriate because they generate the general design codes and are where engineers have been trained to look for materials information. The CSA Group (CSA)

is a product safety standards organization. Outreach through CSA will have a limited effect as compared to ASME, ASTM, and SAE International.
- The materials property database is available for use. Low-temperature and fatigue crack propagation is ongoing.
- The technical progress of the project seems to be limited during the past year. The project established a database, which was useful, although additional testing data were not as prevalent in the project review. In addition, Sandia National Laboratories (SNL) has been developing the fundamentals for hydrogen embrittlement for a significant time. It would be helpful to highlight the previous progress and aspects that still need to be developed.

Question 3: Collaboration and coordination with other institutions

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

- This project has a high level of collaboration with standards organizations, industry, and international organizations. It is excellent that the work associated with this project is directly linked to several hydrogen materials compatibility standards, including ASME, SAE International, and CSA. The collaboration of this project sets a good example for other projects to follow within the DOE C&S portfolio.
- The project has solid collaboration with standards development organizations, industry, and international entities. Data from international entities are clearly integrated and attributed. The project has great collaboration and outreach.
- The coordination and collaboration seem valid. The collaboration for documenting test methods is questionable. Test methods on this topic would most likely be published by ASTM. It is not clear why ASTM is not in the loop.
- The project is working with standards organizations, industry, and international research groups.
- The presentation gives limited information regarding collaboration in the database tools and low-temperature testing elements. It is important to articulate this for the low-temperature testing so that it is clear that there will be future broad buy-in and use of the methodology when standards are implemented based on the data created. In the advanced storage element, the collaboration with industry is discussed in terms of materials being provided by the partners. It is not clear how they have responded to the initial results and whether there is active dialogue with them regarding the methodology.

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 is highly supportive of the DOE research, development, and deployment goals to provide critical data and information needed to define requirements in developing C&S. The hydrogen embrittlement understanding and test method development is highly relevant for the industry.
- Continued migration to performance-based standards is critical to improving cost and timing of hydrogen-related product development. This project makes an important contribution to that migration by developing a methodology that should support and encourage standards that can reduce unnecessary conservatism in design.
- The project is highly relevant. Accelerating the pace of the research would not be inappropriate.
- The project is aligned with DOE goals and sharply focused.
- Developing standard test methods for materials testing would help develop harmonized standards.

Question 5: Proposed future work

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

- The proposed future work makes sense. It would be good to publish the test methods at ASTM, generate a non-mandatory appendix for ASME (either Boiler and Pressure Vessel Code Section VIII Division 3 and/or

ASME B31.12) and create a technical information report through the SAE International Fuel Cell Standards Committee.
- Improving tools and database is a good path.
- The future work is appropriate for the project at a high level. Additional details and work plans would be useful to confirm the project has the needed focus and momentum to deliver results for the various standards organizations and collaboration efforts. It would be helpful to highlight the current gaps in the various hydrogen embrittlement standards and discuss the alignment to bridge the gaps.
- The project could benefit from a clearer statement of planned work. A summary of who does what when with specific deliverables would help make the project priorities clearer. It is likely that a project plan discussing resources and timing exists to support the future funding request. Perhaps that plan can serve as a starting point for a more concise plan for future work.
- Please coordinate results with Phase II of Global Technical Regulation (GTR) No. 13 on hydrogen and fuel cell vehicles. Phase II involves materials compatibility for hydrogen containers in vehicles.

Project strengths:

- The project provides significant support for the objective of simplifying materials selection and the designing/testing of hydrogen-related components, and highlights the need for specialized test capability and methodology (hydrogen at low temperatures and various pressures). These attributes coordinate well with SCS-026 regarding testing of polymeric materials and with the higher-level DOE objective of providing data to enable performance-based standards.
- The project strengths are its technical excellence and the value-added task in support of industry.
- The project is very focused and critical. Clear progress has been made.
- The researchers have the needed expertise, and their approach is good.
- The project serves a necessary role in developing and coordinating the technical data for hydrogen embrittlement test methods.

Project weaknesses:

- Most weaknesses from last year seem to be addressed. The need to continue to improve industry collaboration and conducting industry-led testing is the only “weakness.”
- A statement of specific areas in which future work can support standards development would be helpful and would support the need for more concise definition and prioritization of future deliverables.
- SNL has been involved with hydrogen embrittlement for a significant time, and the progress and remaining key actions should be explained because it is difficult to distinguish the relevance and new information from the project.
- There is room to improve on the end user aspect of the research.

Recommendations for additions/deletions to project scope:

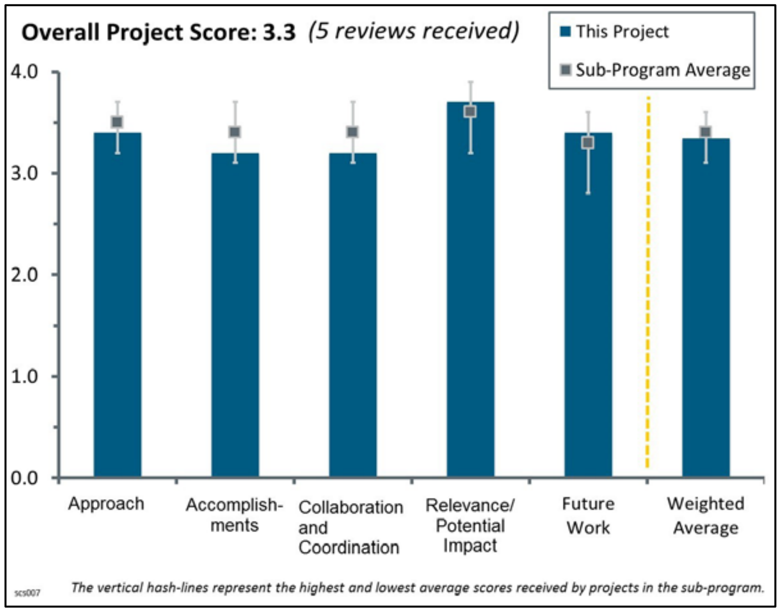
- More documentation through ASME, ASTM, and SAE International is recommended.
- The project should coordinate results with Phase II of GTR No. 13 on hydrogen and fuel cell vehicles. Phase II involves materials compatibility for hydrogen containers in vehicles.
- The recommendation for this project would be to ensure the database is accessible and helpful for users. The project should also include a roadmap of needed steps to complete the effort on hydrogen embrittlement. It would be beneficial to include in the scope a key next step for many initiatives being coordinated by this project.

Project #SCS-007: Hydrogen Fuel Quality

Tommy Rockward; Los Alamos National Laboratory

Brief Summary of Project:

The objectives of this project are to (1) focus on polymer electrolyte membrane fuel cell testing and collaborations and work with the American Society for Testing and Materials (ASTM) to develop standards and (2) develop an electrochemical analyzer to measure impurities in the fuel stream. The analyzer will be inexpensive, will be sensitive to the same impurities that would poison a fuel cell stack, and will support quick responses to contaminants.



Question 1: Approach to performing the work

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

- This is a broad project that covers three main objectives: ASTM work, an in-line analyzer, and the impact of a contaminant. The collaboration efforts with the VTT Technical Research Centre of Finland (VTT) and European Commission Joint Research Centre (JRC) on the impact of contaminants are valuable to this work. In addition, the approach taken to understand the effects and responses of CO and H_2S has been key in developing the analyzer prototype.
- The approach, which includes parametric studies of CO and H_2S tolerance coupled with single pass and hydrogen recirculation, seems very appropriate.
- The approach of evaluating the fuel quality under more realistic conditions, including recirculation, is a useful addition to the analysis. Other realistic conditions should be considered, such as the evaluation of the effects of complete stacks rather than cells. The international collaboration is a positive aspect of this project. The approach of using a fuel cell for the in-line gas analyzer appears to have significant risks because the noise factors within the fuel cell may cause difficulties with evaluating the impurity signal and the impurities may damage or alter the signal.
- Recirculation effects are significant. These were studied by the Japanese Automobile Research Institute (JARI) and presented to the International Standards Organization (ISO) Technical Committee (TC) 197 Working Group 12 in 2007. The approach could be improved to leverage existing data. JARI is listed as a collaborator in the overview slide, but the nature of that collaboration is not clear.
- The approach is very good. However, a more aggressive posture is needed with ASTM. The need for vetted test methods is moving from chronic to acute.

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.

- Accomplishments on this year's efforts have been outstanding. This has been demonstrated with the in-line analyzer results to include the instant response at 50 ppm CO plus the development of a prototype.
- Accomplishments are impressive.

- The project has made good progress in evaluating various impurities in a recirculation application and has attempted to validate the gas analyzer with a redesigned hydration scheme. The preliminary prototype results for the analyzer appear interesting, but further detail on the operation and validation is needed.
- Progress on hardware prototypes appears promising. Progress on ASTM standards cannot be evaluated from this presentation. Slide 6 states that eight standards are under consideration; however, the subject of these standards and their relevance to DOE goals is not clear. Progress on the development of standards since the 2015 DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) is not clear.
- The progress at ASTM is slow, but this is probably beyond the ability of Los Alamos National Laboratory to influence. The progress on the sensor is excellent.

Question 3: Collaboration and coordination with other institutions

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

- Collaborations seem to be appropriate and fruitful. With the recent work initiated under ISO/TC 197, this could be added to the list.
- The collaboration on this project is very good, especially with respect to ISO. It would be helpful to have additional information about the coordination with other organizations, such as SAE International. In addition, a status on the round robin testing would be useful.
- The collaboration on the ASTM work is appropriate. The work on the sensors is promising.
- The researcher has strong collaboration with international organizations that are leaders in the hydrogen fuel quality area. The team should explore further collaboration with scientists and engineers from the National Renewable Energy Laboratory who are working on fuel contaminant analyzers for hydrogen stations.
- The effort is focused on hydrogen quality; however, there is no mention of collaboration with ISO/TC 197, where international standards for hydrogen fuel quality have been developed and are undergoing revision. There is no mention of using the wealth of data already available on contamination testing and effects of recirculation. It is good that SAE International and ISO hydrogen-quality specification limits will be used in testing the in-line hydrogen analyzer. Further information on how the results may feed back into standards development would be useful.

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 two project objectives related to hydrogen fuel quality and the in-line quality analyzer are highly relevant to the DOE research, development, and deployment goals and important for the fuel cell industry.
- Understanding the real impact of contaminants and continuous fuel quality monitoring are some of the main challenges that will need to be addressed not only in the United States but also globally to enable a successful rollout of safe and reliable hydrogen stations.
- Both topics are highly relevant. The work with ASTM is a critical path.
- Resolving fuel quality issues is one of the most critical objectives of the DOE Hydrogen and Fuel Cells Program.
- Development of the in-line analyzer will have a significant impact on deployment of fuel cell electric vehicles (FCEVs) and hydrogen dispensers. Impacts of the work with ASTM are less clear. The impact of the work is difficult to evaluate from the information provided in the presentation.

Question 5: Proposed future work

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

- On the fuel quality part of the project, the team seems to be on the right path by continuing the collaboration with VTT, JRC, and CEA (CEA-Liten). For the in-line analyzer work, the focus has been on

CO and H_2S, and the path is clearly defined. It would be interesting to include in the future work some efforts on other potential contaminants, such as H_2O and NH_3, and have some preliminary analysis on the potential target cost for this device.

- The proposed future scope seems appropriate to meet the project objectives.
- The future work at a high level appears to be appropriate for the project scope. Additional details on the plans and timing would be useful to understand the expected timeframe for the deliverables.
- The proposed work is appropriate.
- Future work on ASTM standards needs better-defined milestones. The hydrogen fuel quality work appears reasonable; however, it would be useful to spell out in the presentation how this effort fits into existing efforts at SAE International and ISO beyond validating the in-line analyzer to the hydrogen quality specifications from those standards development organizations. Information, data, and idea exchanges should be described if they take place. A solution for the need for test sites and/or funding for testers needs to be determined.

Project strengths:

- Project strengths are the project's strong knowledge, experimental base, and international partnership.
- The strength of the project is the importance of the scope and focus that is underlying the effects of hydrogen quality and developing a gas analyzer.
- Development of an in-line analyzer for hydrogen fuel quality will facilitate deployment of FCEVs and hydrogen refueling dispensers.
- The expertise of the laboratory is a project strength.

Project weaknesses:

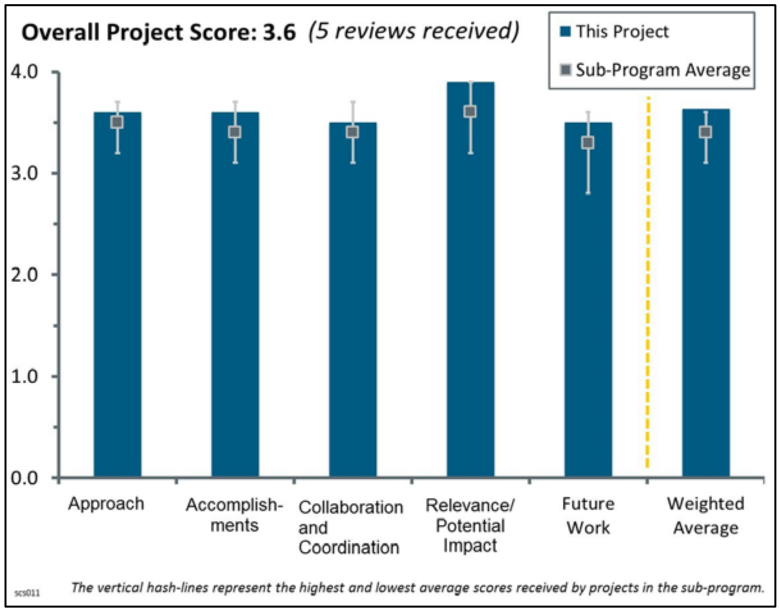
- Project weaknesses are the lack of time scale for publication of ASTM standards; lack of testing due to a shortage of suitable test sites or the need for funding testers; and lack of clear ties to available resources, such as previous studies. Decision points, milestones, risks, barriers, and challenges are not described. The final slide acknowledges a similar comment from the 2015 AMR; however, the response makes an erroneous assumption that reviewers have access to reports presented in other forums. Projects are reviewed based on materials provided for the reviewers in the presentation package. It is recommended that the principal investigator take advantage of the opportunity for reviewer-only slides to provide the details that were "presented to the Codes and Standards Tech Team."
- The weakness of the project is the under-appreciation of the noise factors that could change the gas analyzer signal.
- Dependence on test sites that may be lacking funding is a project weakness.
- The lack of urgency at ASTM is a project weakness.

Recommendations for additions/deletions to project scope:

- Go/no-go decisions should be defined for this project, particularly for the ASTM activities. The project scope should include cohesive reporting to an audience that is broader than the Codes and Standards Tech Team. The project should consider publishing project progress and next steps in appropriate peer-reviewed journals or industry news articles to raise awareness of the activities and help inform interested parties.
- An addition to project scope would be to highlight the feedback of the desires from the hydrogen production industry to adjust certain impurities and develop a work plan to evaluate whether the purity standard can be relaxed to these values. The scope should also provide a better correlation of the effects of impurity on a cell level to a full stack level. For the gas analyzer, a disciplined study on the various noise factors needs to be conducted and evaluated to ensure viability of the approach.

Project #SCS-011: Hydrogen Quantitative Risk Assessment

Katrina Groth; Sandia National Laboratories



Brief Summary of Project:

The primary objective of this project is to provide a science and engineering basis for assessing the safety of hydrogen systems and facilitate the use of that information for revising regulations, codes, and standards (RCS) and permitting stations. Sandia National Laboratories will develop and validate hydrogen behavior physics models to address targeted gaps in knowledge, build tools to enable industry-led codes and standards revision and safety analyses, and develop hydrogen-specific quantitative risk assessment tools and methods to support RCS decisions and to enable a performance-based design code compliance option.

Question 1: Approach to performing the work

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

- The coordinated approach is well thought out. Targeted efforts to develop tools and put them into the hands of those needing them are directly relevant to U.S. Department of Energy (DOE) goals and the barriers and needs uncovered in recent years. As the software gets used, there may well be suggestions for further improvements.
- The project is effective and contributes to overcoming most barriers.
- This is really great work.
- The approach appears to be appropriate.

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 demonstrated excellent progress on adding the flexibility and improvements that stakeholders identified previously. Publication of reports and user guides is an excellent way to help meet objectives and make it easier for industry to be aware of and use the tools developed. Good efforts are being made to get the input and feedback required from users. This input is necessary to advance the project and is unfortunately not completely within the control of the project team.
- The progression from a beta version to a usable version of the Hydrogen Risk Assessment Model (HyRAM) has been impressive and will produce a great tool.
- The project has made substantial progress on HyRAM, its rollout, and its acceptance.
- The progress and accomplishments are impressive. However, it would be helpful to benchmark the hydrogen work against compressed natural gas (CNG).
- The project is effective and contributes to overcoming most barriers.

Question 3: Collaboration and coordination with other institutions

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

- Agreements to work directly with key industry partners to validate efforts are excellent. There are very good international partnerships and a good approach that involves sharing ideas to build consensus in order to harmonize separation distance methodology.
- This team has coordinated with National Fire Protection Agency (NFPA) 2 Technical Committee (TC) and International Organization for Standardization (ISO) TC 917 and brought value to both committees.
- There is good collaboration; partners participate and are well coordinated.
- Although AVT Research, Inc., may be an exception, referencing one-man shops (Zero Carbon Energy Solutions, GWS Solutions) may not be as impressive as it sounds. Shell; Chevron; Air Products and Chemicals, Inc. (APCI); Praxair; etc., would carry more weight.

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 combination of timely research, direct participation in codes and standards development activities, and interaction with stakeholders to validate and build upon the modeling work is outstanding and likely to result in harmonized, acceptable methodologies for science-informed separation distances.
- The project is critical to the Hydrogen and Fuel Cells Program and has potential to significantly advance progress toward DOE research, development, and demonstration goals and objectives.
- This is very useful for NFPA work.
- The work is highly relevant.

Question 5: Proposed future work

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

- Adding suggested modules and developing a mechanism for user-supplied data as planned will make the tool even more useful.
- The work is effective and contributes to overcoming most barriers.
- Focusing on pushing a prototype program into the fire codes may not be well accepted. A benchmarked tool would be easier to get accepted. Continuing work on liquid hydrogen and the practical lower flammable limits and lower explosive limits would be helpful.
- The project needs more emphasis on adding liquid hydrogen capability. It would be helpful if the project could address releases in a container that did not trap hydrogen under a roof, thereby limiting the concentration.

Project strengths:

- HyRAM provides a visual representation of the underlying assumptions and physical phenomena, which is great.
- This project represents a science-based approach to address barriers directly relevant to DOE goals with significant interfaces for user input to ensure the tool meets stakeholder needs.
- The project will have excellent impact on hydrogen safety, codes and standards.
- Strengths include the skill set and value-added approach.
- There has been good progress on HyRAM release and rollout.

Project weaknesses:

- There may be additional recommendations from users as they begin to work with HyRAM, which may result in the need for development of additional modules, further data, etc. It would be good if this project were to continue with sufficient time and funding to accommodate such feedback.
- The proposed work to evaluate cold/liquid releases does not appear adequate to meet the needs of the project if the intent is to address large-scale releases.
- The project lacks benchmarking against other fuels.

Recommendations for additions/deletions to project scope:

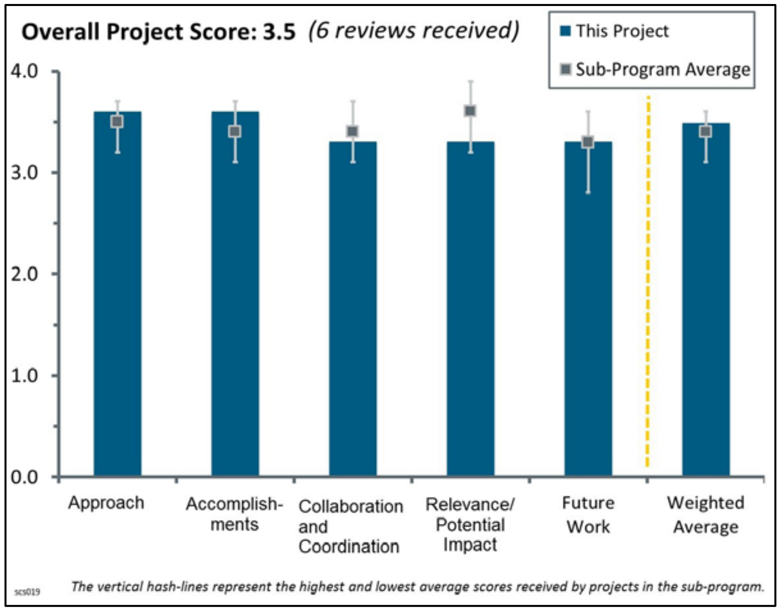
- The project should continue to promote the availability of the tool. The team might consider integrating it into future permitting workshops held by authorities having jurisdiction or other forums in which stakeholders can get hands-on experience using the tool and provide feedback. Experience and input is critical to improving the tools. While this model is focused on hydrogen fueling stations, it would be great to develop a tool (whether additional modules or a new modeling tool) that could help answer questions posed by regulators, such as what would happen if a hydrogen vehicle tank ruptured inside a parking garage, tunnel, etc.
- The project should continue to work on uncertainty analysis and sensitivity studies.
- The project needs to address roofless enclosures and evaluate the potential differences and/or advantages of such a design.
- The project should add benchmarking against other fuels.

Project #SCS-019: Hydrogen Safety Panel, Safety Knowledge Tools, and First Responder Training Resources

Nick Barilo; Pacific Northwest National Laboratory

Brief Summary of Project:

This project provides expertise and recommendations though the Hydrogen Safety Panel (HSP) to identify safety-related technical data gaps, best practices, and lessons learned, as well as helps integrate safety planning into funded projects. Data from hydrogen incidents and near misses are captured and added to the growing knowledge base of hydrogen experience to share with the hydrogen community, with the goal of preventing safety events from occurring in the future. The project also aims to implement a national hydrogen emergency response training resource program with adaptable, downloadable materials for first responders and training organizations.



Question 1: Approach to performing the work

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

- There are really three distinct tasks in this project. Each is important. The HSP, in particular, is an excellent way to help ensure projects include a quality safety review.
- This project (actually three projects) continues to perform very well, particularly given the limited budget. The approach and focus clearly enable project success.
- Focus on objectives is outstanding.
- The approach is generally effective but could be improved. The project contributes to overcoming some barriers.
- A panel, a website, and training/education, while critical, are obviously subject to scope creep without clear definition. It is not that these are not important but that clear definition/scope (and what is out of scope) around each activity was not fully clear.

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.

- Given the focus of these activities, the measurable output is excellent and well-focused on those who need it. The outreach from the HSP is exactly what was suggested in previous reviews, and the portal is clearly a used resource—indeed, people are finding it on their own and find it useful. The responder training remains a hallmark of this work. It is particularly good to see this effort collaborate as closely as it does with the California Fuel Cell Partnership. The book is really nice.
- Accomplishments are outstanding. The addition of new members to HSP helped to increase high-quality output. Recent work with the California Energy Commission indicates appreciation of HSP work and value for stakeholders other than those of the federal government. The release of a product (field) certification guide is a tremendous help to both industry and regulators.

- There has been demonstrated accomplishment in the HSP, safety webinars, equipment certification guide, engagement with states and the public, safety knowledge dissemination, the H-prize, and the H2Tools website. There has been a significant ramp-up in activity since 2015.
- The increase in number of projects reviewed by the HSP is indicative of this effort's success. Developing classroom training that is adaptable and accessible to emergency responders is progressing well, with great feedback from those who are taking part. The presentations on safety knowledge tools appear to be effective in generating interest. There are some concerns regarding the H2Tools portal part of the task. The first concern is in regard to the response of a 2015 reviewer comment pertaining to data maintenance. Although it makes sense to have the owners keep this information updated, this may not happen for a number of possible reasons. Such updating requires resources. There is no clear commitment or plans to provide such resources. There is a risk that the data will not be maintained or that such maintenance may end up needing to be performed by project participants. The second concern has to do with the home page of the H2Tools website, which looks like the Fuel Cell & Hydrogen Energy Association (FCHEA) Hydrogen and Fuel Cell Safety Report, with articles pertaining to codes, standards and regulations—several of which have recently been featured in the FCHEA publication. There are also several articles featured that are unrelated to safety. The home page has the look and feel of a trade association or advocacy group rather than a site to aggregate hydrogen tools focused on safety.
- All areas show good progress and accomplishments. Providing "approval" guidance does not solve the problem of lack of certified equipment. It is not clear how HSP supports codes and standards (C&S) directly or whether they can review/provide input on drafts. The project needs more metrics on training—retention of knowledge, etc.
- The project is effective and contributes to overcoming most barriers.

Question 3: Collaboration and coordination with other institutions

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

- Domestic collaboration and outreach is truly outstanding. Indeed, this activity has recently opened collaboration with the HyResponse European Union program. The project has worked with HyResponse previously and recently visited France with firefighters to learn from the HyResponse project and for HyResponse to learn from U.S. firefighters to get feedback on the project. This is outstanding. This project should give serious consideration to what the United States can adopt from the HyResponse project in its domestic training.
- The project engages with many of the right partners and seeks to leverage resources with similar international efforts. Collaboration with H_2USA so far appears to be limited to a previous International Code Council (ICC) workshop. As H_2USA is also developing tools and information resources and conducting outreach with authorities having jurisdiction (AHJs) through presentations and web-based materials, closer collaboration is suggested, as well as cross-referencing to minimize duplication.
- Having HSP involved is outstanding. The collaboration with H2tools needs improvement (it seems to be mainly driven by national laboratories, leaving out industry associations and industry). Planned collaboration with first responders is good; it sounds like measures are in place to engage firefighters' associations.
- There is good collaboration; partners participate and are well coordinated.
- Collaborations seem to be appropriate and growing.
- More collaboration/outreach with states and local officials/public is needed to get quicker approvals for hydrogen stations.

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.

- These projects are spot on, and impact is high and fairly obvious. If the state of California is asked to name the rate-limiting step on deploying fueling stations, the answer is not permitting, codes, and standards.

Because of the outreach of this project and that of others, the AHJs are very receptive to hydrogen technologies. This comfort level is a direct result of the fine work this project has done over the years.

- The success of HSP and H2Tools is critical for the DOE Hydrogen and Fuel Cells Program (the Program) achieving its technology deployment objectives.
- The HSP, safety knowledge tools, and first responder training activities should be evaluated separately. The HSP is critical to ensuring early activities have expert safety review. The training activity has the potential to have significant impact if the training can be integrated into the curriculum of one or more organizations responsible for first responder training. The Safety Knowledge: Tools and Dissemination effort seems to be less relevant, as there are already organizations doing much of this work. The original concept was to pull together existing information, such as risk mitigation tools and hydrogen incidents, into formats to make them easier to access on the ground—for example, through applications (apps) on popular mobile phone platforms. The current direction appears more like a stand-alone website that uses information developed by others but which must be updated separately. At the moment, that appears to be duplicating effort. Maintenance of such information will require further duplication.
- Most project aspects align with the Program and DOE research, development, and demonstration objectives.
- There does not seem to be much specific tie-on to DOE relevance and impact.

Question 5: Proposed future work

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

- HSP's proposed future work seems to be to continue doing what is being done, which is appropriate. Expansion to non-DOE projects, and in particular state-level projects, is also appropriate. Identification of a suitable entity or entities to take on the first responder training activity is critical to successfully getting this training into the hands of those who need it most. However, the H2Tools plans appear to have deviated from the reviewer's understanding of this aspect of the project. Adding more than 600 papers from the International Conference on Hydrogen Safety to the Hydrogen Tools Portal may not be the best use of resources. The kinds of tools that have a large impact on safety are tools for calculating or evaluating risks; data to prevent incidents such as those found in the Hydrogen Incident Database, which includes functionality to upload incidents; and the Hydrogen Risk Assessment Model (HyRAM). Loading hundreds of existing papers may not be as valuable an activity. If documents are needed in lieu of tools, perhaps the project could link to the resources rather than import them, as maintenance of imported information remains an issue. Regarding the plan to "Lead the development of the Safety, Codes and Standards outreach plan to establish a multiyear strategy for reaching code officials, relevant stakeholders and first responders through 2020," it is highly recommended that the project do this through or in partnership with H_2USA, as this group is already working in this space, having identified the target regions and markets, as well as with FCHEA, which has launched a significant targeted outreach plan. The plan to develop a C&S guide—a drill-down, question-based tool to provide an outline or checklist of code requirements for a specific application—could have value, particularly if this were to be a mobile phone app.
- The future plans are a natural growth of this project. The project did not receive a score of 4, only because there should be more effort to get the infrastructure development activities in the Northeast to make use of this project, specifically HSP. This will take more effort on behalf of HSP because the Northeast infrastructure developers are using private funds rather than using government funds as California is doing.
- There is a good plan for future activity. It is not clear why the certification guide was not included in future work for fiscal year 2017. This will need constant refinement as C&S and Nationally Recognized Testing Laboratory capabilities evolve. The HSP role in C&S development should be increased.
- The project is continuing ongoing activities and transferring some of the mature activities to a third party, which is good.
- Propose future work seems to be appropriate for meeting the project objectives.
- Proposed future work is generally effective but could be improved.

Project strengths:

- The project has an excellent principal investigator and excellent program/project execution. The project has achieved great results on a limited budget. The project is simply excellent.
- HSP is top-notch and critical. It is good to see this work expanded beyond DOE-funded projects.
- The project has a very strong knowledge and expertise base, which has significant potential for providing services to a broad spectrum of stakeholders.
- The project is well planned, and the project managers and participants have the needed expertise.
- It is important to have interface tools for the public. It is important to continue with HSP but push HSP into a broader role.
- Accomplishments relative to project barriers and challenges are good.

Project weaknesses:

- There are no significant weaknesses. The first responder training element is more of a transition element rather than a weakness.
- There is potential for loss of focus because each project has a different goal and different needs. Perhaps these can be better integrated and leveraged.
- The H2Tools part of the project no longer seems headed where it was when launched. Much of the effort duplicates existing efforts and requires extra maintenance.
- Funds are insufficient to really execute what could be done.

Recommendations for additions/deletions to project scope:

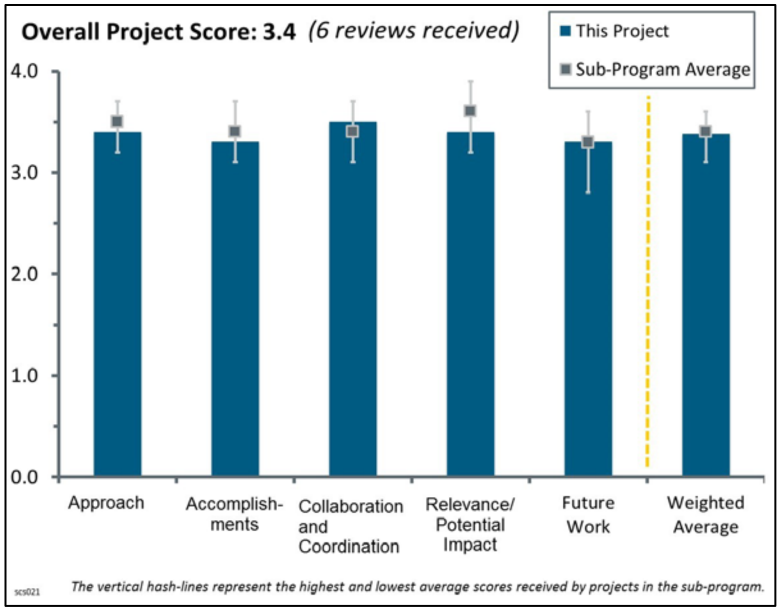
- The project should continue the outstanding effort in supporting/contributing to C&S development such as the published guides for enclosures and product field certification. This role could be extended in reviewing/vetting model code draft standards such as NFPA 2/55, NFPA 30A, etc. (potentially in partnership with the Sandia National Laboratories C&S team).
- H2Tools should be focused on the development of tools that project developers can use in the field rather than on a database of articles that exist elsewhere. The project should consider adding HyRAM to the H2Tools website, as this tool is now available for download by the public. The project should also consider announcing new features, workshops, tools, etc., through press releases shared with organizations that share such news to those most likely to utilize the tools. Examples include the National Association of State Fire Marshals, FCHEA, NFPA, ICC, and announcements at the National Hydrogen and Fuel Cells Codes and Standards Coordinating Committee monthly webinars.
- It is time for more attention to be given to the Northeast, with outreach and possibly more direct interaction with the HSP. To provide more resources for the outreach, training, and portal work, maybe a different funding model for the HSP should be considered. Possibly a user-funded model would help—for example, the financial burden of a safety review could be put on the project that is being reviewed, dividing up the financial responsibility among the projects that benefit from that review.
- More collaboration/engagement/outreach is needed with states and local officials/public to get quicker approval for hydrogen stations. Probably it would help to partner with environmental groups to advocate to the public on the safety of hydrogen fueling stations and other applications of hydrogen and fuel cell electric vehicles.
- The project should continue to address project barriers and challenges.

Project #SCS-021: National Renewable Energy Laboratory Hydrogen Sensor Testing Laboratory

Bill 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. The National Renewable Energy Laboratory (NREL) Sensor Laboratory tests and verifies sensor performance for manufacturers, developers, end users, and standards-developing organizations. The project also helps develop guidelines and protocols for the application of hydrogen safety sensors.



Question 1: Approach to performing the work

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

- The approach taken by this principal investigator (PI) and laboratory is generally excellent. The laboratory is well-thought-out; the collaborations are global, involving excellent laboratories and researchers. This provides an excellent opportunity for blind testing between the facilities to ensure the science and measurements are accurate. The interaction with the “community” involving the research community, equipment manufacturers, suppliers, users, and standards development organizations (SDOs) helps to ensure the project leads the state of the art and stays relevant to the needs of advancing the deployment of hydrogen technologies. The approach is excellent.
- There are many issues associated with development of hydrogen sensor technology, and the capability to evaluate the claims that manufacturers attribute to their products is key. Important aspects include measuring sensor performance at the sensing element level, as a sensing device and in analyzers; quantifying issues of sensor deployment in the application environment; translating overall findings into support of codes and standards (C&S) development; supporting safety and process applications; coordinating laboratory standards with other laboratories; and maintaining confidentiality of proprietary designs. The NREL Sensor Laboratory has worked to achieve all of these functions, which are critical for development and deployment of sensor systems.
- The approach of validating the accuracy of various sensor or sensor systems in a blind study is useful in and of itself to prove or disprove the myth that hydrogen sensors do not work. This is also probably the most extensive testing following the various sensor consumer product safety standard test methods. Feedback on the test methods to upgrade the SDO documents would be value added.
- The approach used is good because it is based on collaborating with organizations and helping them find appropriate sensors for a specific purpose.
- The project approach appears to fit in well with the overall NREL hydrogen safety structure and has provided model support for and interaction with industry in advancing the application of hydrogen sensors (the Service Bay project with Toyota/KPA). This project team has also acknowledged that there is much work to do in the area of appropriate application of sensors. There is evidence of a thorough study of and solid methods for empirical testing of sensors and the classification of sensors. It feels as if the project team is saying that one of the critical needs is documentation of guidance for application, but it is not clear that the work plan focuses on that priority. For example, the measurement of the venting profile does not clearly relate to the barrier reports or to the directions discussed in the relevance section.

- The approach is generally effective but could be improved. The project contributes to overcoming some barriers.

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 sensor laboratory addresses a number of important needs of the hydrogen community that are DOE goals. Among these are providing a consistent and reliable methodology to evaluate sensors; coordinating sensor evaluation methodology with international partners; providing consistent and reliable evaluation of industry sensors, including the recent ones; evaluating the colorimetric indicator tape; and acting as a source of information on safety issues regarding sensor use, including recent CRC Press publications.
- The progress and accomplishments to date are excellent.
- Developing sensor function test facilities and methods is very important to the next step of developing better application information. It seems like many sensor problems are really application or misapplication problems, so having this foundation to start to address those issues is good. The tailpipe sensor selection/assessment work is also very valuable. Perhaps that work can now be coordinated with the component research and development to make sure the durability/reliability of a specific sensor in a specific application is well understood.
- It was a little difficult to score this review category, because all examples of accomplishments and progress, except one, were excellent. The sub-project "Support of Infrastructure Empirical Profiling of LH2 Releases during Routine Venting" could have been presented under Approach. However, what is being executed is an array of vertical pointwise measurements taken about 10 seconds apart. These are not simultaneous data points but separated in time because they are multiplexed into the data sampling system. There are 10 ports, 10 seconds apart, which means that each port is sampled every 100 seconds. Presumably this vertical probe will be moved spatially to try to get some spatial and temporal information on a highly transient, turbulent, three-dimensional (3D) spatially dynamic event. The purpose of this effort is to gain information on the cold plume behavior during a cryogenic fill operation from a cryogenic tank several feet high and presumably on the behavior of the hydrogen during the release. The physics of this event is a function of 3D and of time. The pointwise-in-time and space measurement of anything will yield useless information. If there are non-detects, it says nothing about the jet—for example, the plume could have been at the sensor location earlier than the sample and moved when the sample was taken. The reverse is also true. What is needed, ideally, is a 3D time-resolved movie of the release (hydrogen, water, ice, oxygen, nitrogen, etc.); the next-best option is a line-of-sight integrated volumetric time-resolved movie (such as schlieren or shadow graph). Point measurements in this situation are a waste of time and money because nothing of meaning can be expected from this effort. These pointwise measurements will be woefully inadequate to compare to any computational fluid dynamic calculation. Indeed, a FLACS software calculation would provide a much more trustworthy insight into the plume behavior than data from this proposed experiment. Except for the noted sub-project, this project continues to have an outstanding outreach/publication record. The project contributes to appropriate technical symposia, journals, reports, books, etc., which is excellent.
- The project has demonstrated application of different types of hydrogen sensors for different industry needs. The project could improve by providing a web-based data sheet on the types of sensors to use for different industry applications, as well as the method of verifying sensor performance to verify adequate operation with aging.
- The project is generally effective but could be improved. The project contributes to overcoming some barriers.

Question 3: Collaboration and coordination with other institutions

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

- The technical breadth of collaborators, including industry, original equipment manufacturers, users, and developers, is excellent. This PI has truly developed a very good scope of collaborators, which makes this project relevant to needs, relevant to current state of the art, etc.
- The sensor laboratory works with a wide array of organizations, from manufacturers to C&S organizations to other government agencies—and with international entities charged with performing similar functions.
- The presenter did a good job of coordinating and collaborating with industry and research groups.
- The work with KPA and Element One to support the development of specific applications is good and also builds toward the reviewer's recommended long-term goal of improving application documentation and guidance.
- Collaboration exists, and partners are fairly well coordinated.
- It would have been nice to see sensor manufacturers listed: Det-Tronics, MSA Safety, Kidde Fenwal, 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 **3.4** for its relevance/potential impact.

- The NREL Sensor Laboratory functions to help validate sensor performance, a key element in the development of a national hydrogen infrastructure. It is important to remember that hydrogen is invisible, has no smell or taste, and burns under many conditions with an invisible but extremely hot flame. Hydrogen systems, technicians, and the public must rely on sensors to regulate processes and inform of potentially hazardous conditions. It is critical that sensors be evaluated carefully and to consensus standards.
- The use of sensors in hydrogen applications is in the code structure; therefore, it is very relevant. From a use safety aspect, having confidence that one is using the correct sensor for the application at hand is critical. This project addresses that point. The project is missing one major aspect, and that is rigorous determination of sensor placement and a determination of characteristic time scales for a "leak" to find the sensor to trip an alarm. It is one thing to have a fast-acting sensor system (sensing and acting), but the convection time for the hazard to find the sensor (hydrogen in this case) needs to be considered. The PI does recognize that placement is an important issue—but it is done "informal[ly] and often by intuition." It is strongly recommended that this work be expanded to address this issue on a more technically rigorous basis.
- The work is highly relevant with the potential to have a high impact on increasing the reliability of such sensors. It is to be hoped that increases in the reliability also reduce the costs.
- The project aligns well with the 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.
- Focus on robustness of sensors and documentation of applications are key to this project.

Question 5: Proposed future work

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

- The future work plan is excellent. The project has identified critical areas to advance: validation, fuel quality, wide-area monitoring/distributed sensors, autocalibration, and placement. The rest of the future work is also excellent.
- The work is outstanding and was well outlined. Sensor placement, sensor response with aging, and faster-response sensors are key areas of future work.

- The sensor laboratory will prove to be an integral component in future national hydrogen programs. The laboratory should be adequately maintained to provide support to manufacturer development of sensors, end-user support and deployment, C&S development and maintenance, and the use of sensors for safety.
- The proposed future work is excellent.
- It would be good to see future work focus on the self-calibration project that was discussed and also understanding of sensor degradation due to contamination and aging. The benefit of the vent stack plume measurements, at least those related to hydrogen sensor testing per se, is difficult to see.
- The proposed future work is generally effective but could be improved. The work contributes to overcoming some barriers.

Project strengths:

- The NREL Sensor Laboratory is relevant to the national hydrogen community in that it has successfully provided and continues to provide sensor evaluation; assistance to manufacturers, developers, end users, and SDOs; collaborations with other laboratories; and continued investigation into relevant issues that involve sensor performance (safety, fuel quality, etc.).
- Collaboration with external organizations is a strength. Determining types of sensors to use for different industry/research applications is a strong objective.
- Strengths include the team’s competence and the focused end goal to be value added to industry. This is not a science project.
- With the sub-project “Support of Infrastructure Empirical Profiling of LH2 Releases during Routine Venting” as an exception, this PI continues to produce very high-quality work, publishes well, and has earned international respect for the outstanding quality of work.
- There has been good progress and effort in addressing stated project barriers and challenges.
- There is good development of test facilities and methods.

Project weaknesses:

- The only weakness in this larger body of work is the sub-project on venting plume measurements. The proposed measurements will provide neither the insight nor the data needed for model validation. The experimental approach, data sought, and techniques to acquire the data need to be re-thought.
- The project may be a little unfocused, with some sub-projects outside the investigation of sensor technology. (However, some elements that involve real-world measuring are necessary to validate the approach.)
- The apparent lack of collaboration with major sensor manufacturers is a weakness.
- While there must be issues in providing the support needed from the NREL Sensor Laboratory, this reviewer is not aware of the specifics.

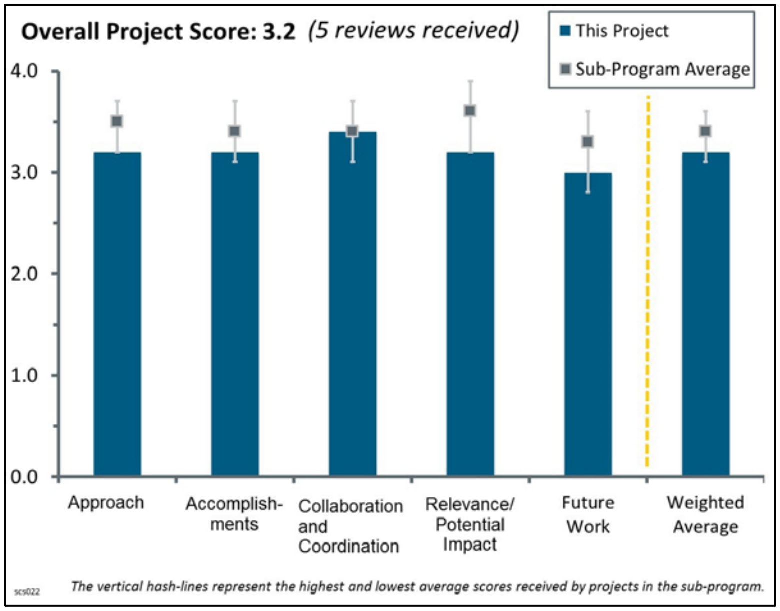
Recommendations for additions/deletions to project scope:

- The NREL Sensor Laboratory appears flexible in providing support to issues as they arise. Nothing specific is recommended, except that the laboratory continue.
- The project should continue to address project barriers and challenges.
- The hydrogen industry will really benefit from a better understanding of how to apply the various types of sensors. Also, sensor self-calibration and self-diagnostics have large potential cross-cutting benefits.
- Scope should include a web-based data sheet for sensor specifications for different applications. The project should also evaluate the life of hydrogen sensors and the effect of aging on response.
- The proposed measurements for the cold plume venting study will provide neither the insight nor the data needed for model validation. The experimental approach, data sought, and techniques to acquire the data need to be re-thought. The team really needs to embrace people who make this type of measurement. Combustion Research Facility (CRF) staff are well trained to make and study this type of behavior. It is strongly recommended that someone from CRF be made an integral part of this team and lead in the experimental design.
- The project should bring major sensor manufacturers on board.

Project #SCS-022: Fuel Cell & Hydrogen Energy Association Codes and Standards Support

Karen Quackenbush; Fuel Cell & Hydrogen Energy Association

Brief Summary of Project:



This project supports and facilitates development and promulgation of essential codes and standards (C&S) to enable widespread deployment and market entry of hydrogen and fuel cell technologies. The goals of the project are to ensure that best safety practices underlie research, technology development, and market deployment activities supported through projects funded by the U.S. Department of Energy (DOE); conduct research and development to provide critical data and information needed to define requirements in developing C&S; and develop and enable widespread sharing of safety-related information resources and lessons learned with first responders, authorities having jurisdiction, and other key stakeholders.

Question 1: Approach to performing the work

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

- The project has an outstanding approach. The established working group (WG) structure and flowchart of activities coupled with dissemination activities are hard to top.
- The WG approach is good, and it is to be hoped that the Fuel Cell & Hydrogen Energy Association (FCHEA) will provide plenty of input on *National Fire Protection Association (NFPA) 2: Hydrogen Technologies Code* this code cycle. FCHEA support for standards development programs at CSA Group, NFPA, and the International Organization of Standardization (ISO) is very important.
- The approach is generally effective but could be improved. The project contributes to overcoming some barriers.
- FCHEA coordinates a variety of national and international standards activities and for this effort receives what must be a contribution (roughly $200,000 per year) from DOE. The scope of what is performed seems very broad and extensive. Direct support of standards WGs is important. Reporting on perceived needs for C&S is valuable. Sharing of safety-related information, while important, is now done by many other organizations. It is not clear how DOE uses the matrix report. There are many collaborations. It is unclear how effective all these efforts are and whether they are all needed by DOE. Up until now, it seems the FCHEA support has been useful.
- The approach of this project is mainly a monthly call and bimonthly report to consolidate information from other organizations and activities that are performing the C&S development. The tracking of these C&S activities are useful, but it is uncertain whether this approach has resulted in any progress for the industry. The tracking matrix could be a good approach, but the method of gathering industry input is uncertain, and value to the industry is not clear.

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.

- FCHEA has been very helpful in the development of ISO standards and product certification standards at CSA. It is to be hoped that the FCHEA will develop and submit comments to NFPA 2.
- The progress of all WGs is impressive. The level of effort in coordinating monthly National Hydrogen and Fuel Cells Codes and Standards Coordinating Committee (NHFCCSCC) calls and publishing bimonthly Hydrogen Fuel Cell (HFC) Safety Reports is outstanding.
- The project is generally effective but could be improved, and it contributes to overcoming some barriers.
- The accomplishments of this project are uncertain since it is mainly tracking the efforts of other organizations. The WG effort of this project is highly confounded with the C&S activities occurring in the actual organizations.
- The presentation got lost attempting to explain accomplishments, perhaps because the project is too many initiatives. The priority given to each effort was not clear. The presumption is that the services described were accomplished.

Question 3: Collaboration and coordination with other institutions

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

- FCHEA has been very good at coordination of C&S developments.
- The project has high collaboration with C&S organizations along with industry through the FCHEA membership.
- The established collaborations seem to be appropriate and well-coordinated. It is best to focus on established collaborations rather than spreading efforts too thin.
- FCHEA does collaborate and coordinate with many organizations.
- Collaboration exists; partners are fairly well-coordinated.

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.

- FCHEA's C&S support efforts are essential to development of comprehensive C&S. FCHEA's support for overcoming regional regulatory challenges is also relevant.
- Most project aspects align with the Hydrogen and Fuel Cells Program (the Program) and DOE research, development, and demonstration (RD&D) objectives.
- FCHEA activities have significant relevance to the Program's objectives.
- It is hard to evaluate the impact that the FHCEA activities achieve. The presentation attempted to detail myriad accomplishments, but it is not clear how they specifically benefit DOE. In general, they benefit DOE. Based on internet metrics, the FHCEA is still sought as a source of information. The FHCEA obviously drives three WGs, reports on standards activities, and provides reporting. In the past, the FHCEA ran the National Hydrogen Association (NHA) conferences, now discontinued. One reviewer lamented the absence of the NHA conferences, exhorting that they should be brought back. The presenter was overwhelmed in the attempt to describe all that the project performs and did not complete presentation of a number of slides.
- The relevance of this project is the intent to align with DOE RD&D goals and objectives, although the effort seems to be more of a monitoring and tracking activity rather than a contributing role. As indicated, the project supports other activities with participation, but the direct benefit of this project toward those activities is uncertain.

Question 5: Proposed future work

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

- The proposed future scope seems to be appropriate. Doing more of the same and doing it well is a good thing in this case.
- The proposed future work is generally effective but could be improved. The work contributes to overcoming some barriers.
- C&S activities are not yet matured, and the need to support development of these efforts continues. It would be useful if the FHCEA could condense or summarize the nature of this support to DOE. As a level-of-effort activity, DOE would do well to continue to fund this effort and perhaps make clear what is expected from FHCEA.
- The future work appears to be more of the same monitoring activities without specific tasks.

Project strengths:

- The strength of the project is the coordination of various C&S activities and attempting to provide a summary of their status.
- FHCEA is involved in many efforts that support hydrogen C&S development and has been since FHCEA's inception. It would be hard to find an organization that could do as well.
- Strong knowledge and expertise base of very dedicated people are strengths of the project. Strong legacy and traditions of NHA are other strengths.
- Coordination is a strength.
- There has been satisfactory effort on addressing project barriers and challenges.

Project weaknesses:

- There are no significant weaknesses; however, the danger is always to spread project personnel too thin.
- Perhaps more focus is needed as to which FHCEA activities are vital and should be prioritized, at least as is reported to DOE.
- Beyond tracking and monitoring, the value of this project has not been communicated in a transparent manner. The effort seems to claim progress associated with C&S activities with which the project partners have been only indirectly involved.

Recommendations for additions/deletions to project scope:

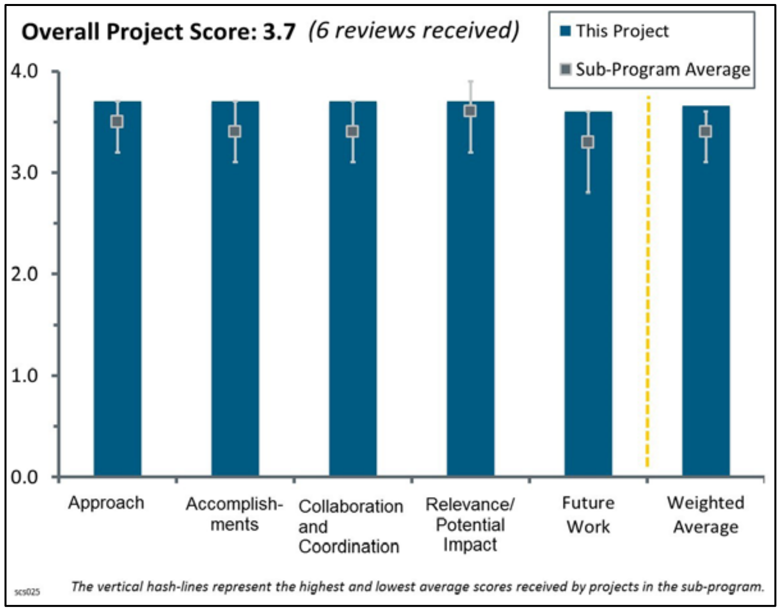
- The project should consider a revival (in some form) of NHA conferences. Those used to be marquee events. Revived events could be industry- or commercialization-focused under the banner of fulfilling the Paris Agreement. These events should be held in Washington, DC, exclusively (as opposed to, for example, a fuel cell seminar that may travel from place to place). It is critical to be in front of the politicians for the next three to five years.
- The recommendation for the project scope is to highlight the value of the project with specific examples of contribution and attempt to use the collaboration strength of the project to assist the industry in prioritizing technical tasks for the C&S development portfolio rather than simply monitoring.
- DOE management should consider what FHCEA products are vital. Alternatively, given the level of DOE investment, the product seems quite acceptable.
- Addressing project barriers and challenges should be continued.

Project #SCS-025: Enabling Hydrogen Infrastructure through Science-Based Codes and Standards

Chris LaFleur; Sandia National Laboratories

Brief Summary of Project:

The goal of this project is to enable the growth of hydrogen infrastructure through science-and-engineering-based codes and standards (C&S). Specific objectives include (1) streamlining cost and time for station permitting by demonstrating alternative approaches to code compliance and (2) revising and updating C&S that address critical limitations to station implementation.



Question 1: Approach to performing the work

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

- This work is extremely relevant to the implementation of hydrogen infrastructure. It has shown great value already with related projects that fed into the primary C&S for hydrogen and are being implemented. Much more can be done and potentially faster with increased funding from the U.S. Department of Energy (i.e., 100%).
- The approach taken by the project lead is very sound. Using science-based methods to improve codes and standards will be a key enabler to successfully implementing a hydrogen infrastructure. A great example of this is the work being supported on liquid hydrogen setback distances, which currently represent a significant barrier to the implementation of liquid hydrogen stations within existing gasoline retail sites.
- The approach followed by this project is excellent and is demonstrating value added.
- The approach is well conceived to address the critical barriers.
- The approach is sound, it is feasible, and it is integrated with other efforts.

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.

- Doing a real-world, actual, in-the-retail-fueling-environment application of alternate means will be of great value to various interested parties: station developers and authorities having jurisdiction, along with other city entities involved in the permitting process, and even the state of California (from the funders to the governor's office). The application is very valuable. A colleague says, "The gaseous separation distances are the most defensible and well defined anywhere in any code" (a project that this program worked on and is continuing to influence, along with other issues like liquid hydrogen [LH_2] separation and working directly with the task force). The Hydrogen Risk Assessment Model (HyRAM) is another tool that should prove extremely valuable in the station project planning process, although it needs more "advertisement" and certainly more support for the early users.
- The project has demonstrated significant accomplishments, including the report on the quantitative risk assessment analysis for the Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) reference station, the science-based approach to update the gaseous hydrogen setback distances, and the initial results on the risk analysis of the liquid hydrogen storage systems, among others.

- Release of HyRAM and its use for updating and informing domestic and international C&S is an outstanding achievement by itself. The rest is icing on the cake.
- The progress and accomplishments are helping DOE meet its goals. The National Fire Protection Association (NFPA) has included a chapter in *NFPA 2: Hydrogen Technologies Code* to allow this type of modeling in lieu of the prescriptive separation distances.
- The degree to which progress has been made and measured against performance indicators is satisfactory, and the progress toward the goals is appropriate.

Question 3: Collaboration and coordination with other institutions

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

- The project demonstrates excellent collaboration efforts that include industry leaders on liquid hydrogen station technology, significant engagements with the international community via the interaction with the International Organization for Standardization (ISO) as well as interfacing with world-class hydrogen safety researchers at the national laboratories.
- The collaboration and coordination seem to be appropriate and well managed with a good degree of involvement of different stakeholders.
- Developed collaborations are essential to this project's success. It is hard to add more to the list.
- The collaboration for outreach is very important and spans all projects, not just this one, but it is key to getting out the message that the knowledge, expertise, and resources are out there for those who need them.
- The collaboration appears suitable. It would be nice to include some state academic facilities to support the individual state fire marshals and to assist local industry on the use and interpretation of the results of this 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 **3.7** for its relevance/potential impact.

- The lack of general public exposure to and acceptance of results is a lack of information of risk acceptance that shows up as separation distances. In the real world, we either use the tool or the published excessive separation distances, which are often a guess because of lack of data. If this project helps reduce separation distances and reluctant acceptance, then this project will have been worth every penny expended.
- This project is extremely relevant to overcome one of the most critical barriers—footprint requirements—for the implementation of hydrogen refueling stations within existing retail sites.
- This project aligns not only with DOE's research and development (R&D) but also with some deployment, which still needs assistance. In general, DOE needs to maintain its focus on R&D for future advancements, but DOE also needs to continue to support demonstration and implementation as the commercial market is emerging and maturing. The market is not quite ready to expand on its own.
- The project has the potential to provide a high impact and is very relevant to the Hydrogen and Fuel Cells Program goals and objectives.
- Science-based contributions to regulations, codes, and standards development are critical to hydrogen infrastructure and hydrogen fuel cell technologies market deployment.

Question 5: Proposed future work

This project was rated **3.6** for its proposed future work.

- The proposed future work on the characterization of liquid hydrogen release via a science-based approach is probably the most critical activity in order to significantly improve the main issues with liquid hydrogen setback distances.
- The proposed future scope seems appropriate within the available budget and timing (i.e., personnel) constraints.

- The future work proposed is good and needed, and it continues in line with previous efforts.
- The proposed work is appropriate. The current model does not address the general public's acceptance of risk. Modeling similar applications, such as methane, and comparing the results with the current separation distances for methane would be useful in determining whether the computed separation distances are too conservative (or whether the methane rules are not adequately conservative).
- Perhaps the project should consider working with an authority having jurisdiction (AHJ) in another state on the Alternate Means/real-world component of the project. Another consideration would be to work on the metering issue(s) with the National Council on Weights and Measures, which again serves the effort to aid deployment across the country. The project needs to keep the pace with the setback work; it is unfortunate that a consensus agreement on a suitable means of quantifying hydrogen system mitigation features was not reached in time for this code cycle. There is much concern from industry, which should translate into support for the next fiscal year.

Project strengths:

- The project has superb talent and expertise on the C&S team. There are very strong domestic and international collaborations. HyRAM is a jewel.
- The project has extremely knowledgeable personnel, and the tasks are very relevant.
- The skill of the researchers and the perceived need for the product are strengths.

Project weaknesses:

- Restrictions (imposed by DOE policies) disallowing more in-depth participation in C&S committees, which are limited to providing scientific input, are a weakness. This restriction sometimes leads to misuse of the scientific input by those committees (e.g., NFPA 2/55 separation distance tables).
- The lack of financial support from headquarters puts strain on the projects and causes delay of very important results.
- Outreach and benchmarking are both weaknesses.

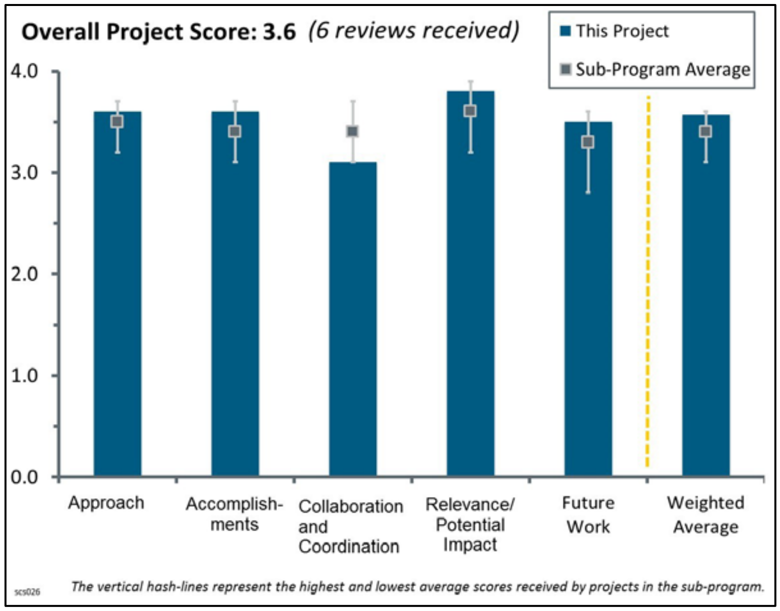
Recommendations for additions/deletions to project scope:

- Instead of just being science-based, it is important that C&S are evidence-based, which would include science, best practices, and lessons learned. A far-fetched suggestion is to partner with the Hydrogen Safety Panel to engage a review of codes such as NFPA 2/55, future 30A, etc. to ensure that the scientific input jives well with the best engineering practices and lessons learned. This partnering should help in avoiding situations such as NFPA 2/55 separation distance tables that in certain parts lack common sense and consistency.
- The project should work directly with AHJs in the upcoming states of deployment. The project should also work on metering, perhaps with the National Renewable Energy Laboratory.
- The project should model methane.

Project #SCS-026: Compatibility of Polymeric Materials Used in the Hydrogen Infrastructure

Kriston Brooks; Pacific Northwest National Laboratory

Brief Summary of Project:



The project objective is to fill a critical knowledge gap in polymer performance in hydrogen environments. Investigators will gather and assess stakeholder input about challenges and materials and conditions of interest for hydrogen compatibility, develop standard test protocols for evaluating polymer compatibility with high-pressure hydrogen, characterize polymers, and develop and implement an approach to disseminating information.

Question 1: Approach to performing the work

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

- The approach is perfect and hard to top.
- The approach is well-thought-out and realistic.
- This project is just starting. The approach taken to get this project going is very good; first, survey the stakeholders to understand problems and needs; next, define the operating domain; then expand laboratory capability to cover the domain; define the materials to be investigated; and develop the test method so repeatable, trusted data can be obtained. However, there was no reference to SAE International J2601 that will provide the temperature (T) and pressure (P) as functions of time for a J2601-compliant fill and hence define the P, T domain of interest.
- Three barriers are well addressed in terms of methodology. The approach, consisting of a consultation with stakeholders, is sound and helped in setting priorities. However, the presentation does not allow for assessing how much previous results have been taken into account (it is not the first time that a U.S. Department of Energy project is dedicated to hydrogen in polymers).
- There is too much focus on wear/abrasion, to the exclusion of other, more difficult issues to assess. The temperature bands should be widened to provide a safety factor to address the impact of excursions to a normal temperature range. For example, the lower range could be expanded to -60°C to see how close the materials are operating to the edge of problems.

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.

- This project is just getting started, with only seven months' progress at the time the presentation was produced; however, the team is moving quickly. The laboratories are up and running, and preliminary tests are already being performed.
- The start of the project is awesome. The information to date is value added.
- The project is still in its infancy (14% accomplished), and it is too early to evaluate whether results are able to overcome barriers. However, the progress so far is encouraging, and the first semi-quantitative results are convincing.
- It is too early in the project to fully assess this item. The project is off to a good start.

- The project could be rated as “outstanding.” Insufficiently explained selection criteria for polymer testing is the reason for a lower rating.

Question 3: Collaboration and coordination with other institutions

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

- The collaborations seem to be perfect and include very good coordination between the national laboratories and enthusiastic participation of an automotive original equipment manufacturer.
- The partners in this project all bring a lot to the table; they are experts with unique capabilities that together form a very powerful team to perform the needed investigations. The project has a really nice broad spectrum of “stakeholders” that will be used throughout the project and not only during the problem-definition stage. Neither a station provider nor a dispenser manufacture was included in the stakeholder list; the project team should include these additions.
- The collaborations to date are appropriate. Thought is needed on how to supply this information to the stakeholders.
- The project consists of a collaboration of three DOE laboratories. The activities are well distributed among the laboratories, and there is no reason to doubt smooth collaboration between them. One of the laboratories will provide the structural characterization to the other two to link macroscopic performance behavior with degradation phenomena and micro-level. A direct interface with industry is also present in the form of a sub-contractor. An international interface is not mentioned.
- It would be helpful to include more representation from fuel providers as well as feedback on areas experiencing particular problems.

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, if successful, will book considerable technological progress by providing validated materials performance data and underlying degradation phenomenological models.
- Understanding hydrogen effects and the extreme pressure and temperature environments in this domain is critical to the safe, successful deployment of hydrogen technologies.
- Polymeric materials compatibility is one of the critical elements in successful market deployment of hydrogen technologies.
- These materials are very important to the hydrogen industry, and there is little information available for them.
- On a scale of 1 to 10, this project is a 12.

Question 5: Proposed future work

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

- This work is just getting started, but the approach and direction this project is taking are exactly right. The project team needs to run tests under a J2601 protocol including removing the nozzle. J2601 will define the temperature–pressure time history, and removing the nozzle creates a sudden drop in pressure. As shown in some of the preliminary results, some of these materials take up large amounts of hydrogen when exposed to a hydrogen soak. A sudden drop in pressure will result in that hydrogen coming out of the material. Understanding that phenomenon is very important.
- The project is well on track, and the plan for the two future years is convincing and feasible.
- The proposed future work is well thought out.
- The proposed future scope appears to be appropriate for project objectives.

- The project should consider narrowing the scope to provide narrower but deeper understanding of materials for specific applications. In particular, hose materials are an issue and are exposed to extreme temperature and pressure cycles simultaneously. It would be helpful to see these conditions tested.

Project strengths:

- This is a very powerful team with excellent laboratory capability and thorough understanding of where the project needs to go. There is carefully constructed stakeholder outreach to learn what is needed.
- The project partners and collaborators have a very strong knowledge, expertise, and experimental base.
- The project strengths include interaction with stakeholders, the broad range of characterization, and the testing techniques.
- The project strengths include the topic, the collaborations, and the expertise of the laboratories.
- This project will provide useful information on these materials.

Project weaknesses:

- There are no significant weaknesses; however, the process for polymeric materials selection for testing could be improved.
- There are too many possible materials and too many potential applications, which creates too many combinations and permutations to adequately evaluate and address any of them fully. It would be better to narrow the focus. Numerous materials are subjected to simultaneous exposures, and the testing has to incorporate those exposures at the same time, as well as transient effects.
- It is not clear how the project has taken into account the results obtained in previous DOE activities. This project is not the first to investigate degradation of high-density polyethylene for liners.
- Outreach is a weakness.

Recommendations for additions/deletions to project scope:

- Engineers will go to one of three sources for this information: the American Society of Mechanical Engineers (ASME), SAE International, and the Parker O-Ring Handbook. Publishing the data through all three venues would be of value.
- The project should consider introducing purposeful flaws in the material to assess the impact of typical/potential manufacturing defects. This is similar to fracture mechanics for these polymeric materials. The temperature band of testing should be widened to better understand the impact of temperature excursions.
- As found in various risk assessments, many of the hydrogen technology chain components will experience operative conditions in their lifetimes beyond their design values (e.g., filling causing liners to exceed temperature limits). It would be extremely important to answer questions related to the behavior of plastic materials under these conditions and to quantify the degradation in terms of reduced lifetime. The project should also look into these aspects.

2016 — Market Transformation
Summary of Annual Merit Review of the Market Transformation Program

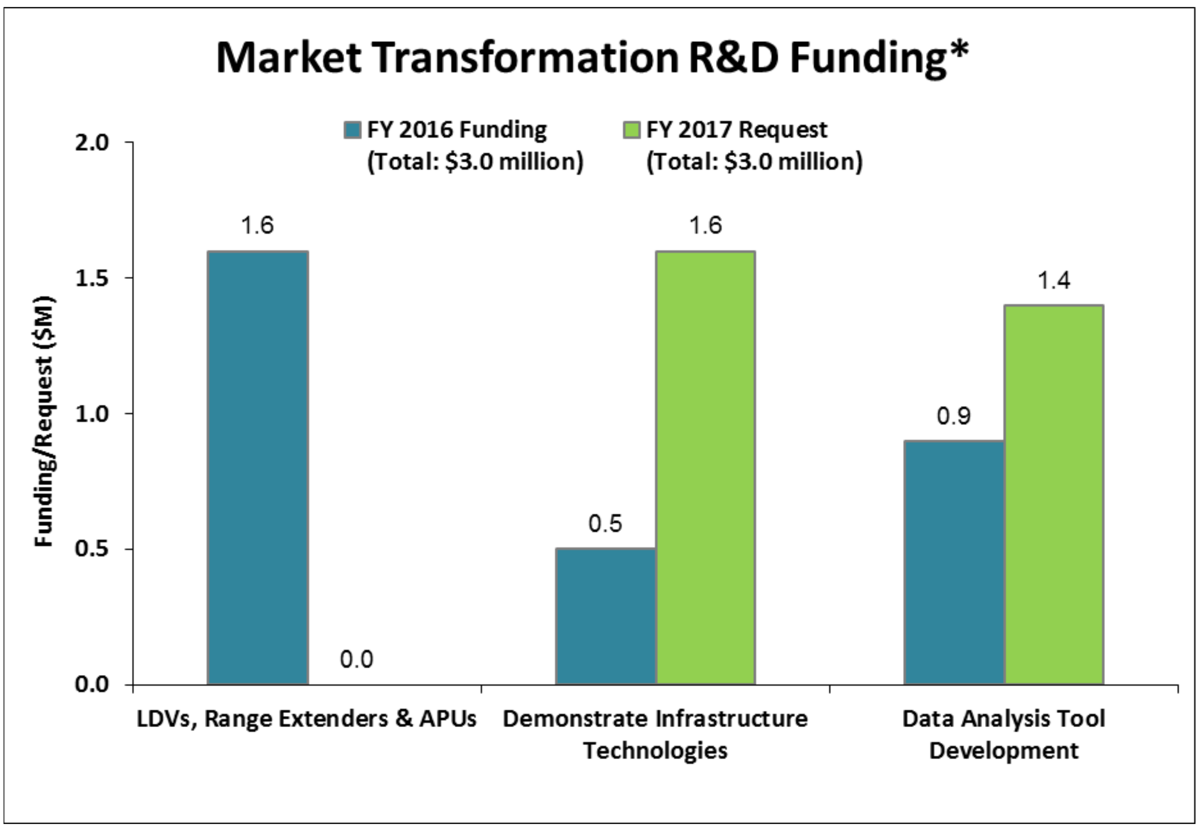
Summary of Reviewer Comments on the Market Transformation Program:

The purpose of the Market Transformation program is to spur market introduction by demonstrating pre-commercial technologies in real-world applications. By doing so, this program helps to identify and overcome market barriers and to reduce the life-cycle costs of fuel cell power through technical and non-technical solutions. Six projects were reviewed this year, and these projects are highly leveraged, with more than half of the funds provided by the U.S. Department of Energy's (DOE's) partners. This substantial commitment of external resources shows the high level of interest in exploring applications and markets in which the hydrogen and fuel cell industry can expand, and the technologies can play a valuable role.

Reviewers generally shared positive comments about the program's projects, with five of the six projects scoring at 3.1 or above. The reviewers noted that, as the projects advance, there is great potential to expand hydrogen fuel cell applications beyond the current established base in material-handling equipment and backup power. General recommendations were received to increase focus on addressing and overcoming specific barriers in each project and obtaining firm commitments for industrial partnerships.

Market Transformation Funding:

A new application begun in fiscal year (FY) 2016 was the battery/fuel cell light-duty hybrid service vans (LDVs), which will demonstrate a value proposition for utilities and other fleets used in operations and maintenance. The Market Transformation program budget for FY 2016 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 and the relative merit and applicability of projects competitively selected through planned funding opportunity announcements.

Majority of Reviewer Comments and Recommendations:

The Market Transformation program's projects were rated average to high, as overall ratings ranged from 2.7 to 3.4, with an average score of 3.1. The projects were judged to be relevant to DOE activities and to employ good or adequate technical approaches. Reviewers emphasized the need for data collection to develop business case reports that can be used to support further market expansion.

Airport Ground Support Vehicles: This project received an overall score of 3.4. Reviewers stated that this project has high potential to meet program goals and enable demonstration for a wide breadth of additional applications. Although reviewers were satisfied in general with progress made in terms of evaluation, design, and development of learnings, concerns about fuel cell stack performance and the timeline for completing the project were expressed. Reviewers also stated that the specific stack problems should also have been explained more thoroughly.

Hydrogen Energy Systems as a Grid Management Tool: This project received an overall score of 3.1. Reviewers stated that this project ties together multiple benefits (e.g., electrolyzer demonstration, renewable hydrogen for fuel cell deployments, enabling intermittent renewables) into a single package, and helps increase awareness and clarity of the permitting process for deployments. Reviewers stated that the proposed future work is similar to the future work that was proposed for 2015 and they are not clear on the reason for all the delays. For example, the MTA shuttle bus conversion was previously scheduled for September 2015 but is now listed as future work for 2016. Reviewers also commented that more attention to project execution barriers is needed.

Maritime Fuel Cell Generator Project: This project received an overall score of 3.3 for its efforts in developing, designing, and testing a first-of-its-kind hydrogen fuel cell power generator for maritime applications. Reviewers noted that this project's objectives were relevant: specifically, lowering emissions and technology/finance risk in a market that needs more efficient power technology. Reviewers commented that the project addresses DOE's goal to enable and accelerate expansion of hydrogen and fuel cell system use and that lessons learned from this deployment can be used for similar applications at other ports. They felt that development of a business case and identification of follow-on opportunities are imperative. Additional deployment phases with the current and concrete plans on how to expand the number of deployments are needed, according to reviewers.

Fuel Cell Hybrid Electric Delivery Van Project: This project received an overall score of 3.4. Reviewers stated that this application has great potential and that the project fits well within DOE's goals and objectives. Bringing one system online, evaluating its performance, and then deploying nineteen at various sites is a reasonable approach, according to reviewers. Some noted that, although there has been a setback with collaborators, evaluating duty cycles and designing appropriate system specifications was time well spent. One reviewer noted that more explanation on refuel planning is needed.

Fuel Cell Auxiliary Power Unit Project: This project received an overall score of 2.7. Reviewers agreed that the project is relevant and is a logical extension of other fuel cell applications, such as forklifts. Reviewers mentioned that very low operational time is hampering progress and specific go/no-go decision points were not expressed clearly. Also, reviewers stated that the timeline for the demonstration with the recently added partners is not yet clearly developed. Reviewers noted that progress has been slow and the degree of commitment on the part of the industrial partners is questionable.

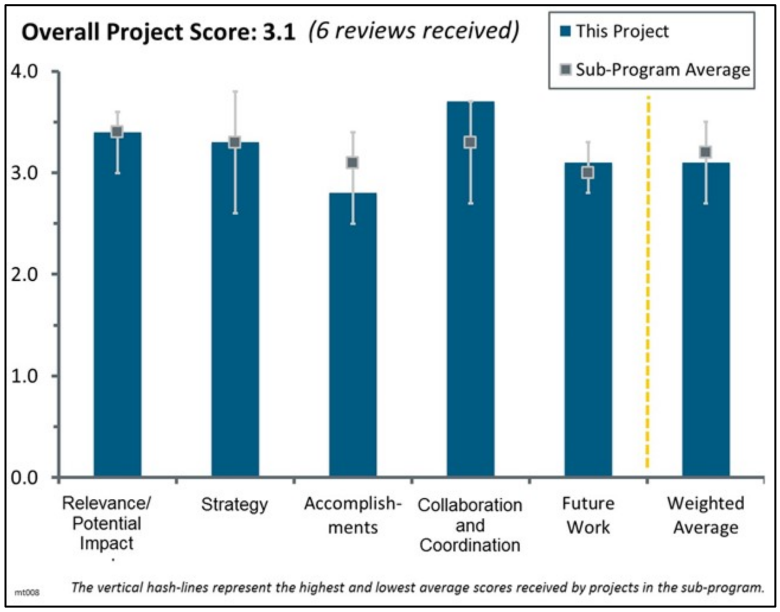
Fuel Cell-Battery Electric Hybrid for Utility of Bucket Trucks Project: This project received an overall score of 3.1. Reviewers noted that this application is an opportunity for near-term deployment of fuel cell technology, and this project is making progress toward evaluating the market. Reviewers commented that the potential impact of this project will be very limited without a better financial analysis. Insufficient information was provided to definitively understand the energy efficiency and air pollution reductions that could be achieved. Reviewers said that there is an absence of go/no-go decision criteria and that there is not enough detail on the battery storage system.

Project #MT-008: Hydrogen Energy Systems as a Grid Management Tool

Mitch Ewan; Hawaii Natural Energy Institute

Brief Summary of Project:

The objectives of this project are to (1) support development of a regulatory structure for permitting and installation of hydrogen systems in Hawaii and (2) validate the performance, durability, and cost benefits of grid-integrated hydrogen systems. The validation entails three tasks: (1) dynamic operation of electrolyzers to mitigate the impacts of intermittent renewable energy, (2) demonstration of the potential for multiple revenue streams from ancillary services and hydrogen production, and (3) introduction of hydrogen fuel for shuttle buses operated by the County of Hawaii Mass Transit Agency and Hawaii Volcanoes National Park.



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 relevant to Fuel Cell Technologies Office goals for deploying hydrogen technologies and supporting deployment of renewable energy sources. By providing renewable hydrogen to support fuel cell deployments while simultaneously providing a sink to accept excess power from intermittent renewables, electrolyzer deployment could have a significant impact on market acceptance of several technologies of interest.
- This is an outstanding project, as its design is to maximize efficiency and effectiveness of grid energy production systems. The project and results are critical to fine-tuning one of the necessary applications of fuel cell systems: the ability to maximize other existing energy generators.
- This project meets several goals and advances research in multiple areas, including renewable energy, electrolyzers, using hydrogen for energy storage, permitting, contracting, public outreach, and state and federal collaborations.
- There was a good explanation of the project thesis and why it is relevant to the electric power grid. Identification of the relevance to U.S. Department of Energy (DOE) barriers was also good.
- The location and project concept are aligned with Hydrogen and Fuel Cells Program objectives.

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 project effectively addresses multiple barriers, including the lack of private sector resources for deployments. The project is an effective illustration of the potential for renewable hydrogen to help with energy storage requirements and with grid frequency management.
- The presenter explained how the approach fits into the existing power sources and the interaction with the grid. There is clear justification for central site production and distributed distribution. The project

incorporates existing models developed for battery systems that can be modified to investigate electrolyzer applications for grid regulation. There is strong utilization of existing infrastructure to optimize the funding.

- This project is overcoming several barriers in the state of Hawaii and will be a good example for repeating the concept at other locations.
- Necessity seemed to reinforce the need to remain sharply focused on all barriers—not just the barriers listed but other unique challenges that could have scuttled the project.
- Deployment of actual equipment at the site has been significantly delayed, and it is not obvious how the project has addressed barriers. DOE funding was completed at the end of fiscal year 2015, and there does not appear to be a demonstration period of performance at the site or metrics that will determine the success of the demonstration.

Question 3: 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.

- The project demonstrated progress in preparing components and understanding load profile and system limits. Baseline ramp rates were measured and were below battery exchange storage system ramp rates. The project determined that the electrolyzer system must be modified to provide proper response rates for the grid. The project demonstrated improved response rates, but these were still not adequate. The project is working with the electrolyzer manufacturer to resolve issues. The project demonstrated hydrogen production and pasteurization by the electrolyzer system. Fuel cell shuttle buses were evaluated. Hydrogen storage issues were identified, and solutions were presented. The project addressed permitting with Hawaii officials and senior fire department leadership, paving the way to these officials' accepting installation. The project demonstrated that a hydrogen energy system (HES) could be used to fuel a fuel cell battery hybrid bus system.
- The project focused on critical barriers to improve effectiveness of load management and, importantly, identified the shortcomings of the subject of the study. No weaknesses were noted.
- This project has been slowed owing to several barriers including siting issues, changes in public perceptions, funding, and contracting hurdles. The team has been addressing the barriers, but it has caused delays in the project.
- An electrolyzer system was commissioned and operated at Powertech, producing some useful results so far, including a better understanding of transient response that suggests that hybridization with a battery storage system may be required. Progress was also made toward installing a test site at the Natural Energy Laboratory Hawaii Authority. However, the lack of clear milestones and timelines for the work makes it hard to gauge progress achieved versus what was planned.
- New accomplishments since 2015 were not clear. It seems that there is a lack of progress. These delays have been explained, and it seems that instead of addressing the barriers, the project has only confirmed those barriers. It is not clear how this project is changing or removing those barriers. One interesting result is that difficulty in communicating with a commercial electrolyzer system limits the ability to quickly respond. The project should follow up with the National Renewable Energy Laboratory (NREL) electrolyzer team to more thoroughly address this issue, as it is not seen when there is detailed ability to control the electrolyzer system. Perhaps the project could develop best practices for electrolyzer controls for grid services. The potential of electrolyzers to provide grid support is already known. It is more valuable to understand specific aspects of an island environment and the specific challenges for grid support in the island environment, including costs, benefits, and the most valuable opportunities (selling hydrogen or providing grid services).

Question 4: Collaboration and coordination with other institutions

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

- There is a large, widespread mix of collaborative partners. Seven of the sixteen partners provided cost-share contributions. In addition, critical roles were identified for all the partners, including the local electric

company, which, while only an observer, is the critical potential future user (or non-user) of this technology.
- The collaboration on this project is outstanding. The team has been working with several entities to make this project a success. The team has had to meet requirements from each of the partners to make this a success, which has caused the project delays.
- The project has assembled a good team, including partners in state and local government and industry. Inclusion of an electrolyzer company (Proton Onsite) is especially valuable.
- The project has a strong list of collaborators that covers all aspects of the project.
- The collaboration team includes many local partners. It would be valuable to understand how these collaborations have addressed (or will address) barriers identified for this project. One example is hydrogen safety training. More examples are needed to avoid being in similar situations in which it takes more than six years to get one site completed.

Question 5: Proposed future work

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

- The project is noteworthy in that while DOE funding has ended, the Hawaii Natural Energy Institute project was valuable enough that it continues to draw funding from other sources.
- The project identified a detailed list of future efforts.
- Equipment installation is planned for August 2016, but significant work must be completed before then. The August timeline seems aggressive and may not be achievable if any new barriers arise before then.
- The proposed future work is reasonable, though a timeline for completion is needed.
- Actual site completion is needed and will likely be completed this year. The proposed future work bullets are similar to those from 2015, and it is unclear why there are delays (e.g., “Complete mass transit agency shuttle bus conversion” was scheduled for September 2015 and is still listed as future work in 2016). Analysis of the performance data and the dynamic model is interesting, has potential, and will hopefully be worked on in collaboration with other electrolyzer grid services projects (e.g. Idaho National Laboratory/NREL dynamic grid electrolyzer validation).

Project strengths:

- The project ties together multiple benefits (electrolyzer demonstration, renewable hydrogen for fuel cell deployments, and enablement of intermittent renewables) into a single package. The project also helps increase awareness and clarifies the permitting process for deployments.
- The project has broken new ground and has had to work with many organizations to get the approvals needed to move forward. The lessons learned can be used as an example for similar projects. There is good leveraging of funds and participation from numerous groups. There is good partnership and development with Proton OnSite on improving the hydrogen system.
- The project developed a strong team with positive support from local and state officials.
- This is an honest look at a potential use for hydrogen.
- The island has many reasons to deploy electrolyzers owing to its goals for high renewable energy use.

Project weaknesses:

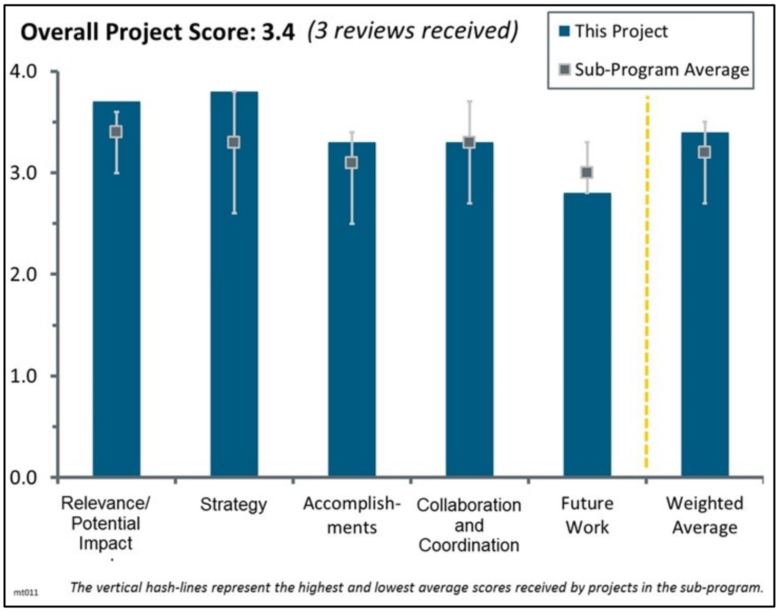
- Working with so many partners has resulted in lots of red tape, which has caused delays. When successful, this project will pave the way for more HES installations in Hawaii, but because it is the first of its kind, it has encountered barriers that prevented the project from meeting the original timeline.
- Delays in the project seem only to confirm that there are barriers; the project does not seem to be addressing the barriers.
- The project needs a clear timeline and a schedule of milestones so that adequate and timely performance can be demonstrated.
- It remains to be determined whether the electrolyzer can be modified to meet ramp rate requirements.

Recommendations for additions/deletions to project scope:

- The project should keep pushing the timeline so that no other delays occur.
- Interaction with the electrolyzer manufacturer should be accelerated. It is unclear what happens to a warrantee.

Project #MT-011: Ground Support Equipment Demonstration

Jim Petrecky; Plug Power



Brief Summary of Project:

The objectives of this project are to develop fuel cell-powered ground support equipment (GSE) that (1) are cost-competitive and more energy efficient, (2) are lower in carbon emissions, (3) reduce consumption of diesel, (4) decrease energy expenditures, and (5) validate the value proposition. These objectives are supported through vehicle testing of the Charlatte CT5E tractor, FedEx dollies, and shock testing at Memphis-Shelby County Airport and the Memphis Division of Fire Services.

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 **3.7** for its relevance/potential impact.

- The project is critical to the development of hydrogen and fuel cells because GSE could be the next major commercial application for fuel cell technologies (i.e., GSE could be the next major advancement after critical power needs, specialized applications, and lift equipment).
- This project, in particular, seems to have a high potential to meet Hydrogen and Fuel Cells Program goals and enable demonstration for a wide breadth of additional applications.
- The overview approach was very good. The “Relevance/Potential Impact” would have been rated as outstanding except the presentation did not relate the activities in the overview to the U.S. Department of Energy’s (DOE’s) barriers. It was unclear why the presentation did not respond to the DOE question on relevance.

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 project is well organized and structured and identifies the key elements for a successful deployment of the fuel cell systems. The project includes not only the fuel cell system but also the delivery, storage, and dispensing of hydrogen fuel. The project has addressed safety issues, including hazard and operability study and operator training. The project’s approach could serve as the basis and reference point for future fuel cell system deployments.
- The presentation was extremely clear and direct; the processes were straightforward and understandable, and all barriers were addressed.
- Investigation of various possibilities for hydrogen procurement adds important value to this project. However, some of the details of the costs for the hydrogen pathways may need reconsideration or further explanation as they do not appear to align with other points of reference. In particular, the cost difference between procurement of gaseous hydrogen and liquid hydrogen alone seems to imply the two options were not evaluated on equivalent bases.

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 a very good project and presentation, fully focused on critical barriers. There are no recommendations for improvement.
- A lot of good progress had been made in terms of evaluation, design, and development of learnings. However, the difficulties described with the fuel cell seem like they have held up the overall project timeline. For a project in this program area, it seems the fuel cells chosen were not appropriate.
- Design and prototyping is well underway. The project reports the performance during year one fell short of the demonstration targets. The project did not explain why the airport truck demonstration missed the targets or what the specific problem components were. It was unclear whether the underperforming components were all balance-of-plant (BOP) components or whether there were stack components that also underperformed. Identifying these issues will help all fuel cell original equipment manufacturers (OEMs) and suppliers, which is an objective of DOE. How the project will correct these deficiencies and whether the alternatives meet the performance and cost objectives were unclear.

Question 4: Collaboration and coordination with other institutions

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

- Collaboration, especially with the demonstration host, seems to be essential for this type of project and seems to have been carried out well.
- The project has a well-organized team that includes end-user and application sites.
- The Plug Power team partnered with FedEx (the user), Charlatte (tractor OEM making the non-fuel cell part of the equipment), the Memphis-Shelby County Airport (the site where the activity occurred), and the Memphis Fire Department (the regulating authority). It is not clear what programmatic role any of Plug Power's partners had beyond FedEx agreeing to use fuel cell equipment, Charlatte modifying equipment they manufacture (if they did), and the airport and fire department performing business as normal (or close to it). While few in number, the role of each of Plug Power's partners appears to be very, very limited.

Question 5: Proposed future work

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

- The integration into the FedEx tracking system should receive particular focus, as it will be important to evaluate the implications for maintenance cost and work that the fuel cell system brings to the GSE application.
- From the presentation, the future work seems to be limited to maintaining and repairing equipment.
- It is unclear whether FedEx will take over service in order to address performance deficiencies with replacement parts. It was also unclear whether the proposed increase in FedEx's involvement suggests that Plug Power will always need to have its technicians onsite. Performing an economic evaluation of the total cost of ownership is a good idea. The presentation did not indicate whether the total cost of ownership will be evaluated without the federal government subsidy.

Project strengths:

- This projects seems like it has great potential for lessons that can be utilized in other applications. Expansion of the fuel cell system demonstrated in this work to other medium-duty applications, or into facilities with multiple platform opportunities (like this project's host airport where many types of vehicles and equipment are important), seems highly probable.
- The project has a strong team and leading experience in the development of the airport truck. Another strength is the project's broad coverage of the technology from the fuel cell system to hydrogen delivery, storage, and dispensing. Finally, the project has strong interaction among partners.
- The performance by Plug Power is a strength.

Project weaknesses:

- It seems that the fuel cell stacks chosen for the demonstration were not ready to be integrated into a market demonstration program yet. This is a concern for a project in this program area. Additionally, because the prime is a fuel cell manufacturer themselves and because the proposed solution is to use their own fuel cells in the next set of demonstration units, it is not clear why the first strategy for the project was to use third-party fuel cells. There may be valid technical reasons this approach was tried, but the discussion did not make this known.
- The project does not identify the components, either in the fuel cell stack or BOP, that are not performing to targets. This omission is not beneficial to the general public; rather, the omission only benefits Plug Power. DOE projects should not selectively benefit one class of people or one company. Fuel cell systems are still dependent on tax credits. Another weakness of the project was the limited contributions by Plug Power's partners.

Recommendations for additions/deletions to project scope:

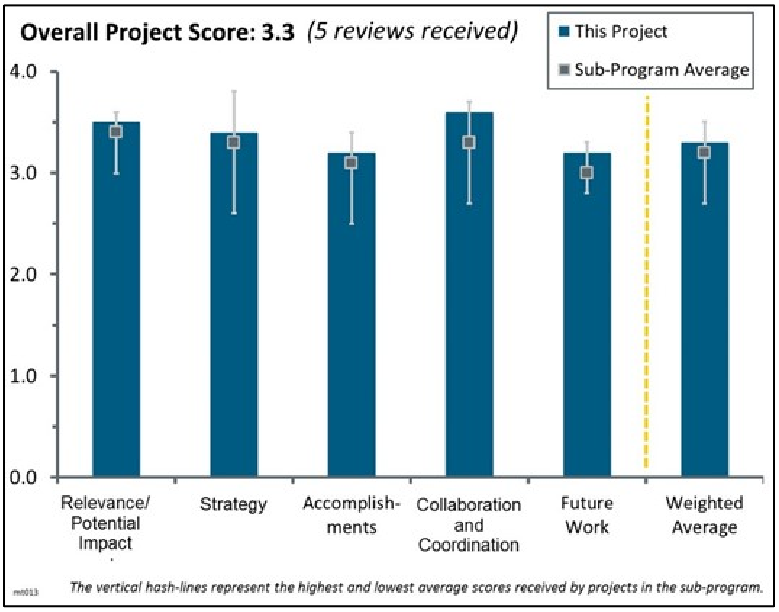
- The discussion of the comparison to incumbent technology should also consider a diesel (or other powertrain) hybridized option as a point of reference. The efficiency bonus of the fuel cell was presented, and the importance of regenerative braking in particular was mentioned as a key a factor. It therefore seems that an equitable comparison to diesel-powered vehicles would require consideration of a hybridized diesel to isolate the benefit of the fuel cell.
- The total cost analysis should be evaluated without the federal or state subsidies and tax credits.
- Although there was a lot of good work in the project, Plug Power might have improved its success if it had developed a stronger team effort.

Project #MT-013: Maritime Fuel Cell Generator Project

Joe Pratt; Sandia National Laboratories

Brief Summary of Project:

The overall objectives of this project are to (1) lower the technology risk of future maritime fuel cell deployments by providing performance data on hydrogen proton exchange membrane fuel cell technology in this environment, (2) lower the investment risk by providing a validated business case assessment for this and future potential projects, (3) enable easier permitting and acceptance of hydrogen fuel cell technology in maritime applications by assisting the U.S. Coast Guard and American Bureau of Shipping to develop hydrogen and fuel cell codes and standards, (4) act as a stepping stone for more widespread shipboard fuel cell auxiliary power unit deployments, and (5) 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 **3.5** for its relevance/potential impact.

- Sometimes, more advancement and learning comes from mistakes and failure than from unequivocal success. Notable in this presentation was the frankness of the principal investigator's (PI's) admissions and the description of the lessons learned from those mistakes and failures. That said, the goals, relevance, and importance of this project are significant in that the project examined an application while also providing a venue for process improvement. The project and the PI did not just align with U.S. Department of Energy (DOE) research, development, and demonstration goals; the project very much exceeded, and provided great advancement toward, those goals.
- The Maritime Fuel Cell Generator Project plays a role in meeting the Market Transformation program goal to enable and accelerate expansion of hydrogen and fuel cell system use by targeting ports and other maritime applications. This project begins the process of raising awareness about possible applications, acceptance of the technology, understanding the hazards, and addressing the codes and standards related to this work. Even if the demonstration was not completely successful, the connections made and the inroads made are worth the effort. The outreach on this project was exceptional.
- Addressing maritime emissions in any capacity is a major advancement, especially considering the scale of the emissions reductions needed and the relatively low number of projects addressing emissions. This is a unique project, and the application has high value.
- The project has good relevance, specifically in lowering emissions and addressing technological and financial risks in a market that needs more efficient power technology.
- The project's relevance and potential impact focused on the application's benefits, e.g., lowering port emissions and reducing business risk. These are beneficial. The presentation did not associate relevance and potential impact directly with the DOE barriers. The project PI needs to recognize the DOE barriers are primary drivers for the project.

Question 2: Strategy for technology validation and/or deployment

This project was rated **3.4** for its project design, approach to addressing barriers, feasibility, and integration with other efforts.

- This project adequately addresses the barriers identified. In terms of adequacy of standards, the project employed the American Bureau of Shipping to ensure the product met required codes and standards, and then it successfully navigated the permitting process. Some issues remain since system refueling is not performed on site. Although details are not provided, a Zero Emission Hydrogen Vessel Working Group and "train the trainer" safety training to address these issues seem like good ideas. The data gathered from the project should help demonstrate the benefits of fuel cells addressing the barriers of lack of cost and performance data and inadequate user experience. The project did a good job of involving all the major stakeholders for the project. As part of the final report, it will be important to include the lessons learned specifically for similar projects as well as generally for future demonstrations.
- The project team may have stumbled across some unforeseen barriers, but they addressed those barriers in a noteworthy and commendable way. The integration of the team's lessons learned experiences into overcoming future barriers appears direct and complete.
- The project outlined the systematic approach it executed to meet project milestones and demonstrate, validate, and deploy the technology.
- The planned approach seemed to be a complete and well-developed one. It is unfortunate that the unit has not yet been able to be deployed on the barge itself, as this would provide information about fuel cell operation in an environment, application, and platform that has not been very thoroughly investigated to date.
- One of the biggest challenges to this project is the strategy of demonstrating a prototype with a customer when that prototype has not gone through enough testing to be ready for customer validation.

Question 3: Accomplishments and progress toward overall project and DOE goals

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

- The DOE goal to enable and accelerate expansion of hydrogen and fuel cell system use was achieved, and the barriers to implementation of this technology were addressed. It is hoped that the lessons learned from this deployment can be applied to similar technologies and to other ports. One area of additional need is to evaluate the market share of the various port options, both in Hawaii and at other ports in the United States. This should be part of the future work business case.
- The team seemed to stumble into a number of barriers, both predicted and unforeseen, and appears to have done well to overcome those barriers as well as possible. It is unclear that the project team foresaw all the barriers, but that is at times the nature of research. The team seemed to learn and then demonstrated the moral courage to report openly and honestly.
- Getting the unit to the site for testing is a good accomplishment, as is the amount of outreach. Completing only 200 hours and 8 fills over 8 months seems too low. It is unclear what the project's expected or preferred operating goals were for those items.
- Progress on the demonstration itself certainly seems to have suffered. However, the reasons were discussed and understandable, given the intent of having the demonstration completed with minimal PI intervention. It will be important to ensure that the lessons learned developed through this project are well documented and communicated in the project deliverables.
- The project demonstrated operation with original equipment manufacturer (OEM) assistance for less than 48 hours. The project was active for nine months but accomplished only 200 hours of operation. It is unclear if that is a good or very limited operational result, and this needs to be explained by the PI. The project demonstrated hydrogen refilling. The project identified that inverters are a roadblock to widespread fuel cell generation deployment. This is somewhat of a surprise since stationary fuel cells using inverters is not a new technology and has been in use for more than 15 years. It is unclear if this issue was due to a particular supplier. It needs to be determined whether Hydrogenics has any suggestions for resolving the inverter issue. Operator issues suggest a poor interface with or commitment by collaborators. The project is working to resolve collaboration issues through outreach programs.

Question 4: Collaboration and coordination with other institutions

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

- The collaboration on this project was exceptional. All the major stakeholders were included and actively involved. Safety training was a key aspect of the project and was provided for all project partners. A significant amount of training occurred as a result of the "train the trainers" effort. It would be good to disseminate the project results beyond Hawaii to other ports, especially in California and the Northeast.
- The collaborations are key to this project's successfully addressing permitting and customer barriers.
- The coordination of this project is complex and requires a large number of collaborators to be involved, and the project looks to be managing the cooperative efforts very well.
- Sandia National Laboratory appears to have a large, balanced number of team partners and used them fairly extensively, i.e., the partners were active participants rather than markers to be added for a line count on a page.
- Collaboration was identified as an issue by the PI.

Question 5: Proposed future work

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

- The top-priority activities for the future are the development of a business case, identification of follow-on opportunities, and continuing the outreach both in Hawaii and other ports, as identified by the project. Gathering lessons learned and addressing operational issues will help make the next deployment more successful. Additional deployments of this system are needed. Concrete plans for expanding the number of deployments should be created.
- The project team's focus is unclear. A number of unforeseen barriers seem to have arisen, and while it is unclear that the planning was complete enough, there is no question as to the team's integrity, the completeness of their report, or the team's value for future work.
- If the customer is not using the system as much as possible, it is unclear whether the project should continue trying to push operation and on-site fueling. It is not clear how much information is needed for the technical and business case analysis or how long that will take.
- Deployment on the barge was mentioned, though it was not clear that could be completed within the budgeted timeline for this project. If not, that will be a major loss for the project.
- The project identifies the next steps and is aware of technical problems. The project should identify more specific steps that would resolve technical and operational issues. The presentation did not provide a high level of confidence the future plans would resolve technical and operational issues.

Project strengths:

- The project's principal strength was the integrity of its management team. A second and nearly equivalent strength was the contribution made by the team to DOE's research, development, and demonstration goals.
- The team has identified an important application and is working with an experienced fuel cell OEM. The team is systematically solving issues as they arise.
- The unique application and market segment addressed by this project make it very important. It will be important to ensure that as many lessons learned are captured as possible and shared with future projects, especially in the context of maritime fuel cell operations.
- This project has strong collaborations and management.

Project weaknesses:

- The loss of operating time due to communication and staffing challenges with the operator are clearly the project's weakness. At this point, there may not be any change that can be made to address the issue, but lessons learned from the experience can and should be maximized.
- The project needs a firmer commitment from the Young Brothers if this is the source of the operator problem. Help from DOE and local government might improve the focus of the Young Brothers. The

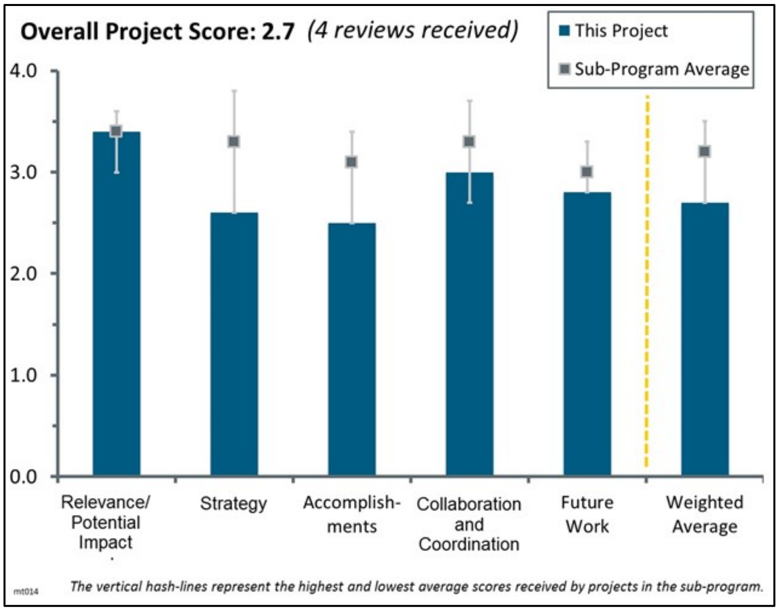
inverter problem is a surprise because inverters have been used for more than 15 years. It is unclear whether this is because of a lack of support from Hydrogenics.
- It may be that the project has delivered a prototype to a customer without sufficient testing and improvements prior to field trials. The inverter issues and other non-technical issues related to operation highlight challenges with placing known technologies in operating conditions outside of the norm. The interface areas are often where issues arise, and not enough focus on development was given to these interface issues.

Recommendations for additions/deletions to project scope:

- The project should increase the interaction with both Hydrogenics and the Young Brothers to solve problems related to delays. Federal and local governments should be used to help with increasing the commitment of the Young Brothers and Hydrogenics. The impact of salt water spray on the performance of the fuel cell should be discussed.

Project #MT-014: Demonstration of Fuel Cell Auxiliary Power Unit to Power Truck Refrigeration Units in Refrigerated Trucks

Kriston Brooks; Pacific Northwest National Laboratory



Brief Summary of Project:

The purpose of this project is to demonstrate the viability of fuel-cell-based transport refrigeration units (TRUs) for refrigerated Class 8 trucks using demonstrations and business case development. Two fuel cell systems will be developed and deployed in commercial operations. Investigators will assess system performance and analyze market viability.

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 potential impacts of TRUs in fuel cell markets can be significant, and the deployment of relevant technologies with suitable business models seems to be highly relevant to U.S. Department of Energy (DOE) goals and objectives.
- Project relevance is good because it is a logical extension of other fuel cell applications such as forklifts. The project can have significant impact on emissions and noise reduction goals.
- The project is relevant to the goal of accelerating market introduction of fuel cell technology, and the target market selected represents a reasonable opportunity for fuel cells to make inroads.
- The presentation provided a well-organized list of explanations of how the project would address commercial applications, socio-economic benefits, and barriers identified by DOE.

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 objectives and approaches of the project are well defined, with specific parameters for data collection regarding technology validation and business case development.
- TRUs are an appropriately sized market for commercialization efforts, and the clear advantages of fuel cell technology for this application suggest that fuel cells may be viable. The data collection and market analysis will help determine how much effort should be invested in this market going forward. The project team needs to get a better handle on the technical requirements, though. The reference to DOE targets for fuel cell auxiliary power units (APUs) is inappropriate for this application, as those targets were developed for heavy-duty truck APU applications using high-temperature fuel cells running on diesel fuel.
- Technology testing does not seem to be sufficient prior to customer deployment. Customer test locations are not necessarily tied into any other existing infrastructure, which makes it both more expensive and a higher risk for technical infrastructure issues because there may not be a good backup option.

- The project strategy for technology validation and deployment was hindered by the choice of fuel cell original equipment manufacturers. Nuvera fuel cell systems have had a mixed success record with DOE projects. Considering past history, it is surprising that DOE would continue to support projects with Nuvera fuel cells. The project approach is systematic and well organized. The data collection process is structured well.

Question 3: 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.

- Nuvera's "supplier pause" makes the evaluation of accomplishments this year difficult. The supplier pause is from February 2015 to September 2016, so it is unclear if the accomplishments related to the supplier are from past years or the current year. The presentation is unclear, as it indicates that the fuel cell is less efficient since the system's hydrogen usage per day is higher than the gallon of diesel equivalent (GDE) for diesel usage. It would be good to see this comparison for emissions reduction in GDEs.
- Progress to date has been slow, apparently in part because of issues with the fuel cell system supplier. The recent addition of another supplier should accelerate progress. The first supplier (Nuvera) has been performing component and program-system testing and has integrated one system with a TRU for a four-hour test. Real world testing will not begin until next year.
- The presentation of the tipping point between positive, marginal, and negative net present values is very beneficial. DOE should consider similar presentations for its other projects. Project accomplishments are delayed because of Nuvera. Drawings of fuel cell systems were presented in some cases, and this suggests that the "real" fuel cell system has not been fabricated. It is unclear what the status of the fuel cell system is. The system has a very low operational time, at first 2.5 hours and then 4 hours. The fuel cell system may not have reached equilibrium operation point; if this is just beginning of life data it is not very beneficial.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- The supplier addition is good, along with other companies, but if a supplier has been added, it is unclear why "TBD" (to be determined) is listed on the collaboration slide.
- The project has been limited by delays in assembling a team. The recent addition of a new fuel cell supplier and demonstration partner will help.
- The Nuvera partnership does not appear to be beneficial.

Question 5: Proposed future work

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

- The project is scheduled to perform two 400-hour demonstrations next year and to analyze data from the demonstration to assess the viability of the TRU market for fuel cells. The timeline for the demonstration with the recently added partners is not yet clear.
- Considering all of the difficulties and lack of performance, the proposed future work should contain more detail. For example, a definition was not provided for "sized appropriately" for developing the system. The project should know this already. It is unclear why value propositions are only being identified now.
- A number of items related to the choice of demonstration sites and reliance on mobile refuelers do not appear to have been considered. It is unclear how mobile hydrogen is paid for and what happens if the mobile refueler is not available. It is unclear what plans are in place for when the demonstration is completed. It is unclear what go/no-go criteria have to be met before the integrated system is placed at the customer site.

Project strengths:

- The project presents an excellent market opportunity and good impact, if successful.
- The identified market appears to be a promising opportunity for near-term fuel cell commercialization.
- This project does not appear to have any strengths.

Project weaknesses:

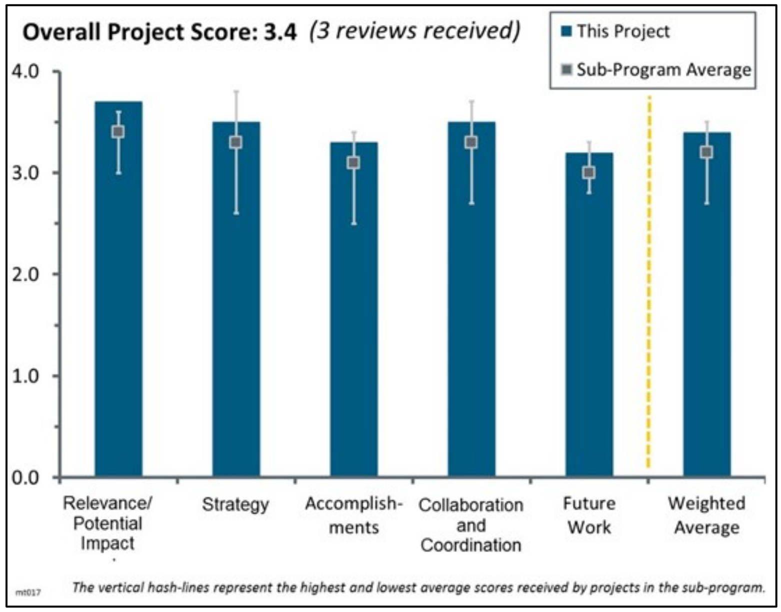
- Project weaknesses include site selection, putting technology at too low of a technology readiness level with a customer, and schedule delays.
- Progress has been slow, and the degree of commitment on the part of the industrial partners is questionable.
- Nuvera is a primary weakness. Pacific Northwest National Laboratory (PNNL) does not appear to have other aspects of this program well organized. It is unclear why PNNL is defining the value proposition after operating the project three to four years. Nuvera will complete two 400-hour tests, but the approach calls for a test of 800–1000 hours.

Recommendations for additions/deletions to project scope:

- One of the main goals of this project could be development of working business case models for an early fuel-cell-based TRU market. The project should conduct a detailed analysis of the customers' needs and expectations regarding TRUs and create solid risk management plans to enable the project to cope with potential system failures during the field demonstration; this would help with acceptance of the technology.
- This is a rare occurrence of a Fuel Cell Technologies Office (FCTO) project that is and has been in a lot of trouble. FCTO should consider terminating the project or having the project refocused with a totally new team.

Project #MT-017: Medium-Duty Parcel Delivery Truck

Thomas Griffin; FedEx Corporation



Brief Summary of Project:

This project will demonstrate hydrogen and fuel cell technologies in real-world environments. Fuels cells are being integrated into 20 battery electric pickup and delivery vehicles. Those trucks will operate in 10-hour shifts, 260 days annually, amounting to at least 5,000 hours per truck for a total of 100,000 hours over 1.92 years. The project is expected to reduce diesel consumption by 100,000 gallons and prevent 270 metric tons of carbon dioxide.

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 **3.7** for its relevance/potential impact.

- Fuel cells as range extenders for battery electric vehicles (BEVs) appear to be a large potential market, given the vast number of fleet vehicles throughout the United States. FedEx Corporation's willingness to step into this new market and invest in and evaluate the technology is commendable. This project fits well within U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program goals and objectives. Going forward, it would be beneficial if FedEx would advertise its use of fuel cells to encourage other similar companies to consider them. It is hoped that FedEx could label trucks with fuel cells to bring greater awareness and acceptance to the community as well.
- The relevance of DOE goals and the commercial goals of FedEx are clearly stated and consistent. FedEx's long-term goals to reduce greenhouse gas emissions, improve efficiency, and reduce operating costs are defined, and a vision of future benefits and applications is clearly stated. FedEx provides perspective for multiple applications of fuel cell systems in electric vehicles dependent on delivery requirements.
- This project has direct relevance to the DOE goals of demonstrating the viability and potential business case for fuel cells in a continually broadening range of vehicle-based applications.

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 strategy of bringing one system online, evaluating its performance, and then bringing on 19 systems at various sites seems reasonable. The information the team collects from each of the trucks should include the efficiency and maintenance required during the demonstration. This information will be beneficial to any future demonstration and to the fuel cell community at large. It is assumed that there will be feedback from the first truck and modifications made to the subsequent 19 units based on the lessons learned from the first one. However, that is not in the schedule. The schedule is very aggressive. The team should make sure that there is adequate time spent on addressing safety issues and first article testing of the integrated

system. It is good to see that durability testing and dyno testing are being performed. Poor performance and especially safety issues have impacts on the entire fuel cell community.
- The project seems to align with DOE's and FedEx's individual strategies, and this was discussed, but there was not substantial discussion of details in the project's strategy.
- The approach and milestones status are reported. It is not clear why plans for Task 2 through Task 5 of Demonstrations need to be revised. It is not clear whether there was an error in Task 5 for the revised plan or whether Task 5 was accelerated (Chart 10). The presentation should have identified what barriers the approach was addressing.

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 project is just getting started, so not much progress has been made. The reviewer looks forward to evaluating the project progress next year. It is good to see that a model has been developed to optimize the sizing of the battery and fuel cell. It would have been useful to show the assumptions of the model and the design space evaluated to optimize the system.
- There has clearly been a setback with collaborators. However, interesting work has been completed in the meantime, especially the evaluation of the duty cycle and the design of appropriate system specifications.
- The project is in the startup stage, and its progress/status is reported. The project is defining drive cycle limits for system development (although it is unclear what a 60 mile "stem" length means). The project is defining the right-sizing of operation and predicting usage of fuel cells and hydrogen, which offers a good perspective of the issues and requirements for the fuel cell and hydrogen storage. It is not clear why a replacement electric vehicle original equipment manufacturer (OEM) was identified; it was not explained on the slides.

Question 4: Collaboration and coordination with other institutions

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

- A strong, experienced team was identified. The fuel cell systems OEM is highly experienced, and the National Renewable Energy Laboratory is a well-established contributor for data collection. Vehicle safety regulations will be covered by Transportation.gov.
- The collaborators selected appear to be sufficient to perform the demonstration with all the right expertise. It would be beneficial to the Fuel Cell Technologies Office (FCTO) to have other collaborators that could participate at a low level and be aware of progress so that they could eventually have a similar demonstration of their own.
- It appears that the project had some initial difficulties with coordination, but the project has recovered from the setback. This ability to move past the setback is important.

Question 5: Proposed future work

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

- Topics are listed but lack the detailing of responsibilities for team members. A top-level overview of the effort is needed. The Future Effort slide could include startup month and responsibilities.
- There is no discussion in the future work as to how hydrogen is going to be provided for refueling both in the short and long terms. A major piece of the FCTO objective is to address the development of the hydrogen infrastructure. How this will be advertised and how that outreach will be planned is also important.
- The project seems to be budgeted for 21 vehicles; however, there was no discussion of contingencies in case unexpected delays or difficulties appear with the first demonstration unit. It is not clear how the budget for the remaining 20 vehicles will be managed in this case.

Project strengths:

- The principal investigator is from an experienced company with well-established goals consistent with DOE goals. The fuel cell OEM is experienced and has demonstrated a commitment to success in other projects.
- The real-world setting and operating requirements, and a selected set of partners to demonstrate this, are real strengths of this project.

Project weaknesses:

- The presentation was a bit short on details for the work plan. Other than this, no major weaknesses are identified.
- It is too early in the project to identify whether there are weaknesses.

Recommendations for additions/deletions to project scope:

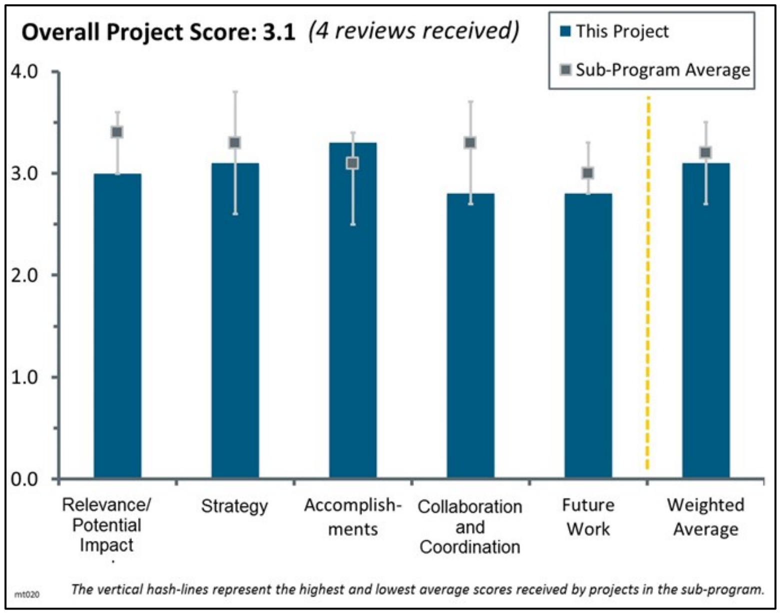
- The project should provide additional inputs on the responsibilities of partners and how achievement of FedEx goals addresses the elimination of DOE barriers.

Project #MT-020: Fuel Cell–Battery Electric Hybrid for Utility or Municipal Medium- or Heavy-Duty Bucket Trucks – Fuel-Cell-Powered Auxiliary Power Module

Abas Goodarzi; US Hybrid Corporation

Brief Summary of Project:

Medium- and heavy-duty bucket trucks are used daily by line crews employed by utilities, telecommunications companies, and municipalities to repair infrastructure. An electrified powertrain reduces operating costs from fuel and maintenance and enables improved handling and noise reduction. US Hybrid Corporation (US Hybrid) is developing fuel-cell-powered auxiliary power modules (APMs) and identifying the most promising APM in terms of commercial viability. This APM will be integrated into a bucket truck for a 200-hour demonstration. Investigators will collect data during operation and provide performance and energy analysis.



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 **3.0** for its relevance/potential impact.

- Bucket trucks and other similar truck applications appear to be an excellent niche market for fuel cells. The reason for their use is quite compelling. These vehicles are distributed throughout the United States and could help increase the use of fuel cells and build out the hydrogen infrastructure. There is concern that the bucket hydraulics and the trucks themselves do not have high enough utilization to justify the increased cost of a fuel cell. If they are used only in the event of a power outage or repair, for example, the cost per hour of usage could be very high. It would be wise to determine the yearly utilization.
- The project would reduce greenhouse gas emissions and oil consumption. There is not enough information to understand definitively the efficiency of the energy conversion. It would reduce air pollution with quiet operation. This has the potential to make a positive impact on the bucket truck industry, but given the complete absence of cost and budgets, and the projected unit costs, it is unknown whether it would be saleable. No economic analysis results are provided. However, after the poster session, it was learned that the project is funded via the Small Business Innovation Research (SBIR) program, not as a full multi-million dollar funding opportunity announcement (FOA) project. The project has made very good progress.
- The potential impact will be very limited without a better financial analysis. It is widely known that fuel cells are clean and quiet, so the functionality of a fuel-cell-powered bucket is not in question in a one-off demonstration. However, one has to show a business case in order to make an impact.
- The project has conducted a paper study on the viability of fuel-cell-powered bucket trucks. Commercialization of fuel cells to power the hydraulic boom and other non-motive power needs appears possible, though cost (including fueling infrastructure) will limit this market to niche applications in the near term.

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 project strategy is to perform an analysis to select the most viable configuration of a fuel-cell-powered bucket truck (Phase I) and to produce and deploy a bucket truck in a demonstration project at the Hawaiian Electric Company (HECO) (Phase II). This represents a reasonable strategy to assess the viability of fuel cells in the target market.
- No barriers were specifically called out in the poster presentation as required by the DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) formatting. However, it seems that several barriers were addressed, including the following: C. Inadequate private funds available for infrastructure development and F. Inadequate user experience for fuel cell applications. Barrier C could be better addressed if potential funding mechanisms were defined and the market was sized to give some idea of the cost and size of increased infrastructure development through bucket trucks and similar fleet applications. Barrier F will occur as the project proceeds and efforts are made to publicize the demonstration and its results. Other barriers may also be applicable. US Hybrid is well suited for leading the project, and the scope of the work is very feasible.
- There are no detailed demonstration plans beyond the statement that it will be demonstrated for 200 hours by HECO. It is unknown whether this includes only 200 hours of operations or 200 hours of possession. There should be independent data collection and testing, and there should be dynamometer testing. As presented, it does not appear to be a well-designed project, and there is an absence of go/no-go decisions. There is no detail on the battery storage system in terms of a manufacturer and organization support in the integration. While slide 11 lists Phase II future work, it is not tied by dates to the tasks in slide 12. There is minimal information about the supply of hydrogen infrastructure in terms of storage and speed to fuel. While the infrastructure is not part of the project, it is critical to success. However, again, it is an SBIR-funded project, so for this level of funding, the approach details are appropriate.

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 funding for this project was not provided as required in the overview AMR format. In spite of this, as a Phase I SBIR, this was not a large project. The researchers did estimate the duty cycle and emissions, evaluate system configuration alternatives in order to down-select to one option, estimate fuel use and operating costs, and select a possible demonstration location. This is substantial progress for an SBIR project and is beyond nominal expectations.
- In Phase I, the project has analyzed duty cycles and produced simulated fuel usage profiles to compare several system configurations. The results of the study support initial commercialization in niche markets of bucket trucks in which non-motive loads are powered by a fuel cell system. Reduction in fuel cell costs could enable more widespread commercialization.
- There are not any performance indicators with which to determine the accomplishments and progress succinctly. However, the SBIR status negates this.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- There is good collaboration in Hawaii with Hawai'i Natural Energy Institute (HNEI) and HECO, and in New England with the Massachusetts Hydrogen Coalition and Eversource Energy.
- Two commercial partners in different regions of the United States have been selected and a possible demonstration drive cycle delineated. It would have been good to discuss more of what is planned for a demonstration in Massachusetts and what the drive cycle looks like for this location.

- US Hybrid will integrate its technology into a commercial bucket truck, and HECO will demonstrate the truck in operation. Other listed partners include HNEI, the Massachusetts Hydrogen Coalition, and Eversource Energy, but the roles and responsibilities of the various partners were not clearly presented.
- It was stated that HECO would demonstrate the vehicle for 200 hours, but there is no indication of the collaboration and coordination with HNEI, Eversource Energy, or the Massachusetts Hydrogen Coalition. It is mentioned that HECO and Altec Inc. will tune the operations; the PI discussed neither what the relationship was between HECO and Altec Inc. nor the scope of the tuning operation.

Question 5: Proposed future work

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

- Proposed future work (in Phase II) consists of integration of a fuel cell power system into a commercial truck and deployment with HECO for a 200-hour demonstration. This demonstration will provide performance data and operating experience that will be used to evaluate the market potential of fuel-cell-powered bucket trucks. This is a reasonable plan, though more details on the integration plans would be helpful.
- Future plans are well laid out in terms of steps and responsibilities moving forward into SBIR Phase II. It would have been beneficial to have additional detail on appropriate decision points and to consider barriers to goals. An important part of any of these demonstrations is the development of a solid business case. Future work should include the development of this business case in terms of sizing the potential market, evaluating potential purchasing approaches, and understanding market needs.
- While the future tasks are listed on slide 11, there are no known go/no-go decision points or identification of potential barriers.
- There is no indication of the cost planned for the future work, so there is no way to assess whether the cost and work are commensurate.

Project strengths:

- This project provides a good demonstration of the benefits (clean, quiet, and energy efficient) of a fuel-cell-powered bucket truck.
- Bucket truck applications may represent a good opportunity for near-term deployment of fuel cell technology, and this project is making progress toward evaluating the market.
- There is a clear need for an economically viable bucket truck that does not use an internal combustion engine to power the accessories and electric power take-off.

Project weaknesses:

- The degree of industry and utility interest in adopting fuel-cell-powered bucket trucks is not clear at this point, and while fuel cells present some clear advantages in terms of noise and emissions reductions, it appears unlikely that they will provide any cost savings when fueling infrastructure costs are taken into account.
- There is no financial information provided, no specifications, no independent testing to support DOE decision-making, no calendar-based timeline, no return on investment analysis provided, no go/no-go decisions, and no information about the battery, including the chemistry, the battery management system integration, and how the battery pack will be charged. Also, the SDU should be explained.
- No convincing business case is presented. There is nothing in the poster related to the cost of the fuel cell system or what the payback time from fuel savings might be.

Recommendations for additions/deletions to project scope:

- The project should conduct a realistic business case analysis, taking into account the cost of the fuel cell system and the cost of the hydrogen fuel.
- A more thorough explanation of the project is needed to fully understand the scope. Clearly, independent testing is warranted during the demonstration phase. The project owners should compete for an FOA if an appropriate one is offered by DOE.

2016 — Systems Analysis
Summary of Annual Merit Review of the Systems Analysis Program

Summary of Reviewer Comments on the Systems Analysis Program:

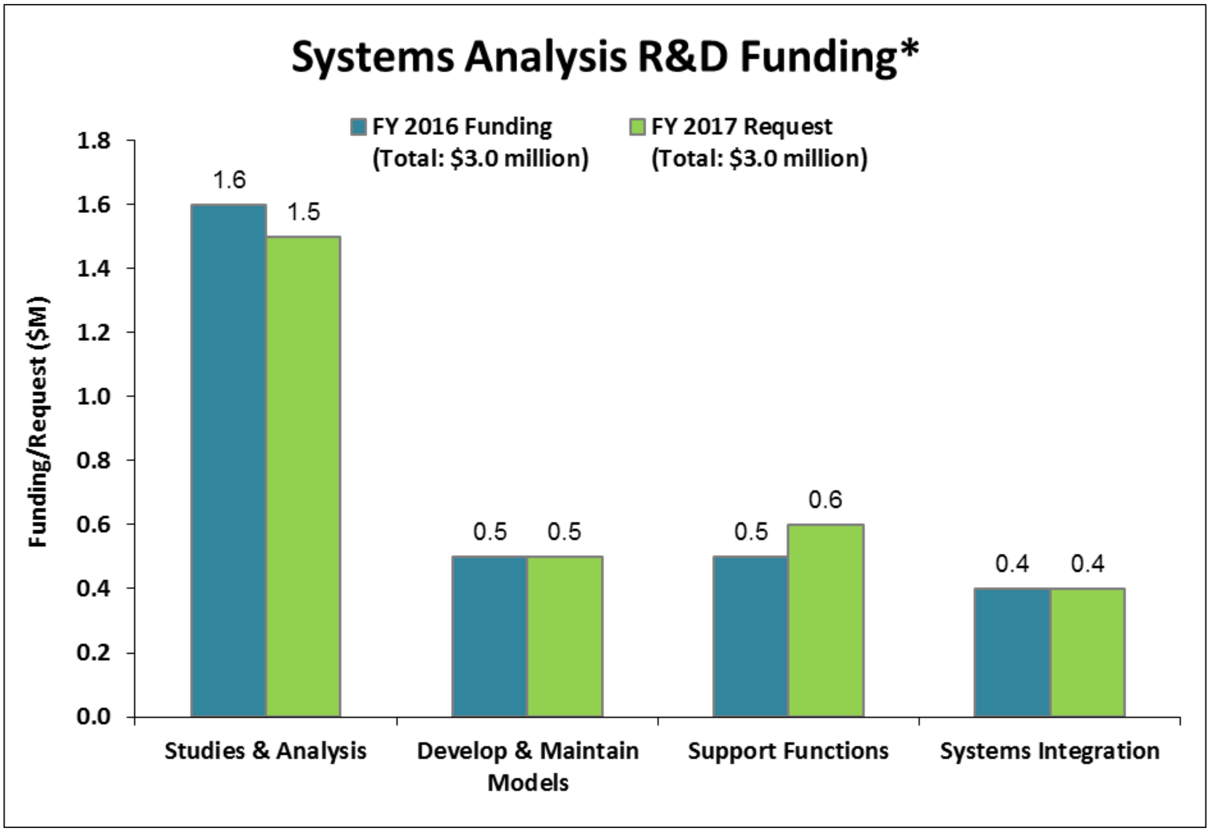
The reviewers considered the Systems Analysis program critical to the U.S. Department of Energy's (DOE) Fuel Cell Technologies Office's (FCTO's) mission and to be focused on the relevant issues that will enable cost-effective implementation of hydrogen fuel cell electric vehicles (FCEVs) in a way that addresses national needs. In general, they noted that the Systems Analysis program is well managed and the projects are diverse and focused on addressing technical barriers and meeting targets. Reviewers commended the program for the excellent mix of near-, mid-, and long-term analyses and the portfolios' focus on assessing progress to targets. They stated that the program has extensive collaboration with industry, national laboratories, and academia and that it exhibits the ability to address immediate analytical needs, meet overall objectives, and focus on supporting hydrogen infrastructure development. Overall, the reviewers commented that the program's research and development (R&D) portfolio is appropriate and comprehensively addresses key technical aspects to achieve the ultimate program targets.

Some reviewers commented that the program is effective in providing analytical support and key insights and guidance for the Hydrogen and Fuel Cell Program's (the Program's) R&D efforts to address key barriers. They noted the Systems Analysis program is one of the strongest areas of the Program because of its extensive interaction and engagement with a broad set of key stakeholders. Reviewers identified the Cradle to Grave analysis and its resultant publication as an example of the program's value. Also, it was noted that the analysis and model portfolio is balanced and has made good progress toward understanding the issues, challenges, and opportunities related to achieving the technical targets. Reviewers praised the program for analyzing near-term issues, such as policies for infrastructure growth, and for focusing on mid-term evaluation of the costs with models such as the Hydrogen Financial Analysis Scenario Tool (H2FAST) and its benefit to states that are developing and evaluating infrastructure deployment.

Key reviewer recommendations for this program include the following: (1) to provide more details on the sustainability project to understand the benefits to the program; (2) to expand analysis to include a longer and wider view of greenhouse gas (GHG) reduction to be more inclusive of the transportation and electrical generation sectors; (3) to continue analysis of consumer behavior and explore further with stakeholders; (4) to incorporate cost analysis for low-volume production and market penetration; (5) to analyze scenarios with carbon taxes and incentives; and (6) to increase funding for the program so that it can continue to address a wide range of analytical topics.

Systems Analysis Funding:

The fiscal year (FY) 2016 appropriation for the Systems Analysis program was $3 million. Funding continues to focus on conducting analysis using the models developed by the program. In particular, analysis projects are concentrated on analysis of early market adoption of fuel cells, continued life cycle analysis of water use for advanced hydrogen production technology pathways, the levelized cost of hydrogen from emerging hydrogen production pathways, employment impacts of hydrogen and fuel cell technologies, the impacts of consumer behavior, the cost of onboard hydrogen storage options and associated GHG emissions and petroleum use, and hydrogen fueling station business assessments. The FY 2017 request level of $3 million, subject to congressional appropriation, provides greater emphasis on analysis of large-scale deployment and utilization of hydrogen through the H2@Scale concept; the employment impacts of hydrogen and fuel cell technologies; sustainability; hydrogen fueling station business assessments; life cycle analysis of GHG emissions and petroleum use for future hydrogen production technology pathways, including solar thermochemical, photobiological, and photoelectrochemical; hydrogen production capacity at the national and regional levels; and the impacts of consumer behavior.



* 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 11 Systems Analysis projects were 3.6, 2.9, and 3.2, respectively.

Infrastructure: The two projects reviewed in this topic area received an average score of 3.0 for assessing the costs of hydrogen infrastructure development and understanding the hydrogen infrastructure costs compared to other alternative vehicle infrastructure.

Reviewers commented that the National Fuel Cell Electric Vehicle and Hydrogen Fueling Station Scenarios project (SA-061) is relevant to assessing FCTO infrastructure goals; made good use of stakeholder experience; and utilized an effective strategy for the scenario analysis of key market parameters. The reviewers noted the project would be strengthened by engaging energy companies to validate their assumptions, especially for scenarios outside California. They recommended the project consider potential upgrades to potential fueling sites to increase capacity as a progression step for infrastructure development.

Reviewers noted that The Business Case for Hydrogen-Powered Passenger Cars: Competition and Solving the Infrastructure Puzzle project (SA-052) is relevant to the infrastructure buildout and informing the investment community. However, the project is in the early stages and does not yet fully evaluate the total program benefits, and it lacks collaboration with key stakeholders such as industrial gas suppliers, the investment community, and infrastructure developers.

Model Development and Systems Integration: Four projects involving model development were reviewed, receiving an average score of 3.4. These projects received favorable reviews and were regarded as well aligned with the current program goals and objectives.

Reviewers acknowledged that expanding the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET) model platform to include water-use life cycle assessment and emerging, renewable hydrogen production pathways to address critical and relevant program issues associated with hydrogen production was beneficial and that the comparative evaluation to conventional fuels was also beneficial. They noted that the model enables industry stakeholders and energy producers to understand the water consumption sensitivity associated with electricity, biofuels, and process cooling methods. Also, the GHG emissions information for the emerging hydrogen production pathways enables DOE to prioritize technology R&D that will be most effective in achieving the largest GHG emissions. Reviewers also found the future work for the GREET project to be robust, but they noted the need for more collaboration with stakeholders, such as utilities. They also stated that future analysis should include uncertainty ranges for the reported results and regional impacts of fuel production on water consumption. The emerging technology expansion for GREET would benefit from the inclusion of international participation and peer review.

Reviewers commented that the Expanded Capabilities for the Hydrogen Financial Analysis Scenario Tool (H2FAST) project (SA-062) aligns well with the program objectives of supporting infrastructure development by providing market and financial information relative to strategies for infrastructure development. They noted that the model is comprehensive with the addition of the stochastic risk analysis capabilities and takes into account the key cost variables required to evaluate a hydrogen station or network. The project made use of an excellent array of collaborators and validation of the model. The reviewers recommended that the ability to evaluate production and logistics options be added to the model.

Programmatic Benefits Analysis: Two projects were reviewed in this topic area, receiving an average score of 3.1 for the sustainability and employment impacts of the institution of hydrogen and fuel cell technologies.

Reviewers commented that the Employment Impacts of Hydrogen and Fuel Cell Technologies project (SA-035) is based on the use of a well-regarded employment model to understand job creation with the development of hydrogen infrastructure and production of fuel cell systems associated with automotive and stationary applications. They found the project to be relevant to understanding the economic benefits and job impacts and that it enables a framework to build in assumptions of the effects of various costs, geographic situations, and market perturbations. The reviewers recommended expanding the work to include competing technologies such as battery electric vehicles and consider the geographic and market implications attributable to displacement of gasoline and regional increases in hydrogen production capacity.

Reviewers commended the addition of the Sustainability Analysis project (SA-059) to the System Analysis program portfolio. They noted that the project takes a rational approach to assessing sustainability for hydrogen pathways by examining environmental and socio-economic aspects. Although the project is in the early stages, the reviewers acknowledged the use of a steering group to guide the project and vet the results and progress. Future work for the project should leverage data from multiple sources to ensure the results are consistent and transparent.

Studies and Analysis: Three analysis projects were reviewed, receiving an average score of 3.1. The projects covered a range of topics, including analysis of incentives and policy and fuel cell storage cost analysis.

Reviewers noted that the Impact of Fuel Cell and Hydrogen Storage Improvements on Fuel Cell Electric Vehicles project (SA-044) has used a very good strategy and has applied the well-respected Argonne National Laboratory Autonomie model to configure FCEV subsystems and assess vehicle cost changes resulting from improved fuel cell peak efficiency and onboard storage. The reviewers acknowledged the project's benefits in assessing FCEV cost and performance resulting from these parameters and provides a useful understanding of the impacts of technology improvements. The reviewers recommended the results include analysis of lower volume fuel cell production, marginal benefits of technology-specific advances be assessed, and sensitivity analysis of market penetration be completed for understanding short-term and long-term effectiveness of FCEVs.

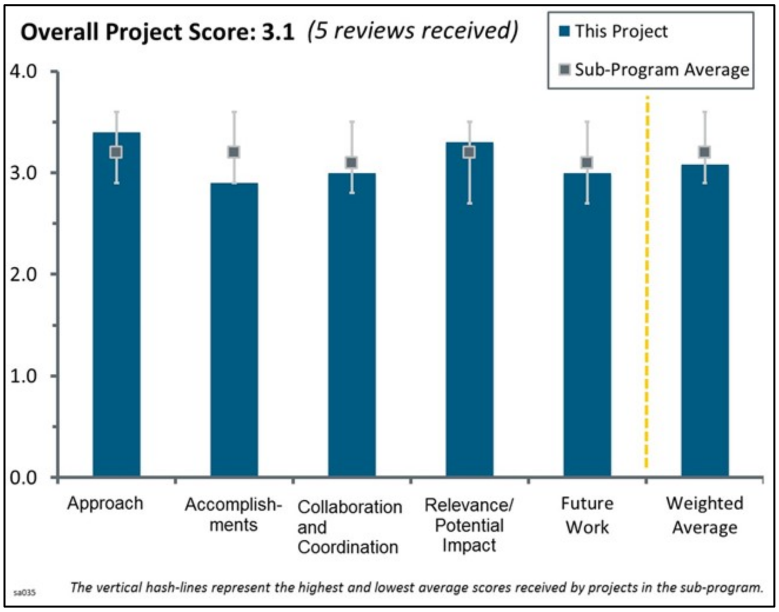
Reviewers stated that the Analysis of Incentives and Policy Impacts on the Market for Alternative Fuels and Vehicles project (SA-058) provides insight about cost drivers and policy impacts of transitioning to alternative fuel vehicles. The "lessons learned" information is informative for federal and state governments in designing effective policies and incentives for FCEVs. The reviewers recommended the information be used for policy development through economic impact analysis. The information search should be expanded to a global literature and information base and include 85% ethanol (E85) and natural gas transportation fueling infrastructure information.

Project #SA-035: Employment Impacts of Hydrogen and Fuel Cell Technologies

Marianne Mintz; Argonne National Laboratory

Brief Summary of Project:

The objectives of this project are to (1) develop a consistent framework to estimate the impact of hydrogen infrastructure investments by the Fuel Cell Technologies Office (FCTO) and others; (2) develop a tool to address barriers/gaps in the FCTO analysis/ modeling portfolio; (3) evaluate impacts of alternative hydrogen and fuel cell infrastructure deployment scenarios; (4) provide input for evaluating FCTO research, development, and deployment targets; (5) work with stakeholders to develop robust, user-friendly tools with appropriate functionality; and (6) report analytical results to demonstrate benefits of the FCTO.



Question 1: Approach to performing the work

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

- The approach seems to have the integrated flexibility to account for geographical and market impacts attributable to displacement of gasoline and regional increases in hydrogen production capacity that could result.
- It was a good approach to switch to the Regional Economic Models, Inc., (REMI) model and to coordinate with National Renewable Energy Laboratory scenario activities. The only drawback might be a lack of comparison with competing technologies, such as battery electric vehicles, or incumbent technologies, such as gasoline and diesel suppliers and vehicles.
- This project's approach is based on the use of a well-regarded employment model to understand job creation associated with development of hydrogen infrastructure and production of fuel cell systems. The analytic framework incorporates an integrated approach that combines economic input–output modeling and general equilibrium modeling to understand the full lifecycle and supply chain of fuel cell and hydrogen infrastructure development. Expanding the analysis to consider non-highway fuel cell applications makes sense, as the market for these fuel cell applications has greatly expanded in recent years.
- The overall approach is good. However, it was not clear how many original equipment manufacturers (OEMs) were engaged. It simply said 30 web-enabled attendees. A breakdown of the different types of attendees (academic, national laboratory, and, most important, industry—OEMs, fuel cells, suppliers, etc.) is needed. The project really needs to include representatives from automotive manufacturers, gas suppliers, and energy companies.
- Analysis is solely focused on hydrogen station construction and station operation, and an input–output model that converts hydrogen station placement to job creation has been developed. It is unclear whether job creation associated with upstream supply chains to stations and hydrogen production, distribution, and operations is included in the jobs growth valuation.

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 project has a well-developed approach and has acquired the models required for the analysis. A good project team has been assembled, and stakeholders have been engaged. A formal advisory group has been structured. It is too early in the project timeline for economic analysis results. Though it appears to be on schedule, development of baseline and alternative scenarios to be evaluated is crucial for project success.
- This project is progressing well, and the project has developed a good framework and roadmap for success. Outreach and solicitation of input through the workshop was an important accomplishment.
- The model framework to date attempts to capture the job categories created and skill sets for mass fuel cell deployment.
- The analysis establishes jobs growth associated with California and U.S. station development through 2021.
- The presentation should provide a breakdown of who the "webshop" attendees were by type of organization: OEMs, suppliers, fuel cell, academic, etc. This information is needed to better understand the results. The results in slide 14 are interesting. There is a very large range indicated (~0%–20%), and the upper limit seems very unrealistic. In 2015, there were approximately 12 million light-duty vehicles sold; assuming that this stays the same until 2025, 10% would be 1.5 million vehicles per year. This is a very large ramp-up in production that the supply chain would probably not be able to economically handle. It is not clear why the participants believed that growth rate would occur. Comparing fuel cell electric vehicle (FCEV) sales to historical hybrid vehicle sales is useful. From 2000 until 2015, sales went from <0.5% of the market to around ~2.8% (peaking at ~3.1%–3.25% in 2013). It would be interesting to know why the participants believed that the FCEV market would grow from <0.5% to 20% in 10 years (2015 to 2025) considering that new fueling infrastructure has to be installed and a new supply chain needs to be developed, especially when compared to the hybrid vehicle market, which did not achieve that growth, and hybrids needed relatively minimal infrastructure changes. On slide 15, it is not clear what FCEV sales were assumed. This is critical to know in order to determine the reliability of the projections. Growing out the FCEV fleet will require significant infrastructure. It is not clear why jobs in the infrastructure were not considered. This seems to be a major gap.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- This project makes excellent use of collaboration. The project taps on personnel and expertise from national laboratories, data analytics firms, and support firms. Use of an advisory group with stakeholders from public agencies, fuel cell and hydrogen infrastructure suppliers, and researchers to assist in data collection, scenario development, and research direction helps ensure necessary expertise is brought to the project.
- There was good engagement in obtaining data from the California Fuel Cell Partnership and California Air Resources Board on California jobs growth. It was also beneficial to engage RCF Economic and Financial Consulting and Northwestern University.
- The collaborations seemed to be California-centric and lacked OEMs and energy companies. The California market has different drivers compared to the rest of the United States. This may skew the results, or it at least assumes that the rest of the country will adopt California's policies, which may not be the case. The project did not have any respondents from automotive manufacturers, gas suppliers, or energy companies.
- Focus groups and input data need to have strong participation from the energy industry (oil and gas and electricity producers).

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 is very relevant, as it helps provide an understanding of the economic benefits and jobs impacts of investments in hydrogen infrastructure and production of fuel cell systems. This provides a necessary piece for policymakers as they consider continued public support of these technologies.
- A consistent way to measure and predict the resulting jobs impact from mass fuel cell deployment is needed. This project provides the initial framework to build in assumptions and study the causes and effects of various costs, geographic situations, and market perturbations.
- There is a need to recognize jobs growth and job cannibalizing of all alternative transportation fuels. Understanding should cover fuel cells and FCEV production growth too.
- The project addresses a key issue for the DOE Hydrogen and Fuel Cells Program.

Question 5: Proposed future work

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

- The work plan for this project is appropriate. The project plan for future work intends to compare jobs impacts for a baseline scenario to alternative scenarios. It is imperative that the yet-to-be-constructed alternative scenarios incorporate a range of assumptions for fuel cell system cost and performance, introduction of hydrogen infrastructure over time and across regions, and public investment (at the federal and state level) in both hydrogen infrastructure and fuel cell systems.
- The Jobs Model is being further expanded beyond the current 2021 forecast horizon. The analysis scope should include upstream supply chains supporting hydrogen infrastructure and FCEV manufacturing. An expanded view of job growth needs to encompass all facets of the overall industry.
- It might be an improvement to anticipate future policy drivers, such as Corporate Average Fuel Economy (CAFE) or carbon pricing policies, to see how those influence the relative benefits.
- It would have been good to see more detail on the future work. Some items to consider for future work include the effect of low oil prices on natural gas prices and the reduction of fracking capacity. If gas prices go up, it is not clear what impact the wind and solar centers play in centralized hydrogen production. It is not clear how reduction in gasoline consumption might affect and shift the large hydrogen production centers currently supplying refineries. It could create the need for pipelines and rail and truck transportation to fueling points. The impact of the Low Carbon Fuel Standard and Renewable Fuel Standard on deployment and hydrogen production hubs is also unclear. Technology shifts in how fuel is purchased and the venue for fueling—e.g., home fueling, mobile fueling, and fueling centers placed on non-traditional sites—should be considered.
- The researchers must get more input from stakeholders. Thirty participants is not sufficient. They must get participation from automotive manufacturers, gas suppliers, and energy companies.

Project strengths:

- The project provides a consistent approach to model and validate complex cause and effect relationships. It also provides motivation for a collaborative effort by all key stakeholders.
- The project proposes a thorough investigation of the jobs and economic impacts of hydrogen and fuel cell investment and deployment.
- Project strengths are the use of standard models (REMI) and good collaborations (Connecticut Center for Advanced Technology, Inc., and others).
- The project is based on actual station costs associated with California hydrogen infrastructure development.

Project weaknesses:

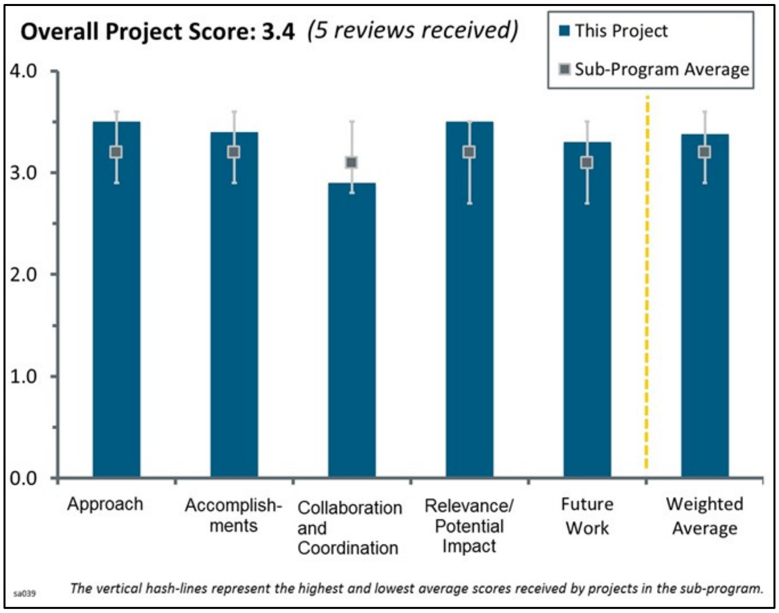
- Though not truly a weakness, as the project has not developed far enough to properly assess this, the development of the alternative scenarios and the breadth, range, and assumptions of these alternative scenarios are crucial to assessing the usefulness of this project and its results. The project has yet to progress to the point at which these alternative scenarios are constructed, but care must be made to ensure that the scenarios capture the range of technology cost and performance progression and public investment in these technologies. Additionally, though the results of the project are still forthcoming, it will be crucial to understand net jobs impacts to the economy, taking into account potential reductions from competing industries.
- Hydrogen and FCEVs are one option of interest for policymakers. Perhaps it would be an improvement to do the same analysis across other advanced vehicle and fuel options, or to at least compare hydrogen and FCEVs to an ongoing comparable analysis being done by others (if it exists).
- The current model does not appear to account for jobs growth in the upstream supply chains to stations and FCEV supply.
- More input and participation from industries most affected by the shift to fuel cells (energy companies) are needed.

Recommendations for additions/deletions to project scope:

- Hydrogen and FCEVs are one option of interest for policymakers. Perhaps it would be an improvement to do the same analysis across other advanced vehicle and fuel options, or at least to compare hydrogen and FCEVs to an ongoing comparable analysis being done by others (if it exists).
- Perhaps the project could focus on those areas/market segments that present the greatest impact on jobs and wages.
- The full role of the advisory group is unclear, but the advisory group should review and approve data, assumptions, and proposed scenarios before the jobs and economic analysis is completed. The project team might also consider involvement of relevant U.S. DRIVE Partnership technical teams as part of its advisory group.
- It may be worthwhile to study the Japanese market because the Japanese government views hydrogen fuel cells and hydrogen infrastructure as a big opportunity for job creation and future economic growth.
- Growing out the FCEV fleet will require significant infrastructure, so it is not clear why jobs in the infrastructure were not considered. This seems to be a major gap. If the project team cannot get participation from automotive manufacturers, gas suppliers, and energy companies, this project should not continue.

Project #SA-039: Life-Cycle Analysis of Water Consumption for Hydrogen Production

Amgad Elgowainy; Argonne National Laboratory



Brief Summary of Project:

The Argonne National Laboratory (ANL) has expanded the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET™) model to include water consumption. ANL has (1) identified major contributors in the upstream supply chain to water consumption and (2) evaluated water consumption for the fuel production stage.

Question 1: Approach to performing the work

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

- The expansion of the well-regarded GREET life-cycle analysis model to include water consumption is an excellent approach to understanding water consumption associated with hydrogen fuels in comparison to other transportation fuels. The project team shows a very good understanding of water consumption aspects over the full fuel production and delivery chain. The project team appears to use a consistent approach to assess the various transportation fuels and develop water consumption factors for these fuels.
- This project continues in a no-stone-left-unturned approach to investigate all water sources and to strive to update input data continually. It is very impressive to see a complete life-cycle analysis of water.
- The latest work provides refinement and greater resolution to previous analysis of water consumption of fossil and renewable fuel pathways.
- The work was comprehensive and enabled detailed comparisons of water consumption for existing energy production approaches and emerging hydrogen production and utilization approaches. The analysis also included some regional data, which is important for providing context for the results. In fact, future work should include even more analysis based on regional data. For example, electrolysis appears to be the largest consumer of water of all hydrogen production approaches, whereas steam methane reforming (SMR) appears to be fairly competitive with respect to water consumption. The project should investigate how this impacts decisions about hydrogen production approaches in water-stressed versus water-rich regions. It is also not clear that the previous year's comment on the fate of evaporated water (either during production and use) was addressed in the analysis work.

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 team has continued to examine and improve its modeling of life-cycle water consumption associated with transportation fuels and to incorporate this modeling into the GREET model. The team's further analysis of water consumption associated with cooling water and allocation of water use for hydro-electric facilities furthers understanding of these processes, and revisions of hydrogen fuel pathways and investigations into liquid petroleum products help enable useful comparisons across fuel types. The project team's analysis of water consumption on a per-mile basis across fuel types provides useful insights, though it is unclear whether per-mile water consumption has been incorporated into GREET (which generally has examined different fuel pathways on an energy content basis, not a per-mile driven basis).

- The work provided by Jacobs® Consultancy provided a good update to the water consumption factors for products produced at a petroleum refinery. The updated information on cooling water consumption for thermoelectric power generation is valuable in understanding regional differences in waste consumption associated with hydrogen production. The work in understanding the viability of water consumption for hydropower is also an important factor in the analysis.
- This analysis work is critical for the Department of Energy (DOE) to make informed decisions on which hydrogen production approaches should be favored in the near term. With California entrenched as the earliest adopter for fuel cells and fuel cell electric vehicles, water usage during hydrogen production needs to be carefully considered. Concern about water availability is also likely to increase in the United States and the world in the future. Thus, this analysis is essential for DOE's decision making.
- It is very good to see a complete analysis of refinery processes and the conclusion that water consumption tracks with energy consumption. It is all very well saying that saline, brackish water, or treated wastewater can replace fresh water for cooling, but these all have corrosion issues that must be addressed (i.e., cost) before they can be fully utilized. It is very good to talk about the variability in water allocations from dams, but the national average may not be relevant, as allocation is something that is going to need to be done on a regional basis. There are great results from removing the wastewater treatment process out of SMR and electrolysis and showing that water consumption is dramatically reduced when using renewable electrolysis.
- Transparency of the work is lacking in parts where consultancies have been used (Jacobs Consultancy, etc.). It would be better to use completely open models.

Question 3: Collaboration and coordination with other institutions

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

- The external collaboration with Jacobs Consultancy was valuable in establishing the water consumption of petroleum products produced within a refinery.
- There is very good collaborations with industry consultants and government entities. This is getting good enough that the team may now need to be getting more detailed information about specific regions.
- The project team appears mainly to consist of the GREET team with additional help on refinery operations from Jacobs Consulting. However, the team has had appropriate interactions with government and industry researchers to understand water consumption associated with wastewater treatment plants, refineries, SMR plants, etc. Given the depth of data on water consumptions for various fuel pathways being incorporated into the GREET model, deeper peer review of the data and assumptions may be warranted.
- Given the nature of this project, the level of collaboration was reasonable. There would certainly be value in collaboration with European and Japanese institutions, although the modest budget for this project would make that somewhat difficult.
- Most of the "collaboration" is just reaching out to get data. It would be better to have stakeholders actively involved in the project. In particular, industrial stakeholders should 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 **3.5** for its relevance/potential impact.

- The project addresses barriers A, C, and D in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. This is outstanding; one cannot do any of this without considering water.
- There is continued interest in transportation sustainability. To further understand the environmental aspects of sustainability, continued life-cycle modeling of vehicle and fuel pathways is needed. This project helps expand life-cycle modeling to investigate water consumption associated with fuels, which will be critical in the future.

- The project addresses the importance of providing a solid understanding of water consumption for various hydrogen supply pathways at an early stage of hydrogen market development. Hydrogen production pathways that are grid- and energy-intensive should be avoided.
- Water use is highly relevant and is part of a broader view into sustainability.
- It is difficult to quantify the impact of this project, but it is essential for DOE to have this kind of information to support its program planning.

Question 5: Proposed future work

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

- It is important to understand the energy and cost penalty of alternative water production process like desalination.
- Generally, the future work is logical. It is important that the team arrive at a meaningful and consistent metric for water consumption.
- The project team should consider where the system boundary is and begin to think regionally while adding missing elements and updating data inputs and models.
- Proposed future activities are appropriate. Continued refinement of fuel pathways already incorporated in GREET and expansion of water consumption to other alternative fuel pathways will be needed to allow comparisons across pathways. Expansion of this type of modeling to the vehicle cycle in GREET2 is needed to provide a complete understanding of vehicle-fuel pathways. In particular, expanding the modeling to account for regional variability will be critically important to evaluating transportation fuel sustainability.
- The stated goals of future work are fine. It would be good to see a more regional flavor added to the analysis and an approach to assess relative impacts of the types of water consumption (i.e., true consumption versus evaporation back to the atmosphere).

Project strengths:

- This work further enhances the GREET model with water consumption data.
- Incorporation of water consumption into the GREET life-cycle model will aid in understanding transportation sustainability and enable comparisons across fuel pathways.
- Life-cycle analysis of water in energy production is a unique and very much needed project.
- A considerable amount of analysis has been performed with a relative modest investment. Comparison with baseline technology is important.

Project weaknesses:

- The project relies on a great deal of data and analyses of water consumption across regions and sectors. It is important that the data and GREET modeling of water consumption be peer reviewed, especially since comparability across fuel pathways is important for future sustainability analyses. Also, water consumption in particular needs to be understood on a regional basis.
- There is not enough analysis on the fate of evaporated water during production and utilization. There is not enough regional consideration.
- The project does not have a clear end date for completion.
- There is probably too much to do, considering the resources available.

Recommendations for additions/deletions to project scope:

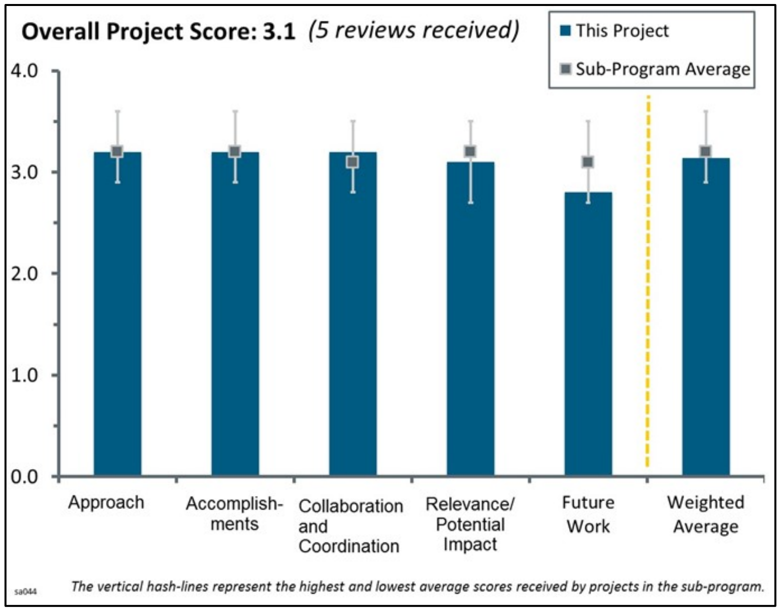
- Once GREET modeling is completed (GREET1 and GREET2) for average water consumption, further work should be conducted to understand regional variability.
- The project should consider adding scope to allow information sharing and collaboration with European and Japanese scientists (this may require an increase in budget).

Project #SA-044: Impact of Fuel Cell and Hydrogen Storage Improvements on Fuel Cell Electric Vehicles

Aymeric Rousseau; Argonne National Laboratory

Brief Summary of Project:

This project aims to quantify the impact of fuel cell system improvements on energy consumption and economic viability of fuel cell electric vehicles (FCEVs). The project will (1) analyze fuel cell stack, hydrogen storage, and fuel cell system improvements in terms of their impacts on the cost of driving FCEVs and (2) evaluate whether current fuel cell and storage technology targets are sufficient to make FCEVs viable.



Question 1: Approach to performing the work

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

- The study used the well-developed existing Autonomie model. Principal focus areas were fuel cell and vehicle hydrogen storage, which represent the largest improvement areas for FCEV performance improvement and cost reduction. The scope of work evaluated the design of fuel cell hybrid vehicles for mid-size class only, but it would be intriguing to understand relative performance of fuel cell hybrid vehicles in larger and smaller vehicle classes as illustrated on slide 5. The simulation results of this study support the long-term viability of fuel cell hybrid vehicles compared to conventional internal combustion engines at large-scale manufacturing of 500,000 fuel cell stacks. The large manufacturing volume represents scale in the distant future. Business planning environments are often short-term, so industry stakeholders also want to know the prospects for FCEVs for the next 10 years.
- The project uses the well-regarded and industry-vetted Autonomie model to assess vehicle cost based on various assumptions for fuel cell system efficiency and hydrogen storage system cost and gravimetric capacity.
- Autonomie is a good analytic tool for addressing the vehicle cost and performance questions. The market competitiveness question (last bullet on slide 3) is only partly addressed by this study.
- The approach is appropriate given that the project is based on Autonomie and U.S. Department of Energy (DOE) targets, which usually assume high volumes of production. However, it would be valuable to also assess cost at lower production volumes or at least compare low-volume to high-volume production to understand gaps. For the out years, the model could assume that production volumes increase over time. Adding that capability to Autonomie would be valuable. It is understandable that, in this case, the researchers wanted to understand the influence of improvements in the fuel cell stack and the hydrogen storage system on the overall levelized cost of driving (LCOD). However, the assumption of high volume is not realistic for the near term. The results slide (slide 19) shows a horizontal line for 2015 conventional vehicle costs. This line could be misinterpreted given that it extends all the way back to 2000 and forward to 2045. Perhaps this is just a matter of better labeling. The use of the word “accelerated” to represent the future cases is misleading, particularly when a National Renewable Energy Laboratory project (Evaluation of Technology Status Compared to Program Targets by Marc Melaina, et al.) uses the word “accelerated” for the cases where 2045 targets will be met in either 2035 or 2025. It is good to see that uncertainties were used to perform the analysis. However, it would be useful to know the uncertainty bands around the 3.5% result, although this is probably not a big deal given that the uncertainty band seems tiny. The uncertainty analysis would be more valuable in the LCOD results.

- The target-based approach is lacking. This project should use actual expected progress, rather than DOE targets. It might be good to leverage a third party consultant to provide this view if national laboratories are unable to project anything but DOE targets.

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 done an excellent job evaluating FCEV configuration, cost, and performance based on various assumptions on the improvement in fuel cell system efficiency and hydrogen storage cost and performance.
- Progress toward completing the project is excellent; the project will be completed in less than a year thanks to the support of an excellent team of Argonne National Laboratory (ANL) researchers. The team did exactly what it was supposed to do, and the progress is good.
- The work provided validation that mid-sized fuel cell hybrid vehicles can realize a lower cost of driving over conventional vehicles in the long term, and performance improvements made in conventional vehicles can cost-effectively be applied to FCEVs. The scope of work only encompassed mid-sized vehicles, which is valuable to understand, but not ideal for understanding the broader market implications of fuel cells hybrid vehicles.
- Results are straightforward, but some additional sensitivity analyses would be interesting. Some discussion or analysis of the “other” relevant technology improvements, such as mass reduction, would also be interesting. This seems to be baked into the results on slide 19. It is hoped that if there are additional factors rolled into that result, they can be unbundled in the final report.
- This project seems very close to the similar study that was conducted in 2015, with a few tweaks on data inputs. It is puzzling why updating a study would require such a large budget.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration is generally good. The team is working with the U.S. DRIVE Partnership and others in industry.
- Coordination is sufficient for a project of this size.
- The project team mainly consists of the Autonomie group and other researchers in ANL, though the team does team appears to have very good collaboration with and receives important inputs from U.S. DRIVE as well as DOE. The project team also notes collaboration with researchers conducting life-cycle cost, energy use, and emissions analyses, though it is not clear how formalized this interaction is or how Autonomie results are provided to other researchers.
- The Autonomie team engaged the correct stakeholders to acquire data and is collaborating with the appropriate DOE and national laboratories staff to provide results. The final results should be vetted by either the U.S. DRIVE Partnership’s Cradle-to-Grave analysis team or the Fuel Pathway Integration Technical Team.
- The project covered analysis only, with no direct collaboration with the fuel cell and storage technology team. It is unclear whether the project lessons learned and results are being shared with these other DOE organizations, because the analysis shows no improvements in hydrogen storage after 2015. It is unclear whether the DOE storage team is aligned with the study 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 project's assessment of FCEV cost and performance resulting from fuel cell system efficiency improvements and hydrogen storage system improvements provides a useful understanding of the impacts technology advancements will have. This analysis will aid DOE in its target-setting activities and evaluating its research priorities.
- This is a good and necessary basis for understanding future market competitiveness. Understanding contributions to strong market growth would require an expanded scope.
- The work confirms the challenges of achieving a big breakthrough in hydrogen storage but does not specifically address fuel cell durability.
- It is very interesting to understand that the potential improvement of vehicle power requirements from improvements to the fuel cell stack and the hydrogen storage system is only about 3.5%, but translated into dollars, that improvement can reduce overall fuel cell system cost by 50%. Of course, that is only valid if both systems are produced at scale, which is a big assumption that may not necessarily reflect future reality.
- This work does not need to be done on an annual basis, especially when the projected long-term changes are relatively small.

Question 5: Proposed future work

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

- The plan to conduct sensitivity analysis on market penetration is valuable for understanding short-term and mid-term cost effectiveness of fuel cell hybrid vehicles.
- The researcher did not address proposed future work during the presentation, presumably because the analysis has already been completed and is only in the review phase. However, he did talk about what he would like to do in the future, which is understand how much technologies should improve to meet targets and whether these improvements are cost-effective.
- The proposed future work was not included in slides but conveyed verbally during presentation: focus on marginal benefits of specific advances. This is acceptable and necessary for a limited set of research and development questions but not sufficient to understand the market potential for FCEVs more fully.
- The presentation did not describe any planned or potential future work activities. A milestone chart did indicate that analysis completion and reporting was expected in fiscal year 2016. It would be useful to better understand what type of reporting is envisioned for this project and how the project team expects to provide results and engage other researchers. The project might benefit from either further research into the impacts of fuel cell system and hydrogen storage system improvements or, more broadly, into other FCEV-related improvements.
- There is no future work. The project is finished.

Project strengths:

- The project's assessment of FCEV configuration and cost resulting from fuel cell system efficiency improvements and hydrogen storage system improvements will aid DOE in setting its research priorities and targets. The analysis is based on the well-regarded Autonomie model. In particular, the Autonomie modeling enables the project team to understand feedback loops that allow smaller fuel cell stack sizes (smaller mass per kilowatt output) and hydrogen storage systems that can reduce FCEV mass and enable lower cost and lower weight FCEV configurations.
- The simulation analysis further validated the long-term cost effectiveness of fuel cell hybrid vehicles and provided a better understanding of the impact of the projected development in fuel cell efficiency and hydrogen storage.
- Project strengths are the strong technical knowledge, strong team, experience with the model, robust model, and defensible data.

- The rigorous analytic approach is a project strength.

Project weaknesses:

- Considering the number of assumptions in the scenario analysis, it would have been valuable to provide more transparency on the key factors contributing to fuel cell and storage cost reductions over time. Also, transparency on the "other" technology contribution to base spark ignition vehicle performance and applicability of spark injection improvement to the FCEVs would be valuable.
- As part of its investigation, the project team conducted preliminary FCEV life-cycle cost evaluations. The Fuel Cell Technologies Office (FCTO) and U.S. DRIVE already are conducting various efforts to assess life-cycle cost and cost of driving. It would be more useful for this project team to work with other DOE and U.S. DRIVE researchers on this area so that multiple evaluations using multiple analysis frameworks can be avoided.
- The project lacks sensitivity analyses and only partially addresses the market competitiveness question.
- The assumed high production levels are a project weakness.

Recommendations for additions/deletions to project scope:

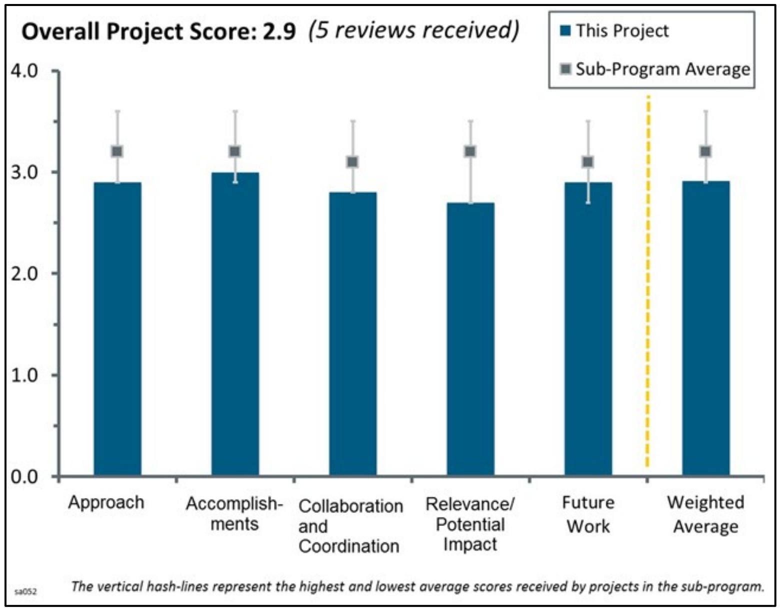
- The project conducts very valuable evaluations into FCEV configuration and cost based on important system parameters such as fuel cell system efficiency. It would be helpful for the project team to engage other FCTO and U.S. DRIVE researchers more formally to provide the results of these analyses to the community at large, to increase collaboration, and to avoid duplication of analysis efforts.
- A broader analysis that covers all vehicle classes and at lower fuel cell production volumes is recommended.
- The same analysis should be performed for business cases with lower volumes of production. The project should show uncertainty bands around LCOD results.

Project #SA-052: The Business Case for Hydrogen-Powered Passenger Cars: Competition and Solving the Infrastructure Puzzle

Robert Rosner; University of Chicago

Brief Summary of Project:

The goal of this project is to develop a plausible business case for marketing hydrogen-powered fuel cell passenger vehicles and building out the hydrogen fueling station network. Researchers will analyze the competitive posture of hydrogen-powered fuel cell passenger vehicles in the marketplace and study the business case for a plausible hydrogen fuel distribution scheme.



Question 1: Approach to performing the work

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

- This is a great project that analyzes scenarios in which the hydrogen infrastructure is mature enough to be profitable to investors without any government subsides. The work uses and fits in very well with other U.S. Department of Energy (DOE) models.
- Although this project just recently started, from the material presented it seems the project leads are taking a good approach to this work. This was demonstrated by the proposed approach of utilizing two robust models, such as the Hydrogen Delivery Scenario Analysis Model (HDSAM) and Hydrogen Financial Analysis Scenario Tool (H2FAST) and collaborating with their main leads from Argonne National Laboratory (ANL) and the National Renewable Energy Laboratory (NREL). In addition, project leads stated they will also be interfacing with established hydrogen station equipment suppliers to gather additional data.
- The approach is good, but there is not a strong need for this report. The total cost of ownership comparisons have error bars that appear too small, particularly for future cases where technology breakthroughs (e.g., lower battery cost) could have a big impact on the competitiveness of the technology. The slides mention that "an optimal hydrogen production technology is not yet established." About 9 million metric tons of hydrogen are produced in the United States annually, mostly through steam methane reforming. DOE's reports confirm that natural gas reforming is an established industrial process that meets cost targets.
- This analysis hinges on which vehicles consumers will choose assuming different total cost of ownership. The vehicles chosen do not compete in the same market. The critical parameters for consumer purchase decisions are first vehicle size and body type (hatchback, sedan, sport utility vehicle [SUV], etc.). Once those are defined, then a consumer can do a cost-benefit analysis. The analysis needs to compare vehicles that represent the same size and body type. A better approach is a component-based buildup of vehicle costs, such as the one done by the Autonomie report or referenced in the cradle-to-grave report (both ANL publications).
- There are a number of questions about the approach: (1) Did the project have a break in 2015-2016?; (2) was venture capitalist (VC) input included in the study?; (3) what input database was used and how were the different modeling tools used? Also, the "101th of-a-kind" station is still a transition game, not full scale.

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.

- Even though the project started only a couple months ago, some results and conclusions have already been reported, and these results are on track with the main goals of this project.
- The project is on track to be completed in a timely manner. The early conclusions appear realistic but not surprising.
- The project is making a great contribution by being able to predict the cost of ownership of a hydrogen fuel cell electric vehicle (FCEV). There is some risk in learning from the current FCEV implementation in California, because the circumstances there may not be applicable to other markets, and it is still such a small market that it may be a terrible test case for market validation. It also seems a little weird that the implication is that the 101st station is a standalone hydrogen station. The team should consider the costs and benefits of adding hydrogen to the portfolio of fuels at a regular filling station. Presumably there will be significant capital savings based on that scenario.
- It is not clear whether the project discussed the data with the VC community. It is not clear why the team did not contact Toyota and Hyundai directly. There is not adequate data to conduct a fair review. It is not clear how the team calculated the number of cars on the road and who is driving the vehicles. It is unclear what the owners' experience would be. It is unclear whether the Tesla Motors experience in cost reduction is relevant. The Toyota Mirai vehicle has fewer parts and much less maintenance cost. It is unclear what the main barrier is to the 101st station. The hydrogen price reduction strategy needs to be analyzed.
- It is too early in the sub-project to understand what was done. It appears to be mostly planning at this point.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- There is very good collaboration with both national laboratories. The project may also benefit by collaborating with industry stakeholders as well as with stakeholders in California where a lot of hydrogen and FCEVs efforts are underway. These could possibly include the California Fuel Cell Partnership, California Air Resources Board, and the California Energy Commission.
- ANL and NREL have been approached to provide feedback on the approach. Both institutions have highly knowledgeable staff familiar with the topic. Collaboration with industry and the VC community is needed to ensure that the assessment is material, the right assumptions have been made, and that the appropriate results are calculated.
- There are no hydrogen gas suppliers or vehicle manufacturers amongst the collaborators, yet the analysis uses hydrogen.
- The study needs much more input from original equipment manufacturers and suppliers of hydrogen fueling station hardware.
- The project needs to contact car companies directly, and the owners' experience needs to be analyzed. Input from the VC community is also 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 **2.7** for its relevance/potential impact.

- The project addresses the relevant barriers of the Hydrogen and Fuel Cells Program Multi-Year Research, Development, and Demonstration Plan.
- This project will definitely inform the VC community about hydrogen and FCEV technology and whether there might be opportunities to invest in it.
- The study needs to go beyond ANL data and make more practical assessments. The study could investigate a battery-dominant FCEV as part of a transition strategy. The PI's comment on Tesla and fuel cells should

be put in terms of the opportunity for FCEVs to extend the range of Tesla vehicles. Performance is much better in battery-dominant FCEVs.

- A number of studies assessing future cost of hydrogen refueling stations have previously been conducted. A summary of such studies is in NREL's 2013 study "Hydrogen Station Cost Estimates Comparing Hydrogen Station Cost Calculator Results with other Recent Estimates" (http://www.nrel.gov/docs/fy13osti/56412.pdf). In addition, the H2FAST model is capable of generating cost data for potential VCs.
- This area of work does not seem to have clear relevance to DOE goals. A business case is something that should be investigated by the stakeholders who would develop the infrastructure, not DOE.

Question 5: Proposed future work

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

- Two aspects of the proposed future work are key: the vetting of input cost data and the market analysis of the rollout of first generation hydrogen stations.
- The proposed future work is adequate, but the materiality is lacking.
- Future work should include addressing the issue of the vehicle comparison.
- The project has a good approach to enter probability to the output rather than simple point output, but the quality of the input data is still in doubt.
- The project should focus on value proposition enhancement as related to the battery FCEVs. This study should consider how to enhance the value proposition, not just conduct analysis.

Project strengths:

- This project is another good attempt to predict future behavior of this exciting emerging market and begin the process of involving venture capitalists.
- Project strengths are the awareness of available tools and resources and access to experts from national laboratories.

Project weaknesses:

- It is still not clear whether the inevitable uncertainties are being correctly handled. It is not clear what the absolute worst- and best-case scenarios are and their true probabilities.
- The subsidy removal question is not practical. Sales of 60,000 hybrid cars made them successful. Analysis is needed for the first 1,000 and first 10,000 vehicles for the 101^{st} station.
- The relevance of the report and the lack of engagement with industry and the VC community are project weaknesses.

Recommendations for additions/deletions to project scope:

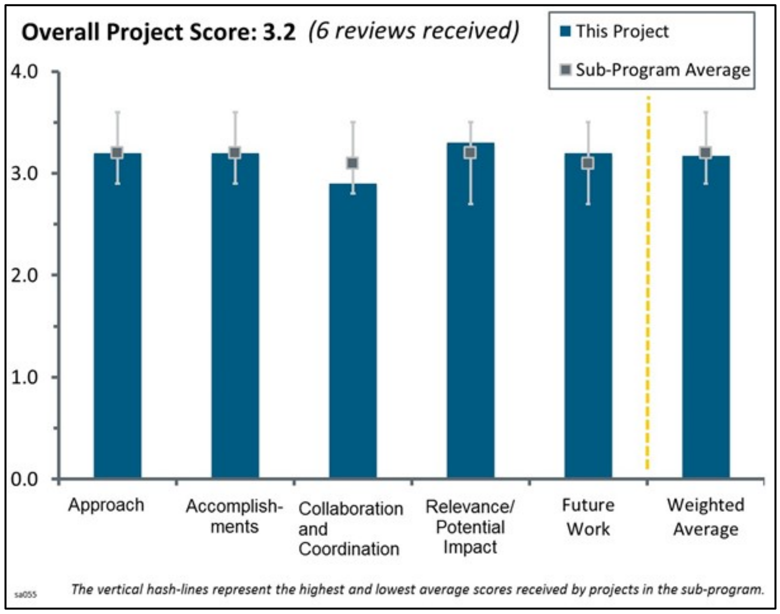
- Collaboration with industry and the VC community is needed to ensure that the assessment is material, the right assumptions have been made, and the appropriate results are calculated.
- The project should obtain more input from potential investors and stakeholders.
- VC funding for the FCEV and hydrogen market is critical. It is unclear how the study data will help the transition strategy.

Project #SA-055: Hydrogen Analysis with the Sandia ParaChoice Model

Rebecca Levinson; Sandia National Laboratories

Brief Summary of Project:

The objective of this project is to understand changes to light-duty vehicle stock, fuel use, and emissions through analysis of the dynamic among vehicles, fuels, and infrastructure. ParaChoice parametric analysis will (1) identify trade spaces, tipping points, and sensitivities and (2) help researchers understand and mitigate uncertainty in data sources and assumptions.



Question 1: Approach to performing the work

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

- This is a great project that attempts to examine scenarios in 2050 and scale price with demand. The way hydrogen production is put into the model is really very good. Comparing drive trains, both established and emerging, is outstanding. The model is deliberately designed to explore uncertainties and tipping points; as it progresses, it will be exciting to see how accurate it is.
- The approach of using other previously developed models as input to the ParaChoice model is commendable. This addresses the barrier of "Inconsistent Data, Assumptions, and Guidelines." This provides credibility to the model. The project should ensure that the assumptions in these previously developed models are consistent with the assumptions throughout the ParaChoice model. The ParaChoice model may be limited by assuming that customer vehicle selection is based solely on cost when studies have shown that customers are not "economically rational."
- The ParaChoice model appears to apply a Monte Carlo simulation to the base U.S. Department of Energy (DOE) models and data parameters. The work explores the impacts of uncertainty around fuel cell electric vehicle (FCEV) cost and performance and hydrogen infrastructure that other DOE models and analysis do not currently address.
- The project has a nice combination of different aspects of other models. The FCEV penalties on slide 10 for battery electric vehicles (BEVs) and FCEVs need to be explained in greater detail for the reviewers to understand the unique contribution of ParaChoice compared to other models, or even to determine whether ParaChoice has correctly implemented approaches used in other models. Slide 6 states that the model has dynamic feedback with infrastructure resolved endogenously, but this is difficult to do, and there is little evidence in the slides (or in the presentation delivery) that there is a significant analytic contribution in characterizing this feedback. That being said, there is significant progress in the integrated economics of supply and market growth compared to last year's updates. The parameterization approach is valuable in general, but it is not necessarily a unique capability compared to other similar models, just a unique presentation of results.
- The model currently includes current and near-future policies. It would be good to start implementing potential future policies to assess their potential impact. The current process assumes that hydrogen production will be driven by vehicle demand. It would be nice to implement a methodology to include fuel availability as a parameter influencing vehicle sales. It would be nice to show one example of how Monte Carlo Latin hypercube sampling is used from generating inputs to market penetration analysis; the current presentation included only a "brute force" parametric study.
- The team said it used input from the Hydrogen Analysis (H2A) model and Hydrogen Delivery Scenario Analysis Model as price data. These tools do not calculate price; they calculate cost. In addition, H2A was

never intended to be used to project price. It was intended to allow for an apples-to-apples comparison of different technologies and/or the progress made on a single technology. If the team is using H2A for hydrogen price projections, it is misusing the model. The principal investigator stated that the team used price and cost interchangeably (see slides 5–8). However, price and cost are not the same. Cost is what it takes to manufacture. Price is what a product is sold for. The team needs to be very clear on its terminology. The assumption that federal and state incentives will not change between now and 2050 does not seem reasonable. To this reviewer's knowledge, most businesses do not include incentives if trying to project more than five years out since there are too many unknowns. In addition, the technologies need to be able to succeed without the incentives and policies (carbon tax). Also, it seems that if the difference between industrial hydrogen and central steam methane reforming (SMR) is the delivery cost (which is included in industrial hydrogen), then central SMR, central electrolysis, and central coal and sequestration would also have delivery costs. The project needs to clarify this issue. The assumptions on how the multipliers (slide 14) are developed need to be made transparent. Without understanding how they were developed, their accuracy cannot be evaluated.

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 based on the funding the project was provided. It was great to see that the project has made efforts to validate the model based on history and performed a sensitivity analysis with the model.
- The work was able to parameterize base input assumptions to existing DOE models and apply market segmentation and market assumptions to establish market penetration rates of FCEV adoption and emissions reduction. Accomplishments or conclusions derived from the ParaChoice analysis confirm the importance of initial FCEV cost and efficiency along with hydrogen costs and emissions.
- The business-as-usual projection was possibly the most important result. It was in the analysis talks, and it will be incredibly useful moving forward. Also, the result that SMR needed to be displaced for hydrogen-fueled vehicles to make a real impact was important.
- The presenter showed which of the seven hydrogen fuel pathways was selected and how the selection evolved over time. It would have been nice to show the impact of assumption uncertainties on the fuel pathway selection, if any.
- The result that "FCEVs displace compressed natural gas vehicles disproportionately to other alternative fuel vehicles" is interesting; it is not clear why this happens. The need to move towards renewable hydrogen production is supported by the finding that the "Prevalence of distributed SMR hydrogen makes FCEVs a GHG neutral addition to the stock." The other findings are not very surprising. For example, if the FCEV cost is lowered, the finding that you will sell more FCEVs should not be a surprise. Likewise, it is not surprising that at lower hydrogen costs more FCEVs are sold. The need to go to cleaner fuels is also not a surprise, as this has been shown in other analyses.
- There are very interesting results. There are some concerns. First, slide 10 has the sales fraction in 2050 being very unresponsive to fuel cost, even near zero. This should be revisited. It could be that the vehicle is so efficient that fuel costs are very small, but either way, the team needs to unbundle this issue more to make that claim convincing. Also, FCEV sales fraction as a function of efficiency seems to contradict results on slide 15. It is hard to see that fuel economy improvements would improve the sales fraction but the cost of fuel does not. Fuel economy is only valuable to the consumer because it reduces fuel costs. This seems like an error unless there are some other correlations that are not being revealed, in which case the presentation is misleading.

Question 3: Collaboration and coordination with other institutions

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

- The project exercised existing analysis models and included frequent collaboration of the ParaChoice modeling team and the DOE model stakeholders from other laboratories. The work received technical feedback from two automotive manufacturers.
- There is good leveraging with the Vehicle Technologies Office (VTO). Collaborations with other stakeholders are unclear. It seems the team talked with one original equipment manufacturer (OEM), General Electric, and the American Gas Association at conferences ("and other conference engagements," slide 18). It is not clear how the energy producers were involved. From the slide, it seems the technical critiques were only from conference engagements. This is not a sufficient review by stakeholders to give confidence in the approach and results.
- It is not clear what benefits have been gleaned from the collaboration that has been claimed. It may be beneficial to highlight what assumptions have been changed or model improvements that have been made based on this collaboration. It is good to see that this work is funded by the VTO and the Fuel Cell Technologies Office (FCTO). This allows cross-pollination between the offices, resulting in a better product.
- It is not expected that funding will be given to other entities, but this work needs to be shared and commented on by a much bigger range of interested parties, multiple OEMs, government, and other agencies.
- The presenter should include collaborations with other agencies that perform similar work. Collaborations with other national laboratories are mentioned, but it is not clear what they are.
- Stronger collaboration is needed. It is not clear what "conference engagements" are. The project should be more explicit about its relationship to these entities and the type and quality of feedback received.

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 model provides insight into the relationship between FCEV cost and efficiency and the resultant sales and GHG emission reduction. This information supports the FCTO objectives. Sensitivity analyses on anticipated improvements in hydrogen production pathways and energy efficiency of the FCEV could help guide the research, development, and demonstration priorities of the FCTO.
- This project addresses A, C, and D of the FCTO Multi-Year Research, Development, and Demonstration Plan.
- The analysis provides a projection of a variety of light-duty alternative vehicle platforms that are competing for the same customers.
- This has potential to be important, but there are several weaknesses that need to be fixed.

Question 5: Proposed future work

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

- It is great to see the deeper dive exercises and the attempts to begin to understand hydrogen fueling station growth.
- Conducting a deeper dive into consumer market segmentation and FCEV adoption will be valuable to understand.
- The model needs additional evaluation of the hydrogen refueling station growth scenarios, which could be very different from one state to another. Regulations and possible market drivers could be different in the different regions of the country. Addressing market niches will assist in understanding the non-cost-related issues in customers' purchase decisions (urban vs. suburban, vehicle class, etc.). The team might consider evaluating people's acceptance of new early adopters vs. late majority distribution.

- The work is fine, but the milestones are not specific, measurable, achievable, relevant, and timely (SMART). A SMART milestone is recommended.
- It is not clear how to do a deep dive into niche markets if the model treats all urban areas equivalently. The project has to take into account heterogeneity of urban markets, and it is unclear whether this is happening. Parametric analysis of station growth is not what is needed. First, a clear and meaningful articulation of the feedback mechanisms is needed.

Project strengths:

- The analysis was dependent on existing DOE models from other laboratories, and there was good collaboration between the Argonne National Laboratory project team and the other laboratories.
- Leveraging a Monte Carlo analysis to assess the uncertainties is a strength.
- Projections are needed to understand where the FCTO should focus its limited resources.
- The project really tries to understand uncertainties.

Project weaknesses:

- There could have been better transparency on the range of values assigned to the key variable in the model that were parameterized.
- There needs to be more input from outside entities.
- Lack of collaboration/coordination with other institutions developing similar tools is a weakness.

Recommendations for additions/deletions to project scope:

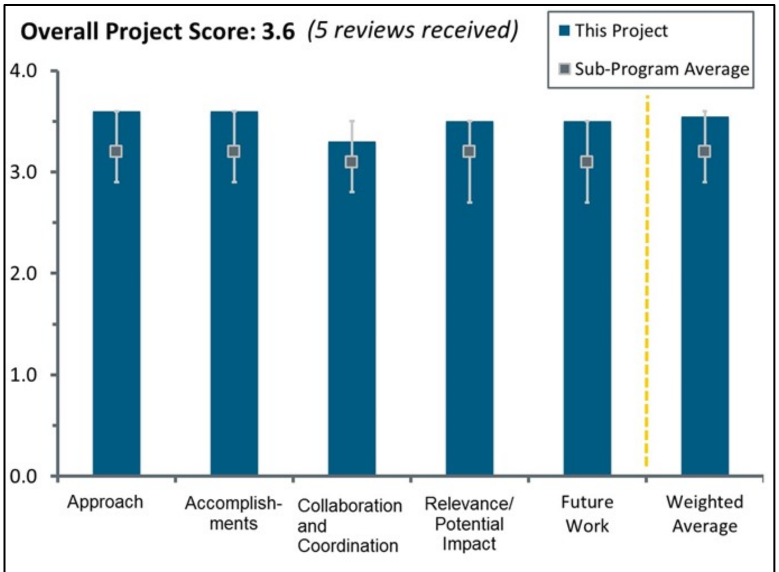
- It may be beneficial to allow the model to adjust the number of years that it forecasts out for switching between distributed and centralized production based on the past year’s market growth. This could be another possible sensitivity analysis.
- The hydrogen supply pathway should be expanded to include biomethane supply to the steam methane reformer.
- The project team needs to be consistent with the FCTO on “cost” vs. “price.” A SMART milestone needs to be added. So far, the results are not surprising. They are very consistent with results that the Systems Analysis program has done in the past. The team should add error bars on its findings.
- It appears that several national laboratories are developing similar models in parallel with little to no coordination. This appears to lead to duplication of efforts.

Project #SA-057: Life-Cycle Analysis of Emerging Hydrogen Production Technologies

Amgad Elgowainy; Argonne National Laboratory

Brief Summary of Project:

This project is expanding the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET™) model to include life-cycle analysis (LCA) of emerging hydrogen production pathways. Investigators will acquire material and energy balance information for emerging hydrogen production technologies from modeling efforts developed by partner laboratories, use GREET to conduct well-to-wheels (WTW) analysis of new pathways, and compare WTW greenhouse gas (GHG) emissions of new hydrogen production pathways with baseline pathways.



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.

- A considerable amount of analysis was completed within a short time and with minimal investment. This was enabled by leveraging existing tools that already have been established. At this early stage of the project, it is difficult to find any faults with the technical approach.
- This project expands the number of hydrogen production pathways to include emerging production technologies based on fermentation, high-temperature solid oxide electrolyzer cells (SOECs), and bio-derived liquids. Developing new pathways as part of the GREET model is an excellent approach because it builds on the existing GREET model framework, which is the standard for LCA of transportation fuels. The project team is well versed in GREET modeling and has worked with the appropriate researchers from consulting firms and national laboratories to understand these emerging hydrogen production pathways.
- The project expanded the current GREET model to address three new hydrogen production technologies. This project appears to have addressed the three barriers that were claimed in the presentation. Modification of a thoroughly vetted model for this analysis is much better than development of a new model. The three technologies are at a relatively low technology readiness level (TRL) and, as a result, are subject to significant changes and improvements. Assumptions for these models should be well documented and then updated as changes occur.
- This is a great project to see attempts made to try to understand a full LCA of emerging hydrogen production technologies. However, the input to the model is data from one developer of each of the approaches; where possible, an effort should be made to include as much data from multiple developers as possible to increase the usefulness of the model. The project team needs to think very carefully about where the system boundaries are drawn; WTW here seems to mean feed-to-process-to-wheels. Many of these feeds have significant carbon dioxide and energy footprints before this model inputs them. Some of this information is known, e.g., for corn stover or the wood that is fed into the pyrolysis oil. The totals for producing and processing the biomass into fuel feedstock should be included in the complete analysis.
- Work included fundamental heat and material balance to support LCA of the three new emerging hydrogen supply pathways.

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 team has successfully developed new GREET pathways for dark fermentation, high-temperature SOECs, and hydrogen from bio-derived liquids (pyrolysis oils) by incorporating production process data and information on feedstocks and process energy. This work enables WTW comparisons to be made of these processes (including process variations) and other hydrogen fuel pathways, such as natural gas steam methane reforming and water electrolysis (wind- and grid-based).
- The project has made great progress starting this work and getting early numbers for how these technologies might pan out. It is great to see the new technologies already being compared to the more established ones.
- This kind of analysis is essential for DOE to make informed decisions on investments.
- The results of the analysis should be provided to those that are developing these technologies and could be used to help direct future research, development, and deployment. Cost, air toxic emissions, and land and water usage should be included in the analysis and, if possible, presented together to show a more complete picture of each alternative and help direct future work. It is not clear whether heat is required for biomass-derived liquid reforming and how much and the source (internal vs. natural gas addition). The other hydrogen paths are also not clear. It was not clear from the presentation whether the hydrogen transportation cost was reduced for the distributed hydrogen generation technologies relative to those that were centralized.
- An LCA was completed for three new emerging hydrogen supply pathways, and GHG emissions were estimated for different power grids.

Question 3: Collaboration and coordination with other institutions

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

- This project was conducted by members of Argonne National Laboratory's (ANL's) experienced GREET team, with appropriate input from national laboratory and consulting firm researchers on the various hydrogen production processes.
- The project could not have been conducted without critical input and collaboration with the National Renewable Energy Laboratory (NREL), Pacific Northwest National Laboratory (PNNL), and Strategic Analysis, Inc.
- The level of collaboration is commensurate with this type of work and the project budget. It would be useful to establish dialogues and collaborations with European and Japanese scientists in this field, although this could be problematic without an increased budget.
- It is good to see involvement from others in the development of the hydrogen production pathways. It is not clear whether the results of the project were fed back to the collaborators to allow them to modify their approach or at least be aware of the issues that should be addressed going forward.
- It is a good start to find one research group at an individual laboratory to get initial data. This project needs input from all developers of these technologies. It will be especially important to be watching for breakthroughs and technology improvements as the work progresses. Also, the project needs collaboration with feed producers.

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 addresses barriers C, D, and E in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. The project is needed to make proper and useful assessments of emerging technologies.

- The project directly addresses the LCA of emerging hydrogen pathways that is important to understand at the very early stage of technology development. The intention of this work is to identify future hydrogen supply technologies that hold promise, first environmentally and then economically.
- There is continued interest in transportation sustainability in general and, in particular, in hydrogen fuel cell electric vehicles (FCEVs) on the life-cycle GHG emissions associated with FCEVs and hydrogen fuel pathways. This project helps expand life-cycle modeling of hydrogen fuels to include emerging hydrogen production techniques and renewable feedstocks. Understanding these emerging production pathways, which have the potential to be low-carbon transportation pathways, is critical to advancing FCEVs.
- This LCA to determine reductions in GHG emissions and its comparison to currently available technologies aligns well with the Hydrogen and Fuel Cells Program and its objectives. It is not groundbreaking work, but it demonstrates an expanded ability for the GREET model.
- Understanding and quantifying WTW GHG emissions associated with various hydrogen production technologies is essential for DOE to make informed investment decisions.

Question 5: Proposed future work

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

- Adding photobiological, photoelectrochemical (PEC), solar thermochemical hydrogen (STCH), and others will be extremely useful. It is very good to add water consumption and other pollutants. This needs to be done for all pathways, so it may be ambitious for the current level of funding. It will be essential to update GREET and get peer-reviewed publications based on the analyses.
- The proposed future activities are appropriate. Continuing to develop modeling for new hydrogen fuel pathways (including those based on renewable feedstocks such as photobiological production, PEC, and STCH) will be important for advancing FCEVs and assisting DOE in understanding the potential of the technologies under its research portfolio. In addition to the new work cited, the project team should continue to modify the pathways investigated in this project, as improvements in these emerging pathways are likely as research progresses.
- The proposed future work appears to be appropriate. The proposed inclusion of water consumption in the analysis (perhaps leveraging ANL's parallel project) is especially important. It is also suggested that the project consider adding land use as a metric for each of the hydrogen production processes. Analysis of regional considerations might also be included in future work.
- Evaluating new hydrogen production pathways would be the next step with this project. It would be good to see a firm basis for the selection of the pathways selected. The selection could be based on technologies with the lowest cost, best chance of commercial success, or the widest range of possible alternatives. DOE should be involved with the selection. Probability distribution functions would be a nice addition to the GREET model. This would better represent the results of any analysis.
- The future work does not address cost analysis of the emerging hydrogen supply pathways. It would be helpful to understand how all new hydrogen pathways provide a balance in regard to life-cycle emissions, energy efficiency, and cost.

Project strengths:

- The strengths are the comprehensive approach to assessing GHG emissions for various hydrogen production processes and the effective use of existing tools to support the analysis.
- The expansion of the GREET modeling of hydrogen production pathways, particularly emerging low-carbon pathways, is important to furthering the market for FCEVs.
- The LCA of new hydrogen production pathways is a strength.
- The project built off previous work conducted at PNNL and NREL.

Project weaknesses:

- There are none at this stage.
- The project needs more resources to include everything, i.e., processes outside system boundaries and more input from the developers of these technologies.

- Though not the fault of the project team, the hydrogen production processes investigated represent emerging technologies at a low TRL. As such, understanding of these processes is preliminary, and the resulting GREET analysis and findings are likely to change. Development of uncertainty modeling and probability distributions will help in this regard.
- It is not clear how the three new hydrogen supply pathways were selected for LCA and whether the new pathways were down-selected from a larger list of future hydrogen supply pathways. The comparative WTW analysis of the new pathways to existing steam methane reforming and water electrolysis is incomplete without the listing of biomethane feedstock to steam methane reforming hydrogen supply.

Recommendations for additions/deletions to project scope:

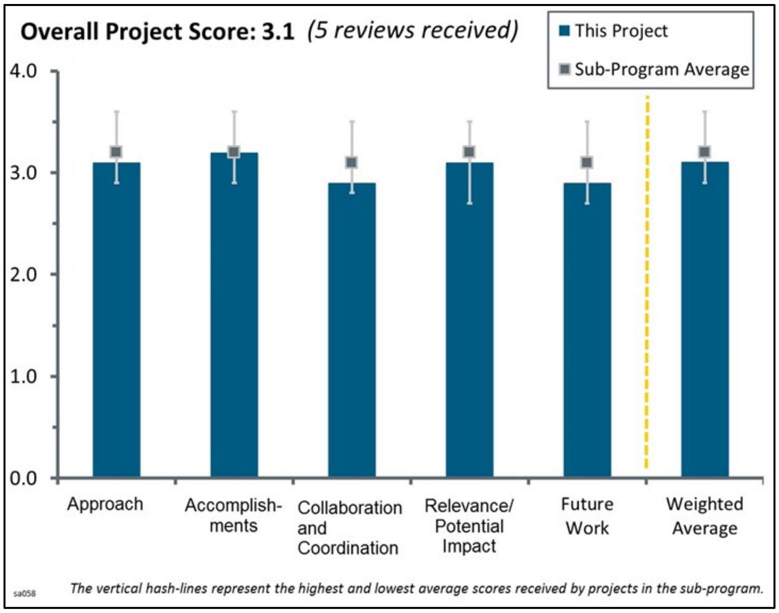
- The LCA should be expanded to include the biomethane supply to steam methane reforming hydrogen supply.
- The project should continue investigations of other emerging pathways (photobiological, PEC, STCH); revision and refinement of the current work on dark fermentation, SOECs, and bio-derived liquids; and the uncertainty analysis, including probability functions.
- The project should consider land use and regional considerations within the analysis. The project should consider expanding the scope to facilitate collaboration with European and Japanese scientists.

Project #SA-058: Analysis of Incentives and Policy Impacts on the Market for Alternative Fuels and Vehicles

David Greene; University of Tennessee

Brief Summary of Project:

This project supports analyses of future markets for hydrogen and fuel cell technologies. Investigators will develop and publish reports that increase understanding of the transition to low-greenhouse-gas (GHG) energy for motor vehicles, the role of public policies in the transition, impacts of policies on the early markets for non-automotive fuel cells, and effectiveness and efficiency of policies for promoting low-GHG energy vehicles and fuels.



Question 1: Approach to performing the work

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

- The collection of these data has been carried out effectively, and the findings are significant.
- Given the small size of the project, the focus and approach are very valuable.
- The project employs a review of relevant literature to understand cost drivers and policy impacts of transitioning to alternative fuel vehicles in general, and hydrogen fuel cell electric vehicles (FCEVs) in particular. This approach is reasonable, but data on FCEVs are limited, requiring greater emphasis on other alternative fuel vehicles. While much can be learned from this literature, recent experiences with hybrid electric vehicles (HEVs) and plug-in/battery electric vehicles (BEVs) are somewhat limited in regard to fueling infrastructure because infrastructure hurdles for these alternative vehicles are much lower compared to hydrogen infrastructure for FCEVs. Investigation of 85% ethanol (E85) and natural gas transportation fueling infrastructure might be informative.
- Barriers are not taken from the Systems Analysis section of the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. The barrier should be “Future Market Behavior.” This is a good literature review of the customers’ behavior in terms of new technology and specifically FCEVs. These results should be used to help direct public policy and the manufacturer’s approach to vehicle development.
- The project is a literature review of policies to promote alternative fuels and vehicles. So far, only U.S. policies have been reviewed, which may not offer a complete view.

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 at uncovering important learnings from the available literature on deploying alternative fuel vehicles generally and on economic and non-financial incentives for vehicle uptake. The lessons learned provide good information for the deployment of FCEVs and will help federal and state governments better understand how to craft policies to incentivize FCEV deployment. The project discussed findings regarding the extent of hydrogen infrastructure that will be needed to support FCEV deployment; however, more work is needed in this area, as recent alternative vehicle markets have not relied as heavily on new and expanded infrastructure as FCEVs will.

- The project has done a great job of sorting through the literature and summarizing it. The report is publicly available.
- It is great to have these topics all reviewed in one presentation. Fairly limited original work was presented, but the review and distillation are valuable.
- A good review of the literature has been completed, but as the presenter mentioned, nothing unexpected was found. It is, however, good to have the summary in a single location.
- While the information provided is informative and relevant, it would be beneficial to go a step further and lay out the results in a coherent and prioritized approach that could be used by DOE for policy development and by researchers to implement into their models to evaluate the economic impact on fuel cell sales and hydrogen infrastructure development.

Question 3: Collaboration and coordination with other institutions

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

- Given the scope of the work as a literature review, the level of collaboration for this initial literature review was good. Going forward, it would be beneficial to work with owners of the currently developed models to implement some of these findings into their models and determine the impact.
- This project was conducted primarily by the principal investigator. However, the project did gain valuable feedback in the form of peer review by several knowledgeable researchers.
- Given the nature of this project, collaboration was not critical.

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 the impact of costs, benefits, and incentives on customer behavior is a challenging yet important piece to the FCEV puzzle. This study and the information it gathered do an excellent job addressing these issues.
- This work is an important contribution to the Hydrogen and Fuel Cells Program (the Program). Having a single summary of all these studies simplifies analysis for other researchers.
- This project helps bridge the gap between deployment of FCEVs and deployment of other alternative fuel vehicles. Understanding what has facilitated the deployment of alternative fuel vehicles will aid policymakers in better understanding how policies and incentives can be structured to support deployment of FCEVs and hydrogen infrastructure.
- The policy issues reviewed are very relevant given the commercialization status of vehicles.
- No major new findings were uncovered.

Question 5: Proposed future work

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

- Comparative infrastructure cost data are a valuable addition.
- Proposed future work activities are somewhat vague. Beyond publishing this work and broadly supporting DOE in its deployment of FCEVs, this project should attempt to bridge the gap between what has been learned from the literature and what DOE should do to support deployment of FCEVs. The principal investigator should provide advice on research gaps, discuss what future research should be done in this space, and investigate the requirements for deployment of hydrogen infrastructure, which differs from HEV and BEV deployment needs.
- The author should look at impactful policies in other countries (e.g., revenue-neutral fee-based in France). It is also unclear how much can be learned from HEVs and plug-in hybrid electric vehicles (PHEVs) related to FCEVs since HEVs and PHEVs do not have infrastructure issues. The project should learn from other countries that have successfully implemented new fuels (e.g., E85).

- Other than publishing the results of the literature review, there are no concrete future work plans. It would be better if the team specified what models it is going to assist in developing and how it will assist. It is not clear whether there is additional research that could be done on compressed natural gas or biofuels in other countries that could be used to augment the current literature review.

Project strengths:

- This project takes a very broad search of the available literature on deployment of alternative fuel vehicles to understand the deployment of FCEVs better.
- This is a comprehensive review of literature by a capable investigator.
- The results of this work are interesting and relevant to the Program.
- This is a good literature review.

Project weaknesses:

- How the project addressed last year's Annual Merit Review comments should be delineated in "Responses to Previous Year's Reviewers' Comments."
- The focus on only U.S. policies is a weakness.
- The lessons learned from other alternate fuel vehicles are limited in regard to the need for hydrogen fueling infrastructure for FCEVs. Research areas such as revealed preferences, in which much of the literature is devoted to HEVs, are limited in application to FCEVs because of the differences in infrastructure needs.

Recommendations for additions/deletions to project scope:

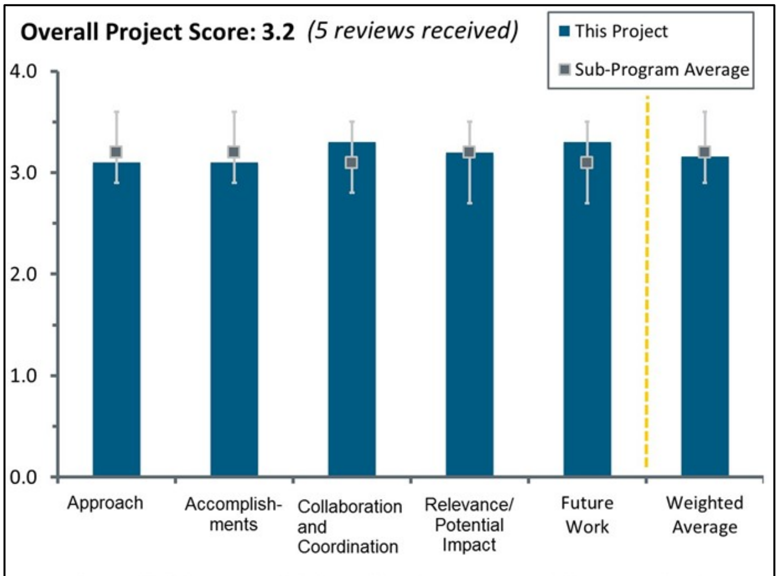
- The project should make some of the cited articles available on the DOE or Baker Institute website if copyright restrictions allow.
- Because hydrogen infrastructure needs are different from HEV and BEV infrastructure needs, the project should investigate this space further. A review of literature on E85 and natural gas transportation fueling infrastructure may be informative.
- The project should review policies, success stories, and failures related to fuel infrastructure worldwide.

Project #SA-059: Sustainability Analysis

Marc Melaina; National Renewable Energy Laboratory

Brief Summary of Project:

This project is conducting a sustainability analysis of hydrogen supply and stationary fuel cell systems using the Hydrogen Regional Sustainability (HyReS) framework. Investigators will develop regional metrics around upstream hydrogen supply chains, ensuring consistency with existing frameworks and tools used by engineering firms, the sustainable business community, and green investors. The project will leverage the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET™) model with the spatial detail of the Scenario Evaluation, Regionalization, and Analysis (SERA) model. Outcomes will include pathway cases, a beta framework, and a final public framework.



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 scope of these projects continues to be very impressive. Understanding sustainability is a totally brilliant addition to the analysis portfolio. The team is developing sustainability indicators, understanding the target audience, and reducing this analysis to regional-level data.
- The project takes a rational approach to assessing sustainability of fuel pathways. Regional production of hydrogen is desirable due to difficulties in transportation. The concept of sustainability is complex enough that it is difficult to derive a single “sustainability index.” The project should continue to calculate and report numerous aspects of sustainability and allow users to do their own weighting of desired outcomes. The open approach should promote wide use and acceptance.
- The project involves life-cycle analysis of hydrogen supply network for fuel cell applications. A unique aspect of the analysis is that it includes environmental impact and sustainability metrics.
- The project title includes stationary fuel cell systems, but project work to date and future work do not address stationary fuel cells.
- The interpretation of “sustainability” does not seem to capture well the social and economic aspects of sustainability. It is more focused on the environmental aspects, which is too narrow a focus. While the categories are captured (slide 4), the economic and social aspects were not emphasized on slides 12–15. If the researchers are endeavoring to extend life-cycle analysis (environmental) to a sustainability analysis, those are the two areas that need to be emphasized.

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.

- Starting with a roundtable of experts was excellent and generated collaborative ideas and ranked users and use characteristics. It is true that system-level indicators are the most important here, but using the DOE suite of models, it should always be possible to drill down and figure out what the most important drivers are.

- Since the project is just starting, most accomplishments are related to planning. The framework structure and goals seem to have been defined well.
- The project appears to be in the very early stage of development and just recently obtained stakeholder input at the roundtable meeting. Sustainability covers a very broad area, and the project appears to be seeking direction.
- Engaging stakeholders is useful. This is a very difficult subject to capture adequately, and the steps taken so far have been useful for informing the effort. It does not appear there is a very clear definition of the metrics that will be used yet, other than a few examples.
- This is a new project (started in September 2015), so there is little progress to report (as of the March date on the slide decks). Much of what is reported deals with the set-up of the project: establishing partners, pulling together models, and hosting workshops to obtain stakeholder input.

Question 3: Collaboration and coordination with other institutions

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

- The HyReS workshop made an effort to gather input from stakeholders. The project has assembled a strong team of partners. The work will access data from other entities to merge into an overall sustainability framework.
- The roundtable meeting held at the National Renewable Energy Laboratory provided a good forum for obtaining stakeholder input and expectations from the project.
- Collaboration is excellent, bringing in partners from national laboratories and industrial leaders.
- Having industry involved on a steering team is an excellent way to encourage collaboration. It is not clear whether there was an effort to get steering team members to contribute resources to the project.
- With this sort of project, there can always be more collaborators and sources of data.

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 totally addresses barriers A, B, and D of the Fuel Cell Technology Office Multi-Year Research, Development, and Demonstration Plan.
- Assuming hydrogen will be a fuel for the future, it is critical to understand its cost and impact on the environment and society. This project tries to develop a framework to quantify these metrics—a very daunting challenge. Knowing what challenges will need to be addressed is critical if hydrogen is ever expected to be a serious energy contender.
- This project is necessary to understand broad implications of different technologies.
- Sustainability is clearly relevant to all hydrogen supply pathways and stationary power generation, but this project appears to be having difficulty in establishing direction and focusing on the plan.
- These metrics are important, but it remains to be seen whether they will have a significant impact on implementation of hydrogen and fuel cells.

Question 5: Proposed future work

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

- The project has a good plan for assembling data and merging them into a rational sustainability framework.
- The project is just six months into its three-year life. There is no need to reframe future plans.
- If the team can bring in more experts and databases, this will be outstanding.

- Future work should cover a broader mix of hydrogen supply pathways such as liquid hydrogen, distributed steam methane reforming, and central water electrolysis. It is not clear whether and how stationary power generation is being addressed in the project.

Project strengths:

- A face-to-face roundtable meeting was conducted to obtain stakeholder input to the project.
- A project strength is understanding the sustainability of energy systems.
- Project strengths are its strong analysis capability and leveraging of data from multiple sources.
- The integration of a broad range of models is a project strength.

Project weaknesses:

- It is difficult to quantify results. There is a lot of subjective input and interpretation.
- The project needs to consider legal and ownership issues. If water is an input, it does not matter where it is; it matters who owns it, etc.
- The overall goal of the project is not clearly defined.

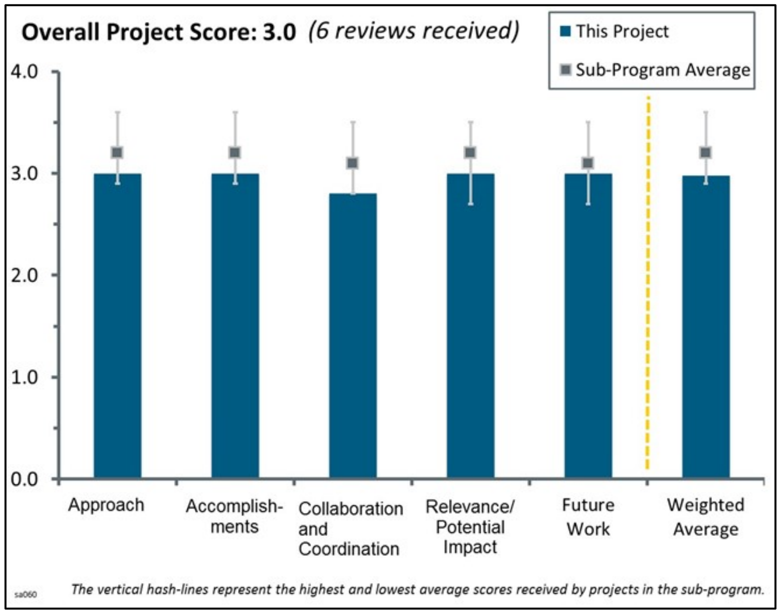
Recommendations for additions/deletions to project scope:

- If stationary power generation is included in the project scope, the work should include proton exchange membrane fuel cells, solid oxide fuel cells, and molten carbonate fuel cells.

Project #SA-060: Evaluation of Technology Status Compared to Program Targets

Marc Melaina; National Renewable Energy Laboratory

Brief Summary of Project:



This analysis is establishing the link between Fuel Cell Technology Office (FCTO) targets and future market dynamics, including explicit policy drivers. Objectives are to understand the influence of meeting, exceeding, or falling short of FCTO goals on future market adoption of fuel cell electric vehicles (FCEVs); consider a wide range of uncertainties around future technology progress; and combine techno-economic and consumer choice analysis of FCEVs in an integrated framework. Investigators will integrate techno-economic (Future Automotive Systems Technology Simulator [FASTSim]) and market potential (Automotive Deployment Options Projection Tool [ADOPT]) modeling capabilities developed within the Vehicle Technologies Office (VTO) analysis framework.

Question 1: Approach to performing the work

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

- The project generally has a good approach to using empirical data to break costs down to components. FASTSim and ADOPT seem like robust models and a good platform on which to base the project.
- This project is developing an analytical approach that integrates vehicle simulation with market adoption potential to create long-term scenarios extending to year 2035. It is using and modifying FASTSim and ADOPT to analyze FCEVs and other vehicle techno-economics and simulate future market share.
- The approach to the analysis project recognizes consumer choice in vehicle purchases and the impact of end-market incentives required to initiate and create market demand for new vehicle platforms. The analysis was focused on the two variables of government incentives for FCEV purchase and U.S. Energy Information Administration oil price that impact initial vehicle purchase and fuel operating costs. The analysis methodology included an update to two previous VTO models and the use of empirical data to support an understanding of FCEV market penetration rate.
- The difficulty of predicting consumer behavior is significant. The ADOPT model is a good effort to address consumer choice, but it may not be adequate for accurate prediction.
- It seems that the goal of the project was to find scenarios under which FCEVs can have a significant level of market penetration, instead of just modeling realistic scenarios to assess the potential future penetration of FCEVs. For instance, an accelerated scenario in which 2045 targets are met in 2035 is very aggressive and highly unlikely. A super-accelerated scenario in which 2045 targets are met in 2025 is even more unlikely. The issue with FASTSim is that the different vehicles evaluated do not have the same characteristics, unlike in Autonomie, which places all vehicle technologies on a more level playing field. The simplicity of FASTSim compared to Autonomie is appreciated though, and the fact that it reflects current vehicle models available commercially is also appreciated. A number of considerations need to be included to make the results more material, but not all of these issues are addressed in the proposed future work slide. These additional considerations need to be included in the analysis to ensure the validity of the model. These considerations should include consumer adoption where incentives are available vs. no incentives or fewer incentives, convenience of refueling, the effect of zero emission vehicles (ZEVs), low

and medium oil price cases, and mass reduction. It should also compare sales scenarios with planned hydrogen refueling station (HRS) deployment.
- The work is tautological. The targets were previously set on the basis of making fuel cells competitive with conventional vehicles, and now this study shows (not surprisingly) that if the goals are met, FCEVs will be competitive.

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 preliminary outcome of the project is encouraging for FCEVs since all analysis scenarios show the lifetime cost of FCEV is lower than for conventional vehicles and on par with hybrid electric vehicles (HEVs). A considerable amount of future work has been identified that covers many factors that can influence FCEV adoption and future analysis results.
- The National Renewable Energy Laboratory (NREL) analyzed several scenarios to determine the lifetime cost of FCEVs relative to conventional vehicles (CVs) and HEVs to identify technological improvements needed for reducing the cost and increasing adoption of FCEVs, and to assess the impact of policies that would support consumer adoption of FCEVs. It is unclear how the DOE used these results in developing policies and plans.
- Simulations have been carried out, and the model calibrated to total vehicle sales. The project should consider using the model to test HEVs, plug-in hybrid electric vehicles (PHEVs), or battery electric vehicles (BEVs) to look at other technology adoption barriers. This may have been done, but results were not shown.
- The analysis is very detailed, thorough, and carefully carried out. A scenario in which technology development falls short of technical targets should have been included. The blanket statement that “mass reduction has a greater effect than fuel cell cost reduction” on total cost does not seem to be accurate. It depends on the magnitude of the assumptions made for the two types of reductions. If one assumes huge mass reduction (38%–45%) but small fuel cell cost reduction (17%), then the project’s conclusion is accurate, but other sets of mass and cost reduction will result in different conclusions. As a result, the analysis, based on its assumptions, may not be guiding the FCTO in the right direction.
- More work needs to be done to ensure the validity of the results. It is important to ensure that the reader understands that the likelihood of the scenarios in which FCEVs can have significant market penetration are very unlikely. It would be valuable to see the sales and relative penalty results for BEVs compared to the other vehicle platforms. It is unclear why BEVs are predicted to have such a small sliver of the market in the future, particularly in the high-oil, extended-incentives scenario, when both of these characteristics benefit BEVs in theory.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- This project does not lend itself to collaboration very well, but at least the models used have been reviewed by original equipment manufacturers (OEMs) and hydrogen stakeholders.
- External peer reviewers, H2USA automotive OEM members, and the Fuel Pathway Integration Tech Team were listed as partners, but there were no specific interactions apparent in fiscal year 2016.
- Project collaboration included engagement in H2USA automotive members. It would be valuable to list the OEMs that were involved in providing input, review, and feedback to this work.
- Collaborations should be increased to include more marketing expertise and consumer-oriented researchers.
- It would be helpful to have the U.S. DRIVE Partnership’s Cradle-to-Grave working group peer review the results of the study. It would also be helpful to be transparent about comments received at the Program Annual Merit Review and other venues, and discuss how these comments were addressed.
- Just having partners review the work is not really collaboration. It would be more “collaborative” if others were actually involved in the work itself, such as development of parts of 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 **3.0** for its relevance/potential impact.

- The project recognizes the importance of policy, technology improvement, and energy costs on the market competitiveness of FCEV.
- Having an understanding of consumer choices and ultimately adoption is useful to many stakeholders who may want to use the information to feed into business case models.
- This project provides a tool for the FCTO (and VTO) to establish the relationship between program targets and future market dynamics, including explicit policy drivers. The tool is primarily for use by the DOE policymakers.
- Studies like these are very detailed and are carried out by smart people, but at the end of the day, the reality is that they do not actually advance the technology. The study may help guide the Program, but the impact could be greater if the funds were invested in research and development instead.
- The model attempts to predict consumer behavior and does a good job within its constraints, but it is unclear how relevant the results are to such a disruptive technology as FCEVs, for which refueling and other consumer concerns are different than other automotive technologies.
- In its current form, the project is not relevant. However, market penetration scenarios for vehicle technologies are important, and an updated model that reflects the latest knowledge using strong scenarios and robust models is needed.

Question 5: Proposed future work

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

- The proposed future work is important and in alignment with considerations that are currently missing from the assessment. The following considerations should be addressed: consumer adoption where incentives are available vs. no incentives or fewer incentives, convenience of refueling, and sales scenarios compared with planned HRS deployment.
- NREL discussed the proposed future work on modeling vehicle systems, better representation of policy drivers, and the effects of consumer choice and fueling availability. It was not clear whether the project has FCTO support to continue.
- The project team should understand the differences and similarities between FASTSim and Autonomie models.
- It is not clear how this work is related to or different from similar work done in Autonomie. Part of the future work should be to delineate clearly when models such as Autonomie should be used instead of FASTSim.
- The project is almost finished.

Project strengths:

- The expertise of NREL researchers, access to third-party stakeholders for review, and existence of models that the project can leverage are project strengths.
- The project scope of analysis was narrowed down to two key variables.
- The ADOPT model is well developed and rigorous.

Project weaknesses:

- The project introduced the FASTSim model to provide techno-economic simulation of vehicle powertrain platforms. FASTSim appears more limited than Autonomie.
- ADOPT does not appear to look at the impacts of infrastructure availability.
- There are a number of considerations that need to be included in the model to make it valid.

Recommendations for additions/deletions to project scope:

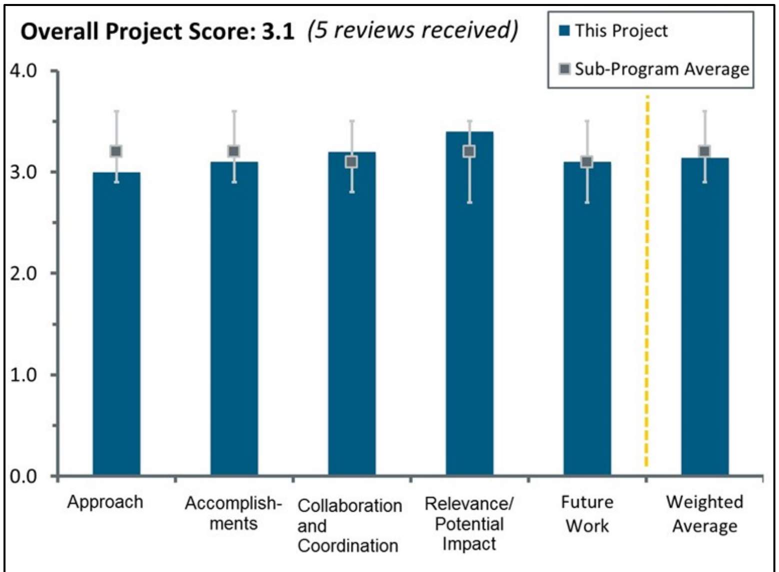
- The current analysis is based on the vehicle purchased by the consumer, and it would be valuable to understand whether a vehicle-leasing model would result in a faster rate of FCEV market adoption.
- The project should consider the use of agent-based models to predict consumer behavior. The project should test the ADOPT model on the market penetration of HEVs, PHEVs, and BEVs.
- The project should add analyses of the regionality effect, consumer adoption where incentives are available versus no incentives or fewer incentives, convenience of refueling, the effect of ZEVs, low and medium oil price cases, and mass reduction. The project should compare sales scenarios with planned HRS deployment.

Project #SA-061: National Fuel Cell Electric Vehicle and Hydrogen Fueling Station Scenarios

Marc Melaina; National Renewable Energy Laboratory

Brief Summary of Project:

This project is conducting integrated scenario analysis to assess interactions between fuel cell electric vehicle (FCEV) adoption, infrastructure requirements, and investment. Investigators will work with industry and other stakeholders to develop and analyze self-consistent national FCEV scenarios, examining early market trends and exploring long-term possibilities for FCEV adoption. The scenarios will be grounded in empirical data, early market plans, and technical analysis.



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 **3.0** for its approach.

- There is good use of stakeholder experience and focus areas, as well as models at national laboratories. The project has a very effective strategy of scenario analysis of key market parameters. There is good focus on California market data, which is very important for useful results to apply to ROC. Scenarios based on historical data and near-term plans make very good sense. Using three time zones for introduction is very valid to get a better understanding of FCEV and station deployments.
- The technical approach is solid.
- The approach seems reasonable. The project is using many existing models. The researchers are trying to examine by region and focus on the most likely areas. For the station siting, they are not taking into consideration zoning laws, accessibility for the delivery, or electrical power accessibility, and they are using land costs that are not realistic (Hydrogen Analysis [H2A] model defaults at $50,000/acre do not reflect urban land prices). Of course, many people will lease their land, and this is also not considered. Some (or even most) of these considerations may not be reasonable, but the land costs definitely need to be changed. Almost every station will require an upgrade in electrical power, and it is not clear whether that is included in the cost estimates. These costs are important for the business case scenarios reported in the presentation.
- It is difficult to assess the validity of this study without understanding how other vehicle technologies would fare under similar scenarios and what the likelihood of the cases is. Current sensitivity analysis in the model has identified key parameters and market penetration bottlenecks. Further, regional economic considerations do not appear to be factored in; regional levels of economic well-being affect the adoption of alternative vehicle technologies. Another factor that does not appear to be considered is the price of alternative fuels and vehicles and how that will affect market penetration. Differences in land availability and cost in urban vs. suburban areas are not considered. Incentives beyond zero-emissions vehicles (ZEVs) (e.g., access to high-occupancy vehicle [HOV] lanes, rebates) have not been considered in the analysis. Also, replicating the California financing structure in other states is not ideal. It would be useful to vet the results against other market penetration scenarios and assess the differences.
- There seem to be very bullish assumptions on adoption of technologies. This is a critical assumption and appears to influence the results significantly. A better approach would be to consider historical adoption rates of alternative technologies. Perhaps the project team could do a look-back analysis on adoption expectations versus reality for some of those technologies and temper the current assumptions.

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.

- FCEV market share growth predictions are supported with realistic assumptions. There are good correlations with stations and daily hydrogen use. Station utilization factor is the key to success; stranded assets must be managed and minimized. The interactive scenario analysis tool is very neat. It has great potential to help in planning future hydrogen vehicles and stations. The Hydrogen Financial Analysis Scenario Tool (H2FAST) use is very well done.
- The project has done interesting work and made good progress.
- Given the complexity of the approach, the team has done a very good job at integrating the different models. Additional modeling of market penetration of the rest of the vehicle fleet would provide a more complete picture of the validity of the model and would address Barrier 4.5 A: Future Market Behavior. The results visualization tool is a very informative addition to the project.
- The project has made good progress, given the budgets. The utilization scenarios are very useful and interesting. Some of the findings are not very significant or surprising (e.g., on slide 12, "The more aggressive scenarios have substantially greater numbers of stations"). Some of the cost assumptions are not very robust and will need to be updated to improve the financial metrics. For example, the land cost of $50,000/acre in an urban setting is unrealistic. In addition, some station owners in urban areas may choose to lease the land. This option should be examined. The project needs to report the hydrogen cost and price assumptions for the business case scenarios.
- Generally, progress is good, but the project needs to have more realistic assumptions to be credible.

Question 3: Collaboration and coordination with other institutions

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

- There are very good interactions with California stakeholders.
- Given the number/type of partners listed in the overview, collaboration with third parties seems adequate. It would be advisable to include additional interactions with states other than California to get feedback on the assumptions and promote the use of the model.
- The collaboration seems California-centric. For this to be a national study, the project needs to recognize that other parts of the nation have different drivers from those in California. In addition to using H_2USA to provide comments on the project's refueling station assumptions, the team should engage energy companies to validate its assumptions, especially for the scenarios outside of California.
- This project would benefit from collaboration outside of the hydrogen space so that historical introduction of technologies can be understood. More challenges to the assumptions are needed, which includes those with viewpoints that hydrogen may not undergo rapid adoption.

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.

- This is interesting work with good potential to inform hydrogen infrastructure development.
- This has some important work in understanding how the market will develop.
- This project has good potential to be used by states to guide funding decisions.
- This is very useful work toward DOE goals.

Question 5: Proposed future work

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

- It is an excellent idea to incorporate municipal- and state-level plans; this will require many hours of work, but it will make the model much more useful for communicating with policymakers.
- The work plan is very relevant and timely. It would be good to know how the project will share its results with stakeholders.
- The future work seems to be engaging California about deployment activities. This makes the work very California-specific. The project needs to engage other entities as well to make this a true forward-looking document.

Project strengths:

- The analysis seems well done; useful information is being generated.
- The team's expertise with robust models is a project strength.

Project weaknesses:

- The project does not consider the potential to upgrade sites to higher capacity as a progression. This could be a path for potential future improvement.
- The magnitude of the assessment is huge, which makes it difficult for the team to integrate data at the appropriate scale to reflect national trends.

Recommendations for additions/deletions to project scope:

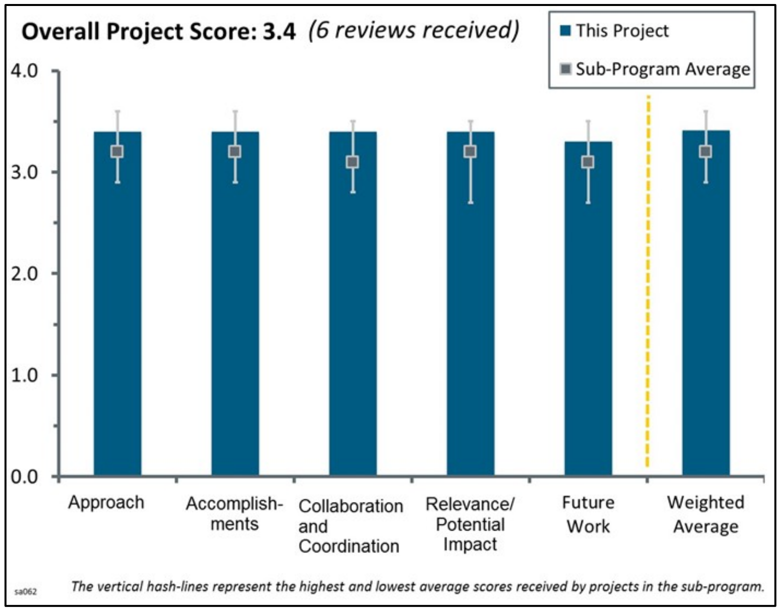
- The project seems well scoped.
- Regional economic considerations should be included; regional levels of economic well-being affect the adoption of alternative vehicle technologies. The price of alternative fuels and vehicles should be considered, as should how that will affect market penetration. The project should also consider differences in land availability and cost in urban vs. suburban areas, as well as incentives beyond ZEVs (e.g., access to HOV lanes, rebates). The project should validate the results against other studies and understand the differences.
- Having financing stakeholders review the results will be beneficial.

Project #SA-062: Expanded Capabilities for the Hydrogen Financial Analysis Scenario Tool

Marc Melaina; National Renewable Energy Laboratory

Brief Summary of Project:

The Hydrogen Financial Analysis Scenario Tool (H2FAST) enables detailed financial analysis for hydrogen infrastructure. This project is enhancing this tool with new capabilities to facilitate investments in hydrogen refueling stations and improve policy design decisions to support early hydrogen station and fuel cell electric vehicle market development. Examples of enhancements include improvements to usability, risk analysis for any input parameter, multiproduct configurations, multiple feedstock considerations, and expanded concurrent analysis of up to 300 hydrogen stations.



Question 1: Approach to performing the work

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

- This project aligns well with the Hydrogen and Fuel Cells Program (the Program) objectives of supporting infrastructure development by providing market and financial information relative to strategies for infrastructure development. Relevance and potential impact will really be measured by the utility of the tool for stakeholders. It would be useful to continue to gather feedback from potential future users as to the utility of the software. This information can be used to promote the tool as well as to evaluate the acceptability of the inputs and format.
- This is a comprehensive financial model taking into account the many cost variables that one would use to evaluate a hydrogen station or a network of stations.
- Improvements to H2FAST are useful and make the model more relevant. The model is comprehensive and flexible, allowing for a meaningful comparison of options.
- It is great to see H2FAST being completely enhanced with a variety of users and outputs in mind.
- The project is in Year 2 of a two-year effort to develop H2FAST financial analysis code for hydrogen infrastructure.

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 list of additions to the model is very impressive. The model looks relatively easy to use, with great graphical output allowing a large number of scenarios to be investigated.
- The additions made this year to the H2FAST project are significant. The stochastic risk analysis tool is an important part of making the tool more useful in estimating the economics of refueling stations. Many of the other additions seem like excellent model improvements. One area to consider is maintenance and repair of equipment at the filling station. With the frequency of compressor failures, this should be taken into account. It would be useful to consider the use of other hydrogen storage materials, such as chemical hydrogen storage materials and cryo-adsorbents. The change in infrastructure required for these materials

may require user-defined feedstocks (liquid nitrogen for example) and materials returning to be reprocessed.
- The enhancements made to include residual values help make this more realistic and allow for reuse and redeployment of equipment, which is critical in early build-out and station expansions. It would be useful to include upstream capital expenditures for trailers and central distribution centers to complete the network.
- The National Renewable Energy Laboratory (NREL) has taken note of reviewers' comments, addressed them, and implemented changes.
- Added features in the model appear to add flexibility.
- The model is a solid foundation to assess potential scenarios of hydrogen refueling station deployment in the United States, but it is unlikely that venture capitalists and/or investors will use models other than their own to make investment decisions. Although the approach is good, it contributes little to meeting infrastructure deployment goals. As part of the approach, NREL should assess whether this model has resulted in any investment decisions or is used purely for academic purposes.

Question 3: Collaboration and coordination with other institutions

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

- There is a good level of collaboration with (including meaningful contributions from) a wide variety of industry and government stakeholders.
- The team seems to have engaged a very comprehensive list of folks representing most expected stakeholders and has used many of them for direct input into the model.
- The project has engagement with the right financial institutions, investment community, and advisors.
- There is a good mix of collaborators representing industry, state, academia, and laboratory stakeholders.
- Collaboration with all the potential users of this software is critical to its success: policy and government decision makers, station operators, equity investors, strategic investors, and lenders. The success of the software requires that they buy into the approach and how the input and output are presented. It would be useful to the reviewers to understand what suggestions the collaborators have made and how those suggestions have been addressed in 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 **3.4** for its relevance/potential impact.

- The project has made an excellent effort to develop information using standard accounting practices and producing results in a form that could be useful for investors and lenders to understand the results. The challenging part of using the H2FAST tool would be determining the inputs. Efforts should be made to facilitate coupling information from the H2A Refueling Station Analysis Model (HRSAM) and other modeling tool outputs to the H2FAST inputs. The types of scenarios and their assumptions should be similar. If the data from other models do not easily feed H2FAST, it may require changes to the H2FAST tool itself.
- The work addresses barriers A and E of the Program Multi-Year Research, Development, and Demonstration Plan.
- It is good that the investigators have expanded the scope to include policymakers and regulators. This is the community for which DOE should be promoting the development of economic analysis tools.
- The industry needs a common tool that varies in complexity based on the level of financial and technical expertise. This tool accomplishes this.
- Information on the total costs of hydrogen as a fuel is needed, and this project addresses that need.
- Although the model appears very robust, it is unlikely that it will result in investment decisions. Investors have their own financial models and are not likely to use financial models developed elsewhere. Further, at this stage, market entrants are mostly technology manufacturers and gas producers that are already familiar with the economics of hydrogen refueling stations.

Question 5: Proposed future work

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

- Even more great additions are planned for the model. It is especially nice to see the plan for customization for a variety of stakeholders.
- Allowing analysis of non-simultaneous projects seems to be a very important aspect of the model that should be included. Emissions calculations could be useful but only if they can be tied to the costs, such as through application of Low Carbon Fuel Standard Program or Renewable Identification Number (RIN) credits.
- Including central source and compression options for large-scale electrolyzers and medium-scale steam methane reformers would be valuable. Perhaps another sheet could be created for central/regional sources and distribution centers that includes the above production sources with capital and operational expenditures and calculations. It is not clear whether incremental capital could be added to existing stations for capacity expansions and enhancements.
- Proposed future work seems appropriate. After the model is completed, the team should focus on outreach, model demonstrations, and assessing whether the model is being used to make investment decisions.
- This project is wrapping up and transitioning to a new project (SA-059).
- It is not clear whether the emissions calculations would be leveraging an existing model. One hopes that this will not be a duplication of work.

Project strengths:

- Increased capabilities are being added to an already useful modeling tool. Analysis is enabled by a large number of diverse stakeholders.
- Project strengths include a common platform for evaluating station projects, the project being fairly comprehensive, and the varying degrees of complexity based on needs.
- Strengths include the knowledge of the strong team and good subject matter expert engagement. In addition, H2FAST is comprehensive.

Project weaknesses:

- Since station profitability depends highly on hydrogen source and distribution methods, having the ability to evaluate production and logistics options would be a great enhancement. This would be most effective if the project partners could collaborate with electrolyzer and steam methane reformer manufacturers.
- A business case is needed to develop the model and determine who is using it and whether there is a need for this type of model by venture capitalists/investors/lenders.

Recommendations for additions/deletions to project scope:

- No responses provided.

Attendee List: 2016 Hydrogen and Fuel Cells Program

Last Name	First Name	Organization
Abraham	Judi	Alliance Technical Services, Inc.
Aceves	Salvador	Lawrence Livermore National Laboratory
Acharya	Gopalakrishna	IndianOil Corporation
Adams	Jesse	U.S. Department of Energy
Adzic	Radoslav	Brookhaven National Laboratory
Afzal	Kareem	PDC Machines
Afzal	Mateen	PDC Machines
Ahluwalia	Rajesh	Argonne National Laboratory
Ahmed	Alauddin	University of Michigan
Ahn	Channing	California Institute of Technology
Ainscough	Christopher	National Renewable Energy Laboratory
Alia	Shaun	National Renewable Energy Laboratory
Alkire	James	U.S. Department of Energy
Allendorf	Mark	Sandia National Laboratories
Allerston	Laura	University of Birmingham
Amine	Khalil	Argonne National Laboratory
Anton	Donald	Savannah River National Laboratory
Ardo	Shane	University of California, Irvine
Arif	Muhammad	National Institute of Standards and Technology
Arntsen	Christopher	University of Chicago
Arthur	David	U.S. Department of Transportation
Asazawa	Koichiro	Daihatsu Motor
Atanassov	Plamen	University of New Mexico
Atkins	Lance	Nissan Technical Center North America
Audus	Debra	Independent
Autrey	Tom	Pacific Northwest National Laboratory
Ayala	Anthony	National Institute of Standards and Technology
Ayers	Katherine	Proton OnSite
Azami Ghadkolai	Milad	Clemson University
Bae	Chulsung	Rensselaer Polytechnic Institute
Bahar	Bamdad	Xergy Inc.
Bai	Qiang	University of Maryland
Baker	David	Army Research Laboratory
Baker	Warren	Naval Surface Warfare Center
Balke	Nina	Oak Ridge National Laboratory
Barbouti	Mohammed	University of Waterloo
Barilo	Nick	Pacific Northwest National Laboratory
Barnes	Ted	Gas Technology Institute
Baturina	Olga	Naval Research Laboratory

Last Name	First Name	Organization
Bayer	Armin	Greenerity GmbH
Beauvais	Christian	Rowan University
Beckman	Michael	Linde North America
Behling	Noriko	Kyushu University
Benard	Pierre	Hydrogen Research Institute
Bender	Guido	National Renewable Energy Laboratory
Bendersky	Leonid	National Institute of Standards and Technology
Benjamin	Tom	Argonne National Laboratory
Bennett	Jeffrey	Southwest Research Institute
Berry	Naveen	South Coast Air Quality Management District
Bershad	Susan	National Fire Protection Association
Bertin	Erwan	National Institute of Standards and Technology
Bessarabov	Dmitri	Hydrogen South African (HySA) Center at North-West University
Beyer	Johan	PowerCell Sweden AB
Bigelow	Erik	Center for Transportation and the Environment (CTE)
Binder	Matthias	Greenerity GmbH
Blackburn	Bryan	Redox Power Systems
Blanco	Mauricio	Ballard Power Systems
Blekhman	David	California State University, Los Angeles
Blount	James	
Boardman	Richard	Idaho National Laboratory
Bollepalli	Srinivas	Sainergy
Bordeaux	Christopher	Bordeaux International Energy Consulting, LLC
Borup	Rod	Los Alamos National Laboratory
Bouwkamp	Nico	California Fuel Cell Partnership
Bowden	Mark	Pacific Northwest National Laboratory
Bowman	Robert	Oak Ridge National Laboratory
Boyd	Robert	Boyd Hydrogen, LLC
Britt	Michael	United Parcel Service, Inc. (UPS)
Brodd	Ralph	Broddarp of Nevada
Broerman	Eugene	Southwest Research Institute
Brooker	Paul	University of Central Florida
Brooks	Kriston	Pacific Northwest National Laboratory
Brown	Craig	National Institute of Standards and Technology
Brown	Daryl	Pacific Northwest National Laboratory
Brunner	Tobias	HynErgy GmbH
Bunnelle	Eric	ExxonMobil
Burgess	Robert	National Renewable Energy Laboratory
Burgunder	Albert	Praxair, Inc.
Burr	Stephanie	RCF Economic & Financial Consulting
Bush	Brian	National Renewable Energy Laboratory
Buttner	William	National Renewable Energy Laboratory

Last Name	First Name	Organization
Byham	Stephanie	CSRA Inc.
Capuano	Christopher	Proton OnSite
Carcassi	Marco Nicola	University of Pisa
Carey	Christian	National Renewable Energy Laboratory
Cargnelli	Joseph	Hydrogenics Corporation
Carlsson	Anna	myFC
Casalegno	Andrea	Politecnico di Milano
Caton	Melanie	National Renewable Energy Laboratory
Centeck	Kevin	U.S. Army, Tank Automotive Research, Development and Engineering Center (TARDEC)
Chang	Sung-Chun	Industrial Technology Research Institute
Chang	Zhihong	Vanderbilt University
Chapman	Bryan	ExxonMobil
Chen	Dejun	Georgetown University
Chen	Yanan	University of Maryland
Chen	Yu	Georgia Institute of Technology
Chen	Yuche	National Renewable Energy Laboratory
Chen	Zhongwei	University of Waterloo
Chernicoff	William	Toyota Motor, North America, Inc.
Chigusa	Sean	Kobelco Compressors America, Inc.
childress	Anthony	Clemson University
Cho	Jinhee	Korea Institute of Energy Technology Evaluation and Planning
Choi	Baeck	University of Florida
Choi	Sun Hee	Korea Institute of Science and Technology
Choudhury	Biswajit	DuPont
Christensen	John	Consultant
Chuang	Steven	University of Akron
Colella	Whitney	Johns Hopkins University
Collins	William	WPCSol, LLC
Colon-Mercado	Hector	Savannah River National Laboratory
Contini	Vincent	Battelle
Corgnale	Claudio	Savannah River Consulting
Cosacescu	Liviu	GWM Limited
Cox	Philip	Mainstream Engineering Corporation
Crawford	Lalida	U.S. Department of Energy
Creager	Stephen	Clemson University
Crouch-Baker	Steven	SRI International
Crum	Matthew	W.L. Gore & Associates, Inc.
Cullen	David	Oak Ridge National Laboratory
Curry-Nkansah	Maria	Argonne National Laboratory
Curtin	Sandra	Fuel Cell and Hydrogen Energy Association
Dai	Qiang	Argonne National Laboratory
Dale	Nilesh	Nissan Technical Center North America

Last Name	First Name	Organization
Dameron	Arrelaine	National Renewable Energy Laboratory
Dandridge	Karen	U.S. Department of Energy
Danilovic	Nemanja	Proton OnSite
Das	Anirban	Wayne State University
Das	Papiya	
Davenport	Rachel	Alliance Technical Services, Inc.
Dawson	Andrew	Resilient Systems Engineering LLC
De Castro	Emory	Advent Technologies, Inc.
Deason	Katherine	WorleyParsons
DeCuollo	Gerald	TreadStone Technologies, Inc.
Derksen	Bridget	U.S. Department of Energy
DeSantis	Daniel	Strategic Analysis, Inc.
Deutsch	Todd	National Renewable Energy Laboratory
Devlin	Pete	U.S. Department of Energy
Dillich	Sara	Retired
Dimitrievska	Mirjana	National Renewable Energy Laboratory
Dinh	Huyen	National Renewable Energy Laboratory
Dirschka	Eric	NASA
Dismukes	Charles	Rutgers University
Dobbins	Tabbetha	Rowan University
Dolan	Connor	Fuel Cell and Hydrogen Energy Association
Dorgan	Byron	U.S. Senate (retired)
Du	Bingchen	DataRevive
Duleep	Gopalakrishnan	H-D Systems
Dunwoody	Catherine	California Air Resources Board
Duranty	Edward	Pacific Northwest National Laboratory
Edgecombe	Brian	Materia Inc.
Edwards	David	Air Liquide
Elangovan	S.	Ceramatec, Inc.
El-Awady	Jaafar	Johns Hopkins University
Elgowainy	Amgad	Argonne National Laboratory
Ellis	Stephen	American Honda Motor Co., Inc.
Elrick	Bill	California Fuel Cell Partnership
Elvington	Mark	Savannah River Consulting
Epping Martin	Kathi	Alliance Technical Services, Inc.
Erlebacher	Jonah	Johns Hopkins University
Ermanoski	Ivan	Sandia National Laboratories
Eudy	Leslie	National Renewable Energy Laboratory
Ewan	Mitch	Hawaii Natural Energy Institute
Fabian	Tibor	Ardica Technologies
Fairbrother	Andrew	National Institute of Standards and Technology
Farese	David	Air Products and Chemicals, Inc.

Last Name	First Name	Organization
Farmer	Richard	U.S. Department of Energy
Felter	Thomas	Sandia National Laboratories
Feng	Zhili	Oak Ridge National Laboratory
Ferrandon	Magali	Argonne National Laboratory
Fisher	Allison	Florida Solar Energy Center
Fisher	Leah	CSRA
Fitzgerald	Jay	U.S. Department of Energy
Flecky	William	PneumatiCoat Technologies
Fontaine	Joe	Naval Undersea Warfare Center
Forbes	Stephen	Air Liquide
Freese	Charles	General Motors
Fronk	Matt	Kodak
Fullenkamp	Patrick	GLWN
Fultz	Brent	California Institute of Technology
Funk	Stuart	LMI
Gagliano	Joe	California Fuel Cell Partnership
Gaillard	Nicolas	University of Hawaii
Ganesan	Prabhu	Savannah River Consulting
Gangi	Jennifer	Fuel Cell and Hydrogen Energy Association
Gangloff	John	U.S. Department of Energy
Gao	Tingting	University of Maryland
Gao	Yong	Southern Illinois University
Garcia	Alberto	Fuel Cells and Hydrogen Joint Undertaking (FCH JU)
Garland	Nancy	U.S. Department of Energy
Garsany	Yannick	Excet Inc.
Garwood	Ryan	Rowan University
Garzon	Fernando	University of New Mexico
Gasper	Raymond	University of Massachusetts Amherst
Geiman	Laura	W.L. Gore & Associates, Inc.
Gennett	Thomas	National Renewable Energy Laboratory
George	Paul	Battelle
Gheytani	Saman	University of Houston
Ghosh	Sujit	U.S. Maritime Administration
Gillette	Eleanor	National Institute of Standards and Technology
Girard	Francois	National Research Council Canada
Gittleman	Craig	General Motors
Goldstein	Brian	Energy Independence Now
Gomez	Carlos	U.S. Department of Energy
Goodarzi	Abas	US Hybrid Corporation
Goto	Risei	Sumitomo Corporation
Goudy	Andrew	Delaware State University
Grabow	Lars	University of Houston

Last Name	First Name	Organization
Grassilli	Leo	Office of Naval Research
Greene	David	University of Tennessee
Greenway	Scott	Greenway Energy LLC
Greenway	Scott	Savannah River Consulting
Groß	Markus	BMW
Grot	Stephen	Ion Power, Inc.
Groth	Katrina	Sandia National Laboratories
Gupta	Erika	U.S. Department of Energy
Gupta	Ram	Virginia Commonwealth University
Gupta	Shalabh	Ames Laboratory
Guthrie	Lisa	Prime Mover International, LLC
Gwin	Ray	Earth Day Texas
Hubner	Gerold	Volkswagen AG
Haak	Nico	SGL Carbon GmbH
Habibzadeh	Bahman	U.S. Department of Energy
Haight	Andrea	Composite Technology Development, Inc.
Halevi	Barr	Pajarito Powder, LLC
Hamano	Masato	Toyota
Hamilton	Jennifer	California Fuel Cell Partnership
Han	Hui-Ling	Lawrence Berkeley National Laboratory
Han	Jeongwoo	Argonne National Laboratory
Hanlin	Jason	Center for Transportation and the Environment
Hardis	Jonathan	National Institute of Standards and Technology
Hardy	Bruce	Savannah River National Laboratory
Hardy	David	U.S. Department of Energy
Harned	Alleyn	Virginia Clean Cities
Harris	Alexander	Brookhaven National Laboratory
Harris	Tequila	Georgia Institute of Technology
Harrison	Kevin	National Renewable Energy Laboratory
Harrison	William	NanoSonic, Inc.
Hartman	Brent	CSA Group
Hauber	David	Automated Dynamics
Haug	Andrew	3M Company
Hays	Charles	Texas A&M University
He	Xingfeng	University of Maryland
Hecht	Ethan	Sandia National Laboratories
Heim	Eric	United States Advanced Battery Consortium, LLC
Heirs	Rupert	Consultant
Held	David	DENSO
Hellstern	Thomas	Stanford University
Heo	Su Jeong	University of Connecticut
Heo	Tae Wook	Lawrence Livermore National Laboratory

Last Name	First Name	Organization
Herbert	Thorsten	National Organization Hydrogen and Fuel Cell Technology
Herring	Andrew	Colorado School of Mines
Heske	Clemens	University of Nevada, Las Vegas
Hess	Karsten	German Embassy
Hickner	Michael	Penn State University
Hicks	Michael	H2PowerTech
Higgins	Drew	Stanford University
Hill	Laura	U.S. Department of Energy
Hilmi	Abdelkader	FuelCell Energy, Inc.
Hirai	Shuichiro	Tokyo Institute of Technology
Hirano	Shinichi	Ford Motor Company
Ho	Donna	U.S. Department of Energy
Hobbs	Milton	Miltec UV International
Holbrook	Michael	Strategic Analysis Inc.
Holladay	Jamie	Pacific Northwest National Laboratory
Holland	Wendolyn	Holland Consulting, LLC
Holzmann	Markus	Greenerity GmbH
Horacek	Philip	Powertech Labs
Houchins	Cassidy	Strategic Analysis Inc.
Hovsapian	Rob	Idaho National Laboratory
Hu	Hongxing	Amsen Technologies, LLC
Hua	Thanh	Argonne National Laboratory
Huang	Kevin	University of South Carolina
Huang	Yi-Lin	University of Maryland
Hulvey	Zeric	U.S. Department of Energy
Hurst	Katherine	National Renewable Energy Laboratory
Hussey	Daniel	National Institute of Standards and Technology
Huya-Kouadio	Jennie	Strategic Analysis, Inc.
Icard	Grady	Teledyne Energy Systems
Igarashi	Hiroshi	N.E. Chemcat Corporation
Iiyama	Akihiro	University of Yamanashi
Ikeda	Tetsufumi	The Research Association of Hydrogen Supply/Utilization Technology
Impullitti	Joseph	South Coast Air Quality Management District
Ireland	Dan	Solvay
Irvine	Lawrence	Plug Power
ISHII	Chiaki	Technova Inc.
Ishikawa	Hiroya	NGK Spark Plugs
Jacobson	David	National Institute of Standards and Technology
Jacobson	Richard	Intertek
Jahnke	Fred	FuelCell Energy, Inc.
Jahnke	Justin	Oak Ridge Associate Universities
Jakupca	Ian	NASA

Last Name	First Name	Organization
Jalan	Radha	ElectroChem
James	Brian	Strategic Analysis, Inc.
James	Rachel	HCATT
James	Will	U.S. Department of Energy
Jamis	Alexandra	ICF International
Janot	Raphael	LRCS–CNRS
Jaramillo	Thomas	Stanford University
Jarvis	William	FLEXcon
Jaworsk	Casey	Tanaka Kikinzoku International (America), Inc.
Jeffers	Matthew	National Renewable Energy Laboratory
Jensen	Craig	University of Hawaii
Jerram	Lisa	Navigant Research
Jeske	Gerald	Umicore
Jia	Chao	University of Maryland
Jia	Hongfei	Toyota
Jiao	Feng	University of Delaware
Jilani	Safia	Georgetown University
Jin	Jonathan	U.S. Department of Energy
Johnson	John	Michigan Technological University
Johnson	Kenneth	Pacific Northwest National Laboratory
Johnson	Terry	Sandia National Laboratories
Joseck	Fred	U.S. Department of Energy
Josefik	Nicholas	US Army Corps of Engineers ERDC-CERL
Joseph	Tom	Bethlehem Hydrogen
Juthani	Nidhi	University of Waterloo
Kadowaki	Masataka	NEDO
Kaiser	Mohammad Rejaul	University of Maryland
Kamijo	Motohisa	Nissan
Kaneko	Yasushi	PANASONIC
Kanesaka	Hiroyuki	FC-Cubic TRA
Karathanou	Argyro	Federation for Fuel Cell Research FCLAB
Kast	James	U.S. Department of Energy
Kastner	Mark	DENSO
Kays	Steven	3M Company
Keller	Jay	Zero Carbon Energy Solutions
Kelly	Patrick	PathTwo
Kent	Ronald	SoCalGas
Khalil	John	United Technologies Research Center
Khan	Awais	U.S. Department of Energy
Kiani	Daniyal	Georgetown University
Kim	Hyoung-Juhn	Korea Institute of Science and Technology
Kim	JuYong	J&L Tech

Last Name	First Name	Organization
Kim	Kiyoung	ILSUNG Machinery Co. Ltd.
Kim	Tae-Young	Korea Institute of Energy Research
Kim	Yongmin	Stanford University
Kim	Yu Seung	Los Alamos National Laboratory
Kimura	Tatsusaburo	Sumitomo Corporation
King	Joel	Alion Science and Technology
Kishimoto	Takeaki	Nisshinbo Holdings Inc.
Klahr	Benjamin	U.S. Department of Energy
Klebanoff	Lennie	Sandia National Laboratories
Kleen	Greg	U.S. Department of Energy
Kliesch	Jim	American Honda Motor Co.
Knight	Douglas	Intelligent Energy
Knights	Shanna	Ballard Power Systems
Knopp	Katy	Fuel Cell & Hydrogen Energy Association
Kocha	Shyan	National Renewable Energy Laboratory
Kolasinski	Robert	Sandia National Laboratories
Komini Babu	Siddharth	Carnegie Mellon University
Komoda	Hiroaki	Honda
Kondo	Shinji	Sumitomo Corporation
Kongkanand	Anusorn	General Motors
Kopasz	John	Argonne National Laboratory
Koski	Brent	United Hydrogen
Kotake	Tomohito	Honda
Kraigsley	Alison	National Institute of Allergy and Infectious Dieases
Krause	Theodore	Argonne National Laboratory
Kriha	Ken	Gas Technology Institute
Kroemer	Joachim	Borit NV
Kung	Steven	Electric Power Research Institute
Kuppa	Shashi	National Highway Traffic Safety Admin
Kurtz	Jennifer	National Renewable Energy Laboratory
Kusoglu	Ahmet	Lawrence Berkeley National Laboratory
Laffen	Melissa	Alliance Technical Services, Inc.
LaFleur	Chris	Sandia National Laboratories
Lakshmanan	Balasubramanian	General Motors
Lalli	Jennifer	NanoSonic, Inc.
LaManna	Jacob	National Institute of Standards and Technology
Lamnin	Sara	City of Hayward
Langdo	Thomas	Redox Power Systems
Laskin	Jay	Hyenergy Consulting, LLC
Lauer	Suzanne	Alliance Technical Services, Inc.
Laughlin	Andrea	Altergy
Le	Duy	University of Central Florida

Last Name	First Name	Organization
Leachman	Jacob	Washington State University
Ledesma	Elias	University of Guanajuato
Lee	Doohwan	U.S. Department of Energy
Lee	Doohwan	University of Seoul
Lee	D-Y	Argonne National Laboratory
Lee	Uisung	Argonne National Laboratory
Lee	Won-Yong	Korea Institute of Energy Research
Leighton	DeLisa	Luxfert-GTM
Levinson	Rebecca	Sandia National Laboratories
Levy	Michael	Aaquis
Li	Gong Liang	NGK
Li	Hongqiang	Canadian Nuclear Laboratories
Li	Lin-Feng	Bettergy Corp.
Li	Tian	University of Maryland
Li	Wenyue	American University
Li	Yiju	University of Maryland
Li	Yongfeng	University of Maryland
Llang	Zhixiu	Brookhaven National Laboratory
Lin	Yuehe	Washington State university
Linic	Suljo	University of Michigan
Linkous	Clovis	Youngstown State University
Lipman	Timothy	University of California, Berkeley
Lipp	Ludwig	FuelCell Energy, Inc.
Litster	Shawn	Carnegie Mellon University
Litzelman	Scott	Booz Allen Hamilton
Liu	Boyang	University of Maryland
Liu	Di-Jia	Argonne National Laboratory
Liu	Dongxia	University of Maryland
Liu	Hong	Oregon State University
Liu	Xiujun	American University
Liu	Yang	Virginia Tech
Liu	Yangwei	Georgetown University
Liu	Yihua	Argonne National Laboratory
Logan	Bruce	Penn State University
Lombardi	Jonathan	U.S. Department of Energy
Long	Jeffrey	Lawrence Berkeley National Laboratory
Lu	Yunfeng	University of California, Los Angeles
Luc	Wesley	University of Delaware
Ludlow	Daryl	Ludlow Electrochemical Hardware
Lukkarinen	Timo	Horizon Educational
Lyubovksy	Max	U.S. Department of Energy
Ma	Zhong	Brookhaven National Laboratory

Last Name	First Name	Organization
Mac Dougall	Jim	Air Products and Chemicals, Inc.
MacLeod	Bill	Emerald Advisory LLC
Madden	Tom	Aquion Energy Inc.
Maes	Miguel	NASA
Mahmood	Zeeshan	Merck KgaA/EMD
Majzoub	Eric	University of Missouri – St. Louis
Maness	Pin-Ching	National Renewable Energy Laboratory
Mann	Margaret	National Renewable Energy Laboratory
Manthiram	Karthish	California Institute of Technology
Marcinkoski	Jason	U.S. Department of Energy
Maric	Radenka	University of Connecticut
Markovic	Nenad	Argonne National Laboratory
Markowitz	Morry	Fuel Cell and Hydrogen Energy Association
Maroufmashat	Azadeh	University of Waterloo
Martin	Josh	National Renewable Energy Laboratory
Martin	Richard	Ardica Technologies
Martinez	Andrew	California Air Resources Board
Masten	David	General Motors
Matsunaga	Tomoya	Toyota Motor Corporation
Matsushima	Toshiro	Honda Motor Co., Ltd.
Matter	Paul	pH Matter, LLC
Matzger	Adam	University of Michigan
Mavrikakis	Manos	University of Wisconsin–Madison
Mayyas	Ahmad	National Renewable Energy Laboratory
McDaniel	Dr. Anthony	Sandia National Laboratories
McElroy	Emma	Fuel Cell and Hydrogen Energy Association
McKeown	Kyle	The Linde Group
McQuaid	Christine	Access Interpreting
McQueen	Shawna	Energetics Incorporated
McWhorter	Scott	Savannah River National Laboratory
Medici	Vera	Technology Transition Corporation
Melaina	Marc	National Renewable Energy Laboratory
Meng	Hui	Brookhaven National Laboratory
Meng	Yang	University of Maryland
Mercuri, Ph.D.	Rebecca	Notable Software Inc.
Mertes	Catherine	RCF Economic & Financial Consulting
Miller	Brenton	Maryland Technical Review, LLC
Miller	Eric	U.S. Department of Energy
Milliken	JoAnn	JMilliken Consulting
Minh	Nguyen	University of California, San Diego
Mintz	Marianne	Argonne National Laboratory
Mitlin	David	Clarkson University

Last Name	First Name	Organization
Mittelsteadt	Cortney	Giner, Inc.
Moffat	Thomas	National Institute of Standards and Technology
Mohan	Ram	Singular Ventures LLP
Mohanpurkar	Manish	Idaho National Laboratory
Mohtadi	Rana	Toyota Research Institute of North America
Mok	Jongsoo	Hyundai
Molter	Trent	Sustainable Innovations
Moorthi	MuMu	Electrodes and More
More	Karren	Oak Ridge National Laboratory
Morelandg	Greg	CSRA
Moretto	Pietro	European Commission / Joint Research Centre
Morimoto	Yu	Toyota Central R&D Labs., Inc.
Motupally	Sathya	Doosan
Moulthrop	Larry	Proton OnSite
Mukerjee	Sanjeev	Northeastern University
Mukherjee	Sundeep	University of North Texas
Mukundan	Rangachary	Los Alamos National Laboratory
Muratori	Matteo	Pacific Northwest National Laboratory
Murphy	KP	Alliance Technical Services, Inc.
Murphy	Lilia	Alliance Technical Services, Inc.
Musgrave	Charles	University of Colorado Boulder
Mushtaq	Ali	IRD Fuel Cells
Muto	Kenichi	NEDO
Myers	Charles	CSRA
Myers	Debbie	Argonne National Laboratory
Nakahara	Hiroshi	EnerSys Advanced Systems
Nam	Sang Yong	Gyeongsang National University
Nawoj	Kristen	U.S. Department of Energy
Newhouse	Norman	Hexagon Lincoln
Neyerlin	Kenneth	National Renewable Energy Laboratory
Nguyen	Nha	U.S. Department of Transportation
Nguyen	Tien	U.S. Department of Energy
Nishimura	Shin	Kyushu University
Norris	Robert	Oak Ridge National Laboratory
Notardonato	William	NASA
O'Brien	Christopher	Ivys Inc.
O'Brien	James	Idaho National Laboratory
O'Connell	Daniel	Fuel Cell & Hydrogen Energy Association
O'Connor	Ian	Saft America, Inc.
Odgaard	Madeleine	IRD Fuel Cells
Oesterreich	Robert	Air Liquide
Offner	Arnold	Phoenix Contact

Last Name	First Name	Organization
Ogitsu	Tadashi	Lawrence Livermore National Laboratory
Ohma	Atsushi	Nissan
Olsen	Michele	National Renewable Energy Laboratory
Olson	Gregory	CSRA
Omasta	Travis	University of Connecticut
Onorato	Shaun	Allegheny Science & Technology
Ordaz	Grace	U.S. Department of Energy
O'Shaughnessy	W. Shannan	GVD Corporation
Ostrander	Graham	Automated Dynamics
Ott	Kevin	Consultant
Ou	Runqing	NEI Corporation
Paczkowski	Benjamin	U.S. Army, Tank Automotive Research, Development and Engineering Center (TARDEC)
Pal	Narendra	Bethlehem Hydrogen
Pallitto	Jeffrey	United Hydrogen
Pan	Mu	Wuhan University of Technology
Papageorgopoulos	Dimitrios	U.S. Department of Energy
Parilla	Philip	National Renewable Energy Laboratory
Park	Jaehyung	Korea Institute of Energy Technology Evaluation and Planning
Park	Seokhee	Korea Institute of Energy Research
Parkan	John Michael	Providence Entertainment
Parker	Eric	Fuel Cell and Hydrogen Energy Association
Parker	Melahn	Joi Scientific
Parks	George	FuelScience LLC
Patel	Pinakin	FuelCell Energy, Inc.
Pearlman	Howard	Advanced Cooling Technologies Inc.
Pecaro	Stephanie	Georgetown University
Pellegrinelli	Christopher	University of Maryland
Penney	Christopher	Rowan University
Peppin	Daniel	3M Company
Perry	Mike	United Technologies Research Center
Peters	Michael	National Renewable Energy Laboratory
Peterson	David	U.S. Department of Energy
Peterson	Lisa	U.S. Department of Energy
Petitpas	Guillaume	Lawrence Livermore National Laboratory
Petkov	Valeri	Central Michigan University
Petrecky	Jim	Plug Power
Petri	Randy	FuelCell Energy, Inc.
Petrie	Mark	SRI International
Pickles	Jeff	The Linde Group
Pien	Michael	ElectroChem
Pierpont	Daniel	3M Company
Pietrasz	Patrick	Ford Motor Company

Last Name	First Name	Organization
Pintauro	Peter	Vanderbilt University
Pivovar	Bryan	National Renewable Energy Laboratory
Polevaya	Olga	Nuvera Fuel Cells
Pollica	Darryl	Ivys Inc.
Pope	Jodie	National Institute of Standards and Technology
Popovich	Neil	National Renewable Energy Laboratory
Powell	Rick	Chevron
Powers	Cassie	National Association of State Energy Officials
Prakash	Amit	Wiretough Cylinders LLC
Prasad	Ajay	University of Delaware
Pratt	Joseph	Sandia National Laboratories
Prendergast	David	Lawrence Berkeley National Laboratory
Price	Rebecca	Energetics Incorporated
Puglia	Vincent	Quebec Government Office in Washington
Qin	Nan	University of Central Florida
Quackenbush	Karen	Fuel Cell and Hydrogen Association
Quong	Spencer	Quong & Associates, Inc.
Rahman	Talat	University of Central Florida
Ramani	Manikandan	Plug Power
Ramani	Vijay	IIT Chicago
Ramirez-Cuesta	Anibal	Oak Ridge National Laboratory
Ramsden	Todd	National Renewable Energy Laboratory
Randolph	Katie	U.S. Department of Energy
Ranisau	Jonathan	University of Waterloo
Rao	Prabhu	McPhy Energy NA
Rasimick	Brian	Giner, Inc.
Reddi	Krishna	Argonne National Laboratory
Rege	Julia	Association of Global Automakers
Ren	Fei	Temple University
Ren	Xiaoming	Army Research Laboratory
Renner	Julie	Proton OnSite
Renz	Tobias	Tobias Renz FAIR
Resende	William	BMW
Rice	Brian	University of Dayton Research Institute
Richards	Mark	FuelCell Energy, Inc.
Rinebold	Joel	Connecticut Center for Advanced Technology, Inc.
Rinker	Mike	Pacific Northwest National Laboratory
Ritzert	Nicole	National Institute of Standards and Technology
Rivkin	Carl	National Renewable Energy Laboratory
Robinson	David	Birch Studio
Rockward	Tommy	Los Alamos National Laboratory
Rodrigues	Aida	General Motors

Last Name	First Name	Organization
Rodriguez	Jose	Brookhaven National Laboratory
Rohatgi	Aashish	Pacific Northwest National Laboratory
Rollins	Selisa	U.S. Department of Energy
Ronevich	Joseph	Sandia National Laboratories
Rosner	Robert	University of Chicago
Roychoudhury	Subir	Precision Combustion, Inc.
Ruan	Hanson	Blue-O Technology, Inc.
Rufael	Tecle	Chevron
Ruiz	Antonio	U.S. Department of Energy
Rupnowski	Peter	National Renewable Energy Laboratory
Rustagi	Neha	U.S. Department of Energy
Ruth	Mark	National Renewable Energy Laboratory
Ryu	Jaeyune	Korea Institute of Science and Technology
Saito	Nobuhiro	Honda
Sakurahara	Kazuo	Honda
San Marchi	Christopher	Sandia National Laboratories
Sandrock	Gary	Consultant
Sasaki	Kotaro	Brookhaven National Laboratory
Sasakura	Masaharu	The Institute of Applied Energy
Sassin	Megan	U.S. Naval Research Laboratory
Satyapal	Sunita	U.S. Department of Energy
Saxena	Ashok	Wiretough Cylinders LLC
Schneider	Jesse	BMW
Schneider	Martin	Hydrogenious Technologies
Schuegraf	Klaus	PathTwo
Schumacher	Christian	Naval Undersea Warfare Center
Schwartz	Hart	Clarify Consulting & Research, LLC
Schwartz	Viviane	U.S. Department of Energy
Semelsberger	Troy	Los Alamos National Laboratory
Serfass	Jeff	Technology Transition Corporation
Serov	Alexey	University of New Mexico
Serre-Combe	Pierre	CEA (Alternative Energies and Atomic Energy Commission [France])
Setzler	Brian	University of Delaware
Shade	Willard	ACI Services Inc.
Shao	Minhua	Hong Kong University of Science and Technology
Shimanuki	Hiroshi	Honda Motor Co., Ltd.
Shimizu	Takahiro	Japan Automobile Research Institute
Shirvanian	Pezhman	Tennessee Tech University
Siegel	Kay Kimberly	H2Safe, LLC
Sievers	Robert	Teledyne Energy Systems
Simon	Aaron	Lawrence Livermore National Laboratory
Singhal	Subhash	Consultant

Last Name	First Name	Organization
Skolnik	Ed	Energetics Incorporated
Smith	Barton	Oak Ridge National Laboratory
Smith	Owen	National Renewable Energy Laboratory
Smith	Phillip	NASA
Snyder	Joshua	Drexel University
Soaper	Kimberly	BCS, Inc.
Sofronis	Petros	University of Illinois
Soloveichik	Grigorii	Advanced Research Projects Agency–Energy (ARPA-E)
Song	Huiyu	University of Maryland
Song	Jianwei	University of Maryland
Sookhoo	Ryan	Hydrogenics Corporation
Soto	Herie	Shell Oil Company
Spear	Chris	Hyundai
Spendelow	Jacob	Los Alamos National Laboratory
Spitsen	Paul	U.S. Department of Energy
Spitzer	Kevin	Air Force Research Laboratory
Sprik	Sam	National Renewable Energy Laboratory
Stamenkovic	Vojislav	Argonne National Laboratory
Stavila	Vitalie	Sandia National Laboratories
Steinbach	Andrew	3M Company
Steiner	Dietmar	Robert Bosch LLC
Stephenson	Thomas	Pajarito Powder, LLC
Stetson	Ned	U.S. Department of Energy
Stevens	John	U.S. Department of Energy
Stewart	Eric	RCF Economic & Financial Consulting
Stottler	Gary	General Motors
St-Pierre	Jean	Hawaii Natural Energy Institute
Stroebel	Raimund	Dana Corporation
Stuckey	Philip	U.S. Patent and Trademark Office
Studer	Sarah	Oak Ridge Institute for Science and Education Fellowship
Suchsland	Jens-Peter	Greenerity GmbH
Suda	Kazuhiro	Honda
Sulic	Martin	Savannah River Consulting
Summers	William	Savannah River National Laboratory
Sun	Yugang	Temple University
Sung Jong	Yoo	Korea Institute of Science and Technology
Sunkara	Mahendra	University of Louisville
Suzuki	Hideki	Honda
Suzuki	Shingo	Tanaka Kikinzoku International (America), Inc.
Swartz	Scott	Nexceris, LLC
Swider-Lyons	Karen	Naval Research Laboratory
Szoucsek	Klaus	BMW

Last Name	First Name	Organization
Tamburello	David	Savannah River National Laboratory
Tamhankar	Satish	Independent
Tanaka	Yukihito	Honda
TANG	Wan Si	National Institute of Science and Technology
Tang	Zhijiang	Oak Ridge National Laboratory
Tao	Ye	University of Maryland
Tartaglia	Katie	Energetics Incorporated
Tarver	Jacob	National Renewable Energy Laboratory
Tchouvelev	Andrei	A.V. Tchouvelev & Associates Inc.
Teprovich	Joseph	Savannah River National Laboratory
Terlip	Danny	National Renewable Energy Laboratory
Tew	David	United Technologies Research Center
Thomas	C. E. (Sandy)	
Thompson	Mark	United Technologies Research Center
Timbario	Thomas J.	Alliance Technical Services, Inc.
Toelle	Sascha	Umicore
Toughiry	Mark	U.S. Department of Transportation
Trainor	Aaron	Zeton Inc.
Tran	Dat	Army Research Laboratory
Tran	Thanh	Naval Surface Warfare Center
Trejos	Vanessa	CSRA
Trocciola	John	SRA International
Tsai	Li-Duan	Industrial Technology Research Institute
Tsimis	Dionisis	Fuel Cells and Hydrogen Joint Undertaking (FCH JU)
Tsutsumi	Shuntaro	Toyota
Turner	John	National Renewable Energy Laboratory
Udovic	Terrence	National Institute of Standards and Technology
Ullman	Sondra	GE Global Research
Ulsh	Michael	National Renewable Energy Laboratory
Ulvestad	Andrew	Argonne National Laboratory
Vaipan	Alex	Hexagon Lincoln
Valdez	Thomas I.	Jet Propulsion Laboratory
Vanderborgh	Nicholas	Consultant
Varhue	Walter	University of Vermont
Veenstra	Mike	Ford Motor Company
Vela	Adriana	Universidad de Guanajuato
Verduzco	Laura	Chevron
Virkar	Anil	University of Utah
Vogel	John	Combined Energies
Vold	Jennifer	Access Interpreting
Vu	Cung	Vu Consulting LLC
Vukmirovic	Miomir	Brookhaven National Laboratory

Last Name	First Name	Organization
Wach	Steve	Savannah River National Laboratory
Wade	Matthew	Virginia Clean Cities
Wagner	Emanuel	Hydrogen Education Foundation
Wagner	Frederick	General Motors
Waldecker	James	Ford Motor Company
Wallner	Thomas	Argonne National Laboratory
Wan	Jiayu	University of Maryland
Wang	Chao	Johns Hopkins University
Wang	Conghua	TreadStone Technologies, Inc.
Wang	Michael	Argonne National Laboratory
Wang	Wei	Pacific Northwest National Laboratory
Wang	Xiaohua	Argonne National Laboratory
Wang	Yanyan	Georgetown University
Ward	Patrick	Savannah River National Laboratory
Warford	Bruce	Oak Ridge Associated Universities
Watanabe	Ryoji	Sumitomo Corporation
Weber	Adam	Lawrence Berkeley National Laboratory
Weeks	Brian	Gas Technology Institute
Wei	Max	Lawrence Berkeley National Laboratory
Weimer	Alan	University of Colorado
Weiner	Steven	Hydrogen Safety Panel
Wen	Fei	University of Michigan
Weng	Baicheng	University of Toledo
Werth	Chris	U.S. Department of Energy
West	Todd	Sandia National Laboratories
Westlake	Brittany	Electric Power Research Institute
Wheeler	Douglas	DJW Technology, LLC
Williams	Mark	AECOM
Wilson	Adria	U.S. Department of Energy
Wimmer	Robert	Toyota Motor North America
Wipke	Keith	National Renewable Energy Laboratory
Wissink	Han Kyung	Independent
Witte	Thomas	Pacific Northwest National Laboratory
Wong	Joe	U.S. Department of State
Wood	Brandon	Lawrence Livermore National Laboratory
Woods	Stephen	NASA
Wu	Zili	Oak Ridge National Laboratory
Xie	Jian	Indiana University–Purdue University Indianapolis
Xin	Huolin	Brookhaven National Laboratory
Xu	Bingjun	University of Delaware
Xu	Fenghua	University of Toledo
Xue	Jisan	U.S. Department of Transportation

Last Name	First Name	Organization
Yamada	Takahiro	University of Dayton Research Institute
Yamanashi	Fuminori	Research Association of Hydrogen Supply/Utilization Technology (HySUT)
Yan	Yushan	University of Delaware
Yandrasits	Michael	3M Company
Yang	Taehyun	Korea Institute of Energy Technology Evaluation and Planning (KETEP)
Yang	Zhiwei	United Technologies Research Center
Yang Neyerlin	Ami	Colorado School of Mines
Yauch	Brandon	Electricore, Inc.
Ye	Siyu	Ballard Power Systems
Yeh	Chiao-Wen	Epistar
Yoo	Sung Jong	Korea Institute of Science and Technology
Yoshida	Toshihiko	Tokyo Institute of Technology
Yoshida	Yuichi	Department of the New Energy and Industrial Technology Development Organization (NEDO)
Yousfi Steiner	Nadia	Labex Action
Yu	Siyoung	University of Connecticut
Yuzugullu	Elvin	CSRA
Zalis	Walter	Energetics Incorporated
Zelenay	Piotr	Los Alamos National Laboratory
Zeng	Xiangqun	Oakland University
Zenyuk	Iryna	Tufts University
Zhang	Changlin	University of Akron
Zhang	Cuijuan	University of South Carolina
Zhang	Feng-Yuan	University of Tennessee
Zhang	Shuo	University of Maryland
Zhang	Xiaoyu	Old Dominion University
Zhang	Y-H Percival	Virginia Tech
Zhang	Ying	University of Maryland
Zheng	Jinyang	Zhejiang University
Zheng	Rongfeng	Georgetown University
Zhou	Hong-cai Joe	Texas A&M University
Zhou	Xiangyang	University of Miami
Zhu	Kai	National Renewable Energy Laboratory
Zidan	Ragaiy	Savannah River National Laboratory
Zimmerman	Jonathan	Sandia National Laboratories
Zou	Shouzhong	American University

Program Comments Provided by Reviewers

Hydrogen Production & Delivery Program Comments

1. Was the program, including overall strategy, adequately covered?

- The program was covered thoroughly, including hydrogen cost status and targets; and the research, development, and development (RD&D) strategies and framework, which addressed the leveraging of resources among stakeholders. The production of renewable hydrogen and delivery of hydrogen was shown to have been analyzed from a techno-economic perspective. Lastly, several advances in research and development (R&D) were presented, e.g., on platinum-group-metal (PGM)-free anion-exchange membrane (AEM) electrolysis, magnetocalorics in hydrogen liquefaction, low-carbon hydrogen production, and joint efforts between the U.S. Department of Energy (DOE) and National Science Foundation (NSF) on photoelectrochemical (PEC) and solar thermochemical (STCH) production.
- This program was very well described by the presenter. The technical challenges, the barriers to implementation, the very broad range of technical approaches that are involved, etc. were very clearly and concisely presented. This appears to be an excellent program, well managed and well executed.
- The strategy of the Hydrogen Production and Delivery program was clearly presented and very well defined. It has a very clear focus on what the near- and long-term challenges are and a very strong and balanced portfolio and excellent resources to address these challenges.
- Yes, the roadmap provided a good overview, and it indicated how far technologies are from commercialization based on technology readiness levels (TRLs).
- Relevant topics seem to be covered.
- Yes.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- One of the strongest attributes of the Hydrogen Production and Delivery program is its clearly defined portfolio to address the near- and long-term challenges. For the production projects, there is an excellent portfolio to cover the durability and efficiency challenges to enable hydrogen production from renewable sources. For the delivery projects, there is a very strong portfolio to address the near-term challenges on the infrastructure needs, which includes reliability improvements and cost reduction of hydrogen refueling station components. For the long-term challenges, the program is strongly focused on key areas: liquid hydrogen technologies, advanced compression options, and issues on pipeline transport.
- The many short-term R&D needs in areas such as fueling stations, delivery hoses, codes and standards, sensors, and systems analysis were well balanced against a longer-term portfolio of hydrogen production approaches and more medium-term issues, such as renewables integration and reforming. This area by its nature must contain a very broad array of timeframes and technical approaches, but it remains a well-balanced portfolio.
- The program has a balanced R&D portfolio, both on hydrogen production and hydrogen delivery vis-à-vis near- and long-term objectives. This balance can be seen clearly on slide 9. In fact, the program should be commended for the thoughtful distribution of resources, e.g., on electrolysis, PEC, and STCH.
- Yes, there is reasonable balance within the Hydrogen Production and Delivery program, although it is clear most of the hydrogen production effort is long-term since renewable hydrogen approaches are still at an early TRL of development; hydrogen delivery and dispensing technologies are relatively near-term.
    - That said, given that renewable and affordable hydrogen is a cornerstone to the ultimate success for fuel cell electric vehicle (FCEV) commercialization, there seems to be a big and clear gap in effort level and budget allocated to hydrogen production research compared to other sub-programs, including those in the Vehicle Technologies Office. In light of this, the 16% budget reduction requested for 2017 for the program is even more puzzling.
    - The program ought to advocate for proportionally bigger effort for renewable hydrogen production R&D, either for accelerating incremental improvements or for advancing breakthrough developments. Short of that, the sub-program’s stated goals of developing renewable hydrogen will seem shallow.

- Yes. However, it is not clear how the FCEV original equipment manufacturer 700 bar path chosen at a global level affects choices made for R&D in different timeframes.
- There are more projects on mid- and long-term R&D. Although two of the high-priority items are reducing costs of FCEV refueling stations and renewable production pathways, there do not appear to be any industry-driven projects to do so, e.g., industrialization of polymer electrolyte membrane (PEM)-electrolysis or refueling station technology development.

3. Were important issues and challenges identified?

- The program management clearly addressed the main issues and challenges, and evidence of this is the broad portfolio for both the production and delivery areas to address the key areas to enable low-cost clean hydrogen production and delivery to meet Fuel Cell Technologies Office (FCTO) cost targets.
- Issues related to renewable hydrogen production and hydrogen delivery and dispensing costs were clearly delineated. The various steps from analysis to stakeholder input to R&D portfolio and related priorities and targets were all presented in a lucid way.
- Yes, in addition to cost, the many diverse technical challenges across this broad portfolio were addressed.
- Yes, meeting the cost targets was an important challenge.
- The nationwide rollout in the United States in an important issue. It is not clear what the achievements of H_2USA are so far.
- Yes, only it is not clear how the 700 bar path affects R&D (i.e., creates issues and challenges).

4. Are plans identified for addressing issues and challenges?

- Yes, both near- and long-term issues and strategies are identified. The near-term plans are primarily focused on hydrogen delivery, while the long-term plans are focused on renewable hydrogen production. However, although not explicitly stated, fossil-based hydrogen sources are assumed for near-term plans.
- A techno-economic analysis of delivery and production costs was presented, and the various cost-influencing parameters were assessed and accounted for. Plans for RD&D were stated on the strategy slide (slide 8) along with the national laboratory support framework.
- The plans were clearly identified as presented on the Applied RD&D Portfolio Development slide.
- In general, the program and its projects are well focused on addressing the key technical challenges.
- Yes, by focusing on funding in different areas, the plans address the issues and challenges.
- Although two of the high-priority items are reducing cost of FCEV refueling stations and renewable production pathways, there do not appear to be any industry-driven projects to do so, e.g., industrialization of PEM electrolysis or refueling station technology development. Slide 7 does not show any industry RD&D.

5. Was progress clearly benchmarked against the previous year?

- There is excellent progress and accomplishments in both production and delivery areas. Some of the main highlights include the Hydrogen Station Equipment Performance (HySTEP) device, the world's first demonstration of gas liquefaction using magnetocaloric materials, the demonstration of PGM-free AEM electrolysis, and the significant progress on PEC and STCH pathways, which are key enablers for renewable hydrogen production.
- A number of advances were presented, such as hydrogen production for bio-gas and fuel cells operating in electrolysis mode. For instance, in the case of fermentation and microbial electrolysis cell (MEC) production, a rate of increase greater than 85% was reported in comparison to 2015. A similar percentage increase was reported in the case of liquefaction through magnetocalorics. Most important, there was the example of the cascading pressure receiver by Sandia National Laboratories (SNL) reactor (CPR2) whereby the concept was moved to demonstration in one year.
- All FCTO sub-programs are very well benchmarked against prior years. This program is no different.
- Yes, hydrogen cost per kilogram and accomplishments of different projects (liquefaction, non-PGM electrolyzer stable operation, H2 Refuel H-Prize, HyStEP, etc.) were clearly benchmarked.
- Although implied, the benchmarking of accomplishment timing was not clear. It was hard to tell what was done this year or last year.

- No, the presentation does not give a benchmark to last year.

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, and the projects are addressing the problems in a balanced way among the alternative technologies. An example is the ultra-high-current, high-temperature solid oxide electrolysis cells and stack. The operation moved from the cell stage to the stack stage.
- The program is dedicating significant efforts and resources to overcome some of the main barriers of the FCTO, which include the cost reduction of hydrogen refueling station components and advancing renewable hydrogen production.
- Yes, the broad problems and barriers are definitely being addressed.
- Yes, they are, especially considering the overall limited funding.
- Yes, this is clear from the projects and the program overview.
- There do not appear to be industry R&D projects that address the high-priority items.

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- This program is very well managed and has quite a large management team that is commensurate with the breadth and diversity of the project areas. This appears to result in uniformly good projects that are addressing the key specific challenges they are resolved to solve.
- The key to the success of this program is mainly attributed to the outstanding management and the team around it.
- Yes, and the program should eventually focus on two or three renewable paths for further RD&D activities. The available analysis results should enable this.
- Given the resources, the program is well managed. However, the allocated budget for hydrogen production projects is insufficient to meet the long-term objectives of securing large-scale renewable hydrogen for less than $2/kg.
- Yes (two responses).

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- The projects on the positive end of the spectrum are the magnetocaloric liquefaction project (Pacific Northwest National Laboratory), non-PGM stable operation project (Proton Onsite), and the reformer/electrolyzer/purifier project (Fuel Cell Energy). No projects are on the negative end of spectrum.
- A key strength is the balanced portfolio on production and delivery involving short- and long-term efforts. The SNL STCH and hydrogen materials compatibility efforts do stand out and constitute assets for the program. Lack of fundamental science seems to be a weakness. Good engineering sets the device parameters in concert, but fundamental science, such as surface chemistry and catalysis, is the way to achieve order-of-magnitude increases in efficiency.
- The strengths of the program are the management and supporting team, clear focus on the main challenges, approach to address these challenges, and excellent interaction with external stakeholders, both domestic and international.
- A key strength is the diversity of approaches being actively managed and rebalanced.
- A very in-depth analysis is being performed as a strong basis for deriving RD&D priorities, but it is not clear that these results are appropriately shared with industry.
  The program has its strength in the quality of its short-term projects, while its long-term projects are generally weak and less focused.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- The program is definitely taking an innovative approach on some of the projects to include significant efforts on materials research for renewable hydrogen production, advanced compression technologies, advanced hydrogen liquefaction technologies, and the ongoing efforts on the HydroGEN initiative.
- Renewable hydrogen is key to the hydrogen-powered society, and the program addresses this challenge through both mid-term (electrolysis, biomass pathways) and long-term solutions (PEC, STCH). The program is similarly well balanced in its strategies for overcoming the delivery barriers.
- Yes, there appear to be innovative approaches in the solar thermal area as far as reactor design. It is too soon to determine whether these innovations will pan out, but there are multiple examples of newly innovative R&D ongoing across the program.
- Yes, for the most part the projects represent novel and/or innovative approaches.
- Yes.

10. Has the program engaged appropriate partners?

- Yes, the program has a wide range of collaboration among academia, national laboratories, and industry. Both the hydrogen production and hydrogen delivery technical teams in the U.S. DRIVE Partnership have decent representation from industry. In addition, the program has regular collaboration and workshops with other U.S. DRIVE technical teams, such as the Fuel Pathways Integration Tech Team and the Hydrogen Storage Tech Team.
- The program has impressive collaborative projects among national laboratories and interagency interactions, e.g., a joint funding approach with NSF.
- The program is receiving significant feedback from industry partners and international stakeholders in addition to excellent intra- and inter-agency collaborations.
- Without a doubt, the hallmark of all FCTO programs is collaboration and teaming with the right partners. This program is no different.
- It is questionable whether the industry link is appropriate. It is not clear whether all relevant players are present in H_2USA and U.S. DRIVE. It is not clear whether industry is really doing its job or whether it relies on laboratories.
- Yes.

11. Is the program collaborating with them effectively?

- Collaboration is outstanding.
- Yes, especially with the other hydrogen technology teams.
- Yes, it appears so from the overview of collaborations.
- This could not be determined from the presentation. Apparently, collaboration details could not be given because of time limitations.

12. Are there any gaps in the portfolio for this technology area?

- If there are gaps, they are gaps in how the portfolio is balanced, but this is a moving target, and funding opportunity announcements tend to make the "right balance," whatever that is, ebb and flow. Hence, the gaps in the portfolio are being adequately managed.
- It is not clear how fundamental science is integrated in the program. Apart from the reference to joint funding with NSF on slide 18, the university community's contributions to the program were not referenced.
- The cost trajectories for renewable hydrogen for the last few years appear to be flat and stalled. Perhaps this is a sign to look for breakthrough technologies.
- There is a gap regarding the effects of global choice for 700 bar for light-duty FCEVs.

13. Are there topics that are not being adequately addressed?

- The effect of hydrogen quality requirements on production cost per kilogram of hydrogen is not being adequately addressed. Inclusion of the right-of-way cost for hydrogen pipeline implementation in urban areas is also not addressed.
- Perhaps the program should address fundamental science to support engineering.
- The program is very robust and well managed, and it is extremely focused on addressing the main challenges and barriers.

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- Larger-capacity renewable hydrogen production projects (beyond 100–200 kg/day) at 500–1,000 kg/day with inclusion of delivery method (not large-scale, such as the capacity of conventional centralized steam methane reforming) should be considered.
- Perhaps the program should consider compressors. It is not clear what the state is of this important component of refueling stations.

15. Can you recommend new ways to approach the barriers addressed by this program?

- A key issue in the area of hydrogen delivery and infrastructure (e.g., materials for compressor technology) is hydrogen-accelerated fatigue of metals and alloys. SNL is successfully driving the codes and standards efforts, but fatigue is an issue that is still unresolved. Further, there are no mitigation strategies, and fatigue could be responsible for potential failure scenarios in the future. A joint program with NSF or DOE/Basic Energy Sciences is indicated.
- The program should expand the H2 Refuel H-Prize award funding to stimulate market players to think out of the box and do something when funded—also because Advanced Research Projects Agency–Energy (ARPA-E) does not cover hydrogen production very well. The project should more closely explore and assess companies and projects that are funded internationally in the European Union and Japan in topical areas of production and delivery.
- The program should have more industry-driven RD&D projects.
- The program should explicitly declare that it will be impossible to meet the DOE production targets with renewable hydrogen for many years. That way, policymakers will be well informed about the dependence on fossil hydrogen for the near term and the need for more investment to make renewable hydrogen affordable.

16. Are there any other suggestions to improve the effectiveness of this program?

- The program should include the university community on fundamental science issues, e.g., in the area of fatigue or the areas of surface science and catalysis for hydrogen production. In summary, the program manager has done an excellent job in shaping the program into one that steadily advances toward the targets with an optimum allocation of resources serving short- and long-term goals.
- The program should provide clearer definitions for “short term,” “medium term,” and “long term.” Approximating timeframe helps with perspective, e.g., short term is 2020, medium term is 2030, and long term is 2050 (with the understanding that uncertainty increases in the longer term). The program should improve the productivity of international collaborations by exchanging information or setting up shared projects at a lower level (actual RD&D level), not only at a high overview level.
- This program needs to make a strong case that the upstream challenges associated with renewable hydrogen production are very significant and that much more upfront investment will be needed to meet the desired cost and other targets.

Hydrogen Storage Program Comments

1. Was the program, including overall strategy, adequately covered?

- In 2016, this program continues to focus on achieving improvements in the storage of hydrogen for onboard automotive applications. The overall strategy is to address a range of short-to-long-term technical strategies to achieve improvements in cost, capacity, balance-of-plant issues (mass, weight, and cost), and overall energy efficiency, among others. The short-term strategy to reduce costs for physical storage of hydrogen at high pressure within tanks focuses on reducing the cost and mass of the carbon fiber structural reinforcements, whereas the longer-term strategy continues to focus on materials-based research and development (R&D) to develop lower-pressure, higher-capacity systems relative to storage in tanks. The sub-program's R&D portfolio adequately reflected the various emphases on storage approaches and short-to-longer-term strategies for achieving U.S. Department of Energy (DOE) storage system targets. A new piece of the overall strategy was introduced this year, that being the Hydrogen Materials–Advanced Research Consortium (HyMARC), which takes advantage of all the prior learning from the three previous materials centers and the engineering center, and asks the appropriate questions about how to solve the extremely difficult materials storage problem. This approach has some risk associated with it, but the benefits could be large.
- The program has addressed the shortcomings in existing storage solutions and has devised strategies for attacking them. The slide showing the current status of technology was clear and concise. The program has addressed the shortcomings in existing storage solutions and has devised strategies for attacking them.
- Yes, a spectrum of activities is well covered in the program: compressed gas, materials-based hydrides and sorbents, and fundamental research and analysis. They include near-to-long-term activities. Support organizations include national laboratories, universities, industry, and other government agencies.
- Yes, the Hydrogen Storage program was explained very well in terms of overall strategy and highlights from the portfolio.
- The program was well covered.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- The balance between shorter-term R&D that addresses physical storage of hydrogen in tanks and approaches to achieving overall cost and mass reductions of physical storage is well balanced with other medium-to-longer-term strategies that are addressing the very difficult problem of materials-based hydrogen storage.
- Yes, compressed gas is near-term, storage materials are mid-to-long-term, and fundamental research efforts (HyMARC) are aimed at improving the progress in long-term materials development.
- Yes, the hydrogen storage portfolio appears to have a good mix of near-, mid-, and long-term research.
- There still seems to be more emphasis on short-term developments; however, compared to previous years, there are improvements regarding increasing emphasis on long-term research (i.e., the HyMARC launch).
- The long-term approach seems to be focused on materials-based storage. Perhaps there should be more long-term efforts addressing physical storage. It is not clear whether cryo-compressed is mid-term or long-term. Materials-based storage research continues to disappoint, yet it is receiving the lion's share of funding. Given the lack of promising results, it is not clear whether the emphasis on materials storage needs to be scaled back. It seems like the research is being driven more by the talents and capabilities of the national laboratories than by the experimental results.

3. Were important issues and challenges identified?

- This program continues to be very focused on addressing all of the key barriers to successful implementation of viable onboard hydrogen storage systems.
- Yes, the program has clear numerical targets for weight, volume, temperature, cost, etc. Most important, there are different targets for onboard light vehicle storage, materials-handling equipment, portable power, and stationary storage.
- Yes, the critical barriers were explained along with the strategy.

- Challenges were identified and briefly explained.
- Yes, continued focus on carbon fiber costs is an example.

4. Are plans identified for addressing issues and challenges?

- Plans for addressing the challenges for hydrogen storage going forward were well described. The new HyMARC approach to materials-based storage is relatively new and in its formative stages. Challenges there are numerous, both organizationally and technically, to develop this very computational-centric approach into a productive enterprise.
- The program has put in place several plans to tackle the challenges associated with short- and long-term R&D.
- Most of the barriers are being addressed, although it would be helpful to provide a clear linkage or matrix that identifies the projects in the portfolio and their focus on a certain barrier. This matrix may identify some challenges that are not currently being addressed in the current portfolio.
- Yes.

5. Was progress clearly benchmarked against the previous year?

- This program continues to do an excellent job of benchmarking progress among its various R&D efforts and also indicating technical areas where more progress needs to occur.
- There was clear benchmarking for appropriate projects. Yes, PAN/MA and glass fiber accomplishments were clearly described, but it would have been nice to have seen the potential impact of those developments on tank costs. More information on prototype systems would have been useful. It is not clear whether the systems met the predicted performance.
- Yes, selected advances made during the last year were listed.
- The progress of each project was highlighted, although the progression of reducing the gaps was not specifically identified from the previous year. It would be useful to identify the progression of some key targets (e.g., cost) or indicate the theoretical potential for advancement with the progress of projects in the portfolio.
- Developments have been explained for this year; however, the progresses were not clearly benchmarked compared to the previous year.

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 projects address the barriers quite closely in most cases. The creation of extensive basic science activities under the new national-laboratory-based HyMARC is especially welcome to provide future ammunition to attack the formidable FCTO storage barriers.
- Yes, the Hydrogen Storage program is an important enabler for addressing the broad problems and barriers that the FCTO is trying to solve.
- The projects appropriately addressed the barriers of the FCTO.
- This program of the FCTO continues to select and fund projects that have promise to address the technical challenges of hydrogen storage for onboard automotive applications. There are a few projects that are in need of some direction either because of technical challenges or because they are very new to working in this particularly target-focused R&D environment.

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes, the Hydrogen Storage program is very well managed. The program managers provide exceptional leadership for a broad portfolio of projects in the pursuit of advancing hydrogen storage for fuel cell electric vehicles. They ensure the research is relevant and focused on the needs of industry.
- The program is well focused, well managed, and continues to be viewed as a world-leading force in hydrogen storage technology.

- Over the years, the program has zeroed in on the critical issues for physical storage and has developed projects to address them.
- The program is well managed and effective.
- Yes.

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- The sub-program's real strength is the way it has built on its previous experience and identified critical issues, and then developed clearly targeted projects to address them.
- Key strengths of the projects are that most are well focused on the key barriers and largely have logical approaches to addressing the challenges. The "analysis" projects are always very informative and help the R&D community as well as DOE to better focus its resources. The Lawrence Livermore National Laboratory project in magnesium borohydride is quite well focused. Areas that are lagging include the laboratory-led effort in alane and two relatively new projects at Ames and Caltech. Otherwise, projects are uniformly moving forward with approaches that are reasonable in achieving their technical goals.
- The strengths of this program are the diverse spectrum of technologies that would be difficult for industry to develop in a comprehensive manner. The weakness of the projects in this program is the lack of commercialization. There should be a greater emphasis on developing technologies that have partners with the intent of implementing the technology in products.
- The key strength is the variety of storage approaches being taken, from conceptually simple compressed gas to more esoteric chemical, hydride, and physisorption materials. The key weakness is the present state of the art in storage materials that makes meeting quantitative storage goals very difficult, if not impossible.
- No projects stand out on either end of the spectrum.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- There is novelty in the conformable tanks project; the new computational focus in HyMARC, while difficult, would be considered very innovative if successful. Time will tell. There is new and innovative work in the characterization efforts, and the newer results from Lawrence Berkeley National Laboratory on adsorbing greater than one hydrogen per metal site is very innovative chemistry; its impact on storage is to be determined.
- Yes, most of the projects are novel ideas with a good balance between risk and rewards.
- Yes, in general the projects represent novel approaches. Some seem practically very complex and unlikely to succeed commercially.
- It was difficult to discern how innovative the approaches are, since there was not a lot of detail on the project approaches in the overview presentation.

10. Has the program engaged appropriate partners?

- There are an extraordinary number of good partners within national laboratories, universities, industry, consultancies, and other government agencies.
- This program continues to demonstrate the value of cross-project collaboration to move the field of hydrogen storage forward; this is what makes the DOE Hydrogen and Fuel Cells Program (the Program) recognized as the world-leading program. This is largely because the management of the Program has always rewarded excellent collaboration, and the participants have risen to the challenge.
- The program has encouraged and enabled collaboration among national laboratories.
- Yes, the program has engaged the key researchers and industry partners to advance the technology. A recommendation is to develop further partners with a focus on commercialization. The program has recently increased the involvement of national laboratories without a strong engagement of implementation partners.
- Appropriate partners are engaged.

11. Is the program collaborating with them effectively?

- There appears to be a continuing high level of communication and collaboration with the program management and the technical community involved in storage.
- Yes, the program is effectively collaborating with partners in the development of hydrogen storage systems. The program managers encourage and facilitate collaboration within the program.
- The program is collaborating effectively with the partners.
- Yes, program collaboration clearly is effective.
- The number of consortia in the program may be growing too fast.

12. Are there any gaps in the portfolio for this technology area?

- No, there are no gaps now that HyMARC has been established to address some critical fundamental science in storage materials.
- There are no gaps in the portfolio of the program.
- Most of the work seems focused on short- or long-range. More mid-term work might be appropriate.
- Yes, there are some gaps in the portfolio for this technology area. This could be apparent with a cross-comparison of the technology barriers and the project portfolio. An example is the fact that the materials cost for the materials-based storage is not being considered in the efforts.

13. Are there topics that are not being adequately addressed?

- While some topics may be underrepresented or overrepresented from time to time, this is a function of how often and how large funding opportunity announcements (FOAs) are that allow technical gaps to be smoothed over time. The program now appears to have a good long-term trajectory to be able to respond to new opportunity areas and to rebalance if necessary.
- There appear to be none.
- Much of the work in production and delivery points to liquid delivery as a more viable option than gaseous delivery. Strategies such as cold-compressed or cryo-compressed need more emphasis.
- Yes, there should be a greater emphasis on the strategic approach of these various storage technologies in terms of system cost analysis and the value proposition to the customer along with the infrastructure modifications required to support the various technologies.
- The new materials effort, HyMARC, was not adequately addressed. It was unclear how the consortium would leverage the R&D of hydrogen storage materials beyond what has been already done before, i.e., a materials center of excellence and independent projects.

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- This is a good, comprehensive program to address the key challenges in onboard hydrogen storage.
- The program should consider novel materials systems for hydrogen storage.
  There should perhaps be more work on low-cost, large-scale stationary storage that will be needed for the newly proposed “Hydrogen at Scale” project.
- The program should consider funding strategic studies regarding the value equations for certain technologies to consider the viability in order to focus resources on the potential options with the probability of achieving a commercial product.

15. Can you recommend new ways to approach the barriers addressed by this program?

- The approach of studying all of the hydrogen storage technologies may need to be reconsidered based on an understanding of the quantified gaps and key enablers for certain storage approaches and then a focus on the technologies with the viable path to reduce the gaps. The program should start developing a set of filters based on the reverse engineering results from the Hydrogen Storage Engineering Center of Excellence (HSECoE) and Argonne National Laboratory (ANL).
- There should be more funding for physical storage because it seems to be the default option at this point. Reduce efforts on materials-based storage until a promising approach emerges.
- No.

16. Are there any other suggestions to improve the effectiveness of this program?

- One suggestion would be to include/attract more new materials concepts leveraged by the HyMARC analytical and computational abilities.
- One characteristic of the overall effort that has changed is that there are far fewer R&D members who come from chemical sciences backgrounds, and so the community that used to bring a good deal of chemical expertise and intuition has eroded over the last several years. For the materials-based approaches, bond-forming and bond-breaking events are required in the final analysis, and this being the domain of chemical scientists, there could be a net benefit to the enduring program to re-engage with more reaction chemists to bring some of that expertise and intuition in reacting materials back into the program.
- Coordination among the many HyMARC partners and associates is very important for the effort to succeed without significant overlap and duplications of effort. Congress and many other stakeholders will be critically watching this expensive new consortium, much like the observations made of the recently ended HSECoE. Dissemination of the results of HyMARC to the next generation of scientists and engineers who must reach the goals and targets of the FCTO Multi-Year Research, Development, and Demonstration Plan is of primary importance.
- The HyMARC and Characterization and Validation Team initiatives to study the fundamentals are useful, although their effort is not very well aligned with the barriers. Also, the fundamentals have been studied in the past, and the HyMARC team should emphasize the novel aspects of its approach. The results from the HSECoE and ANL regarding the reverse engineering for the required materials properties to meet the system targets should be utilized and highlighted in all of the projects related to materials-based storage. This work was important and does not seem to be acknowledged within the program for directing the future work of HyMARC and the Characterization and Validation Team along with other projects in the portfolio.

Fuel Cells Program Comments

1. Was the program, including overall strategy, adequately covered?

- The program was defined clearly, and the overall strategy for the objectives was covered in sufficient detail.
- Yes, the strategy was well covered, and the use of other sub-programs' analyses to guide priorities for this program is a notable highlight.
- The program and overall research strategy were adequately covered. The research approach to address the barriers of cost, durability, and performance has been adequately discussed.
- Yes, it was adequately covered in combination with the FCTO Office Director's talk that filled in the broader context.
- The overview covered all salient aspects.
- The program presentation described the overall strategy. However, the strategy is currently unclear. The efforts should focus on solving known problems. It looks like the broad vision and corresponding strategy are not well established or thought through.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- Yes, particularly when additional fuel-cell-related programs under the Office of Basic Energy Sciences (BES), Advanced Research Projects Agency–Energy (ARPA-E), and the Vehicle Technologies Office are taken into account. The Office of Energy Efficiency and Renewable Energy (EERE) covers a very useful range of pre-competitive research and development, from early-stage catalyst development at the milligram scale to demonstrations with complete fuel cells. Even the most fundamental of EERE projects has a tighter focus on the requirements of practical fuel cell technologies than do fuel cell-related projects funded by other agencies. BES projects pursue fundamental knowledge that could advance the field but are unlikely to affect the next generation of technology development. ARPA-E pursues fringe ideas that have a low probability of working in a practical context but that arguably could cause a major disruption of the field if successful. Taken as a whole, the U.S. Department of Energy (DOE) properly covers a broad spectrum of fuel-cell-related activities. EERE occupies the sweet spot in the center, with enough innovations to stimulate change and enough attention to technical realities that developments within the EERE have a good probability of being adopted by fuel cell developers, thereby stimulating the U.S. economy.
- There is appropriate balance between near- and mid-term goals discussed in the Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan. Long-term development may ultimately be fleet-vehicle, or consumer-vehicle, driven. It is not clear at this stage how fuel cell electric vehicles (FCEVs) will transition from test markets to broader use once cost and durability have been addressed.
- The full gamut was addressed.
- Yes.
- The funded projects seem to be oscillating from immediate (e.g., low-cost compressor) to long-term (non-platinum-group-metal [non-PGM] catalysts) every few years. The stability of approach and clarity of vision is critical for long-term success of fuel cell development in the United States. Further, the area of focus between ARPA-E and FCTO is unclear (both are funding anion-exchange membrane research).
- There seems to be a large focus on the long term. It does not appear that current solutions being investigated will materialize into cost savings (even incremental ones) in the near term. There does seem to be a good deal of movement in getting infrastructure together to potentially result in more near-term benefits (such as the many consortia), but it was not clear what near-term gains were expected.

3. Were important issues and challenges identified?

- Yes, cost and durability are well known in industry to be the main issues, and the program has significant focus in these areas.
- Yes, the primary remaining challenges of cost and durability were properly identified and set into context.
- The challenges and issues regarding the stack cost and durability were identified, and the key focus areas were addressed clearly.
- The issues with the development of fuel cell power systems for transportation have been clearly identified. Durability and cost to address distributed generation/combined heat and power fuel cell systems have been identified to a lesser degree. Issues for fuel cell development in the transportation area may be more difficult to address. Cost and durability have been identified as the issues for fuel cell development, and PGM catalysts are still the issue.
- Important issues and challenges are partly identified. Some of the DOE-highlighted issues (e.g., stability of cathode catalyst support) have been solved by the industry. However, DOE may be unaware of these developments.

4. Are plans identified for addressing issues and challenges?

- The program addressed well-organized plans with collective and effective approaches for the challenges.
- Yes, plans have been identified to address cost and durability. The Fuel Cell Consortium for Performance and Durability (FC-PAD) is a strong collaboration that will meet these goals.
- Reasonable plans have been put forward. The new consortium-based method of funding activities of the national laboratories and drawing connections between outside projects and the laboratories should prove superior to the previous procedure of having the laboratories and outside organizations compete for funding within the same call, with only outside organizations able to provide the required cost share. However, it will take a while for the consortia and their interactions with outside projects to settle in. Great care and effort will be required to maximize the productivity of the new arrangement, and trying to implement a number of new consortia in the same year increases the danger that only pro forma, rather than truly effective, interactions will develop between the laboratories and between the laboratories and the outside partners. The deliverables for the laboratory-call projects should include accounting of activities with the consortia and outside partners, lest the outside work be neglected in comparison to the activities within the individual laboratory-call projects.
- On the cost side, yes. Multiple avenues for addressing cost were presented (although the focus is weak). However, plans to address durability were not as clear, especially for polymer electrolyte membrane fuel cells (PEMFCs). Accomplishments and projects in durability for other fuel cell types were presented, but PEMFCs still seemed relatively unaddressed.
- Yes.
- Plans are only partially complete. It looks like some of the projects were kicked off without much thought into how a single new technology (e.g., nanostructured thin film [NSTF]) will fit into the overall picture and how this will address the primary issues and challenges hindering the commercialization of fuel cells.

5. Was progress clearly benchmarked against the previous year?

- Yes, the progress was clear and comprehensively covered.
- Progress was clearly benchmarked to a great extent.
- Progress in the area of cost has been adequately benchmarked over several years. It is a bit difficult to assess whether catalysts, membranes, gas diffusion media, membrane electrode assembly (MEA) fabrication techniques, or testing are responsible for the cost reductions shown. FC-PAD may address this issue by providing a common evaluation methodology.
- Progress against past performance was properly benchmarked, albeit not generally against the previous year. The field is now sufficiently mature that one should not expect large numerical changes in metrics over one year, but progress is evident over spans of approximately five years. However, examples of individual advancements within the past year were properly highlighted.
- On cost, progress seemed to be benchmarked, but not on durability.

- The cost progress has stalled for the last five years (~$55/kW), and the durability has not progressed much since the 2014 review. Therefore, it is not clear what has been accomplished by the projects funded through this initiative. While many new catalysts are being invented and tested, none of these has made any significant impact on the power density (cost) or improved the durability of the overall fuel cell system.

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 projects in this technology area are addressing the problems and barriers that the FCTO is trying to solve. The projects are focused, correctly, on cost and durability. However, catalyst supports other than NSTF require more research.
- Yes, the work is going to be necessary to support the widespread adoption of FCEVs.
- The current portfolio of projects seems to be a mixture of long-term objectives and many Small Business Innovation Research initiatives and development initiatives focused on addressing key challenges. On paper, it looks like there are too many initiatives and not enough focus on solving the biggest challenge.
- Yes (three responses).

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- The Fuel Cells program is focused, well managed, and effective in identifying and addressing the FCTO's needs (2 responses).
- The program was well managed, and meaningful progress was achieved. The program effectively addressed the important needs of the FCTO.
- The program is well orchestrated.
- This appears to be the major weakness of the program. There seems to be a lack of focus. Given that this is the closest of the sub-programs to basic science, a certain degree of broad research base can be reasonably expected. However, it seems that the overall program lacks focus even with this consideration. Projects seem to overlap in their overall goals and do not sufficiently explain why they may all be necessary together as a suite of initiatives. One clear example from this year's Annual Merit Review was alkaline. In the overview, it was mentioned that long-term targets likely require elimination of platinum and that alkaline was being pursued in this regard. However, neither the program nor the individual alkaline presentations discussed why alkaline in particular was pursued or why certain technologies within the program were pursued. Clearer expression of the promise of alkaline toward both achieving the long-term target (the primary goal) and removing platinum (the secondary goal, which is really only one possible means of achieving the primary goal) needs to be provided. Otherwise, the program does seem well managed and effective. Many important improvements have come out of the program, but as mentioned by a commenter in the audience, it does appear that a new program structure may be necessary to increase focus on technologies that will really help build on past progress and break the stagnation that seems apparent (e.g., reductions in cost that are now flatlining).

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- The strength of this program includes nicely organized and managed projects with excellent collaboration efforts among the leading national laboratories.
- The work that has begun to look into consistent evaluation of the durability of fuel cells in various applications is a particularly valuable effort. This project bridges some of the more exploratory work that must be done in the laboratories with the eventual market concerns. Evaluations like these may be expanded to help bring focus to the program. Additionally, the consortia efforts stand out as having potential for powerful transformation of the technology. The opportunity in these projects simply needs to be capitalized and perhaps more thoroughly discussed in the future.
- The key strength in the Fuel Cells program is in starting collaborations such as FC-PAD. It would be good if a program such as ElectroCat (the Electrocatalysis Consortium) could be similarly developed with

the suggestion of collaborative work being focused to single-year awards with a possibility of a second-year follow-on.

- There were three major strengths: (1) the strong pursuit of and progress toward well-chosen targets developed in consultation with DOE strategists and industry experts; (2) new relationships between laboratory consortia and outside projects that could significantly improve productivity of all projects if enthusiastically pursued by all parties; and (3) EERE projects that often include synergistic collaborations among fuel cell developers, established suppliers, academia, and national laboratories. Such vertical integration limits myopia and fosters eventual commercialization of developments based on DOE funding. There were three major weaknesses: (1) some fuel cell developers are still overly reluctant to share true state-of-the-art data and details of specific technical challenges with DOE; (2) the recent increase in emphasis on alkaline membrane fuel cells may be misguided; and (3) non-PMG catalyst projects still place too much emphasis on oxygen reduction reaction kinetic activity and not enough on improving transport properties through the much thicker catalyst layers needed for non-Pt systems.
- The focus of the program needs to be improved. Also, regarding achievement of targets/goals, every year the achievement of fuel cell performance targets comes with the caveat that these achievements are not all met by the same technology all at once. This method of counting accomplishments is not ultimately of high value. For practical application, all of the targets will need to be met at the same time by an individual technology. Perhaps focus can be gained not only by presenting the separated target achievements but also by evaluating technology potential by looking at performance versus the full suite of targets for a given technology.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- The projects represent a wisely chosen spectrum between innovative projects still far from application (e.g., non-carbon supports or fancy structured catalysts) and incremental improvements to previously studied systems.
- Yes, the projects do investigate novel ideas, especially for catalyst development. However, the program overall perhaps needs to think differently about its approach and use new principles to guide its focus.
- Yes, within the funding available (2 responses).
- Many novel ideas seem to be stuck in technology readiness level three (ex situ testing). There is no clear path for moving these ideas to an in situ fuel cell environment and solving the high current density performance issues to meet 2020 cost and durability targets simultaneously.
- The projects discussed do not represent novel/innovative ways to approach the technology barriers of cost and durability. The projects discussed are evolutionary in nature. This issue may be addressed in the ElectroCat program.

10. Has the program engaged appropriate partners?

- The Fuel Cells program has engaged the appropriate partners to address the various issues identified for the broad commercialization of fuel cell technologies.
- The program engages in extensive, worldwide collaboration.
- Yes, there is significant coordination among many of the stakeholders in the area.
- Yes, although fuel cell developers and materials suppliers need to be more open in sharing details with DOE to make progress more efficient. More should be done to foster precious-metal-catalyst development and manufacturing by U.S.-based companies. Foreign ownership of all major suppliers of precious metal catalysts should be investigated as a possible national security issue.
- Yes (two responses).

11. Is the program collaborating with them effectively?

- Yes, it appears the collaborations are well suited to the individual projects.
- A good example of effective collaboration is FC-PAD. FC-PAD is a good platform for collaboration between many stakeholders and investigators.
- Yes (two responses).

- Yes, though the fuel cell developers need to be more open with DOE, particularly about technology status. Catalyst suppliers need to be more open to unrestricted analysis of their materials. Because process, not composition, is usually the basis for competition in the catalyst world, open analysis should not generally compromise any company's competitive position.
- Partially.

12. Are there any gaps in the portfolio for this technology area?

- None within the funding allowed.
- There do not appear to be significant gaps in the research.
- Not particularly.
- There are few gaps in the Fuel Cells program portfolio.
- Experienced catalyst manufacturers should be recruited to attempt development and scale-up of advanced catalyst types out of the national laboratories. Scale-up efforts at the national laboratories have proceeded slowly. For non-Pt catalysts, insufficient attention has been given to engineering thick (~100 micron) electrode layers with adequate transport properties. This needs to be looked at, perhaps initially using very low-loaded Pt on derivatized carbon supports that are structurally similar to pyrolyzed non-Pt catalysts. If effective thick electrodes turn out to be an engineering impossibility, then the substantial effort in improving activity and durability in non-Pt catalysts is wasted.

13. Are there topics that are not being adequately addressed?

- No (two responses).
- This may be more appropriate in another program area, but it seems that there needs to be a risk/cost assessment performed for research ideas being pursued that would require significant change in direction for industry. For example, in moving from PEMFC on vehicles to alkaline, it is not clear whether stranded assets (or other economic risks) could result from such a large shift in technology onboard the vehicles. It is also not clear whether such a risk could affect the market; some automobile manufacturers may see current technology as too unsettled and therefore are waiting for more optimal/proven/viable technologies to become apparent and avoid the possibility of being stuck with stranded assets.
- There are two topics that may not be adequately addressed. The first is MEA fabrication with catalyst supports other than NSTF. There have been durability questions with NSTF-based catalysts for many years; the pathway for these being addressed is not clear. Perhaps more work that addresses membrane degradation directly or additives that address membrane degradation can be future topics. There is funding for the membrane work; it was included in the package, but it was not adequately addressed.
- Greater attention should be given in all of the projects to the implications of the local oxygen transport effects that limit the performance of low-Pt-loaded fuel cells at high current density. These limit the utility of catalysts with very high Pt-area-specific oxygen reduction activities unless the Pt-mass-specific Pt surface areas are reasonably high (above about 30 m^2/g). This issue, unless it can be solved by something like a new ionomer for use within catalyst layers, forces a reprioritization between approaches to lowering Pt loadings on fuel cell cathodes from those that give high area-specific activities to those that give high Pt-mass-specific surface areas. Certain specific projects are addressing local oxygen transport, but awareness of the implications of the effect does not seem to have permeated through to the planning within all of the projects.

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- There is an increasing focus on contaminants in the hydrogen supply from infrastructure currently being installed. Industry response has been to focus on quickly detecting and addressing impurities at the supply side. Another approach is to look into fuel cell impurity tolerance. There has been some focus on this in the past, and it may be worth considering adding more focus on this area again. In particular, this would be most helpful if it resulted in near-term developments.
- The recent requirement that all multiyear projects be completely funded upfront within the budget year in which they are initiated has led to the frequent issuing of more tightly focused funding opportunity

announcements (FOAs), with very few projects being funded from each FOA. For example, catalyst projects have been funded one year, and MEA integration projects have been funded another year. Unfortunately, making substantial progress on the status of fuel cell development requires coordination between catalyst development and MEA integration. Good proposals can be rejected because they do not fit the topic restrictions of the current FOA. Therefore, the current funding procedures, while likely making the accounting more transparent, distort the technical projects from what would be planned for optimal technical effectiveness. Extreme care must go into the long-term planning of a multiyear series of FOAs if significant distortion of the program is to be avoided, and some activities that are unrelated to a given year's FOA topic should be tolerated.

- No.

15. Can you recommend new ways to approach the barriers addressed by this program?

- This program needs a clear vision of what the step-out ideas need to be and a well-balanced portfolio that works toward the long-term vision while supporting the industry and developers to achieve near-term cost and durability goals.
- A program like FC-PAD that allows for one-year contracts with a one-year follow-on based on performance review could help address technology development barriers in the Fuel Cells program. In this (1+1) model, research would be evaluated at a program review, and continuation (funding for the second year) will be assessed. If a project does not make it to the second year, others would be given an opportunity to contribute.
- Reconsider the assignment of so many resources to alkaline membrane fuel cells. Yes, they make it easier to replace Pt on the cathode, but then one faces significant challenges in getting adequate hydrogen oxidation activity on the anode without significant use of precious metals. The performance of OH-conducting membranes still falls short of requirements for transportation applications. An explicit effort to engineer thick (100-micron) electrode layers with adequate transport properties is necessary to see whether continued non-Pt-catalyst development is warranted. One could start with a very low-loaded Pt catalyst.

16. Are there any other suggestions to improve the effectiveness of this program?

- Establish clear go/no-go criteria based on state-of-the-art MEAs; if any project is unable to meet the majority of the performance benchmarks of state-of-the-art MEAs, there is not much value in trying to make progress on this one focus area if other objectives are being moved negatively (e.g., metal oxide support for cathode catalyst improves stability at the cost of high Pt loading).
- Do not waste effort on high-throughput synthesis of non-Pt catalysts, whose success or failure is a matter of proper processing, not of a particular composition. Instead, focus on the engineering of transport-effective thick electrodes. As with most funding agencies, it seems difficult for new applicants to break in and get a project approved. Many of the new projects are essentially extensions of previous projects, recast to fit the requirements of the current FOA. Some of this is good, as one does not want to throw away experience and demonstrated project performance. However, two changes might be productive:
    - Push for ambitious go/no-go criteria and shut down projects that do not fulfill the criteria. It is difficult administratively to shut down a project, but doing so occasionally should stimulate the remaining projects to higher performance and would clear the way for new applicants.
    - Consider setting aside a portion of each year's appropriation to support smaller projects (likely in a separate call), with particular encouragement given to new applicants. This could help get more academic activity back into the EERE mix, thereby fulfilling the goal of training the future technical workforce. Perhaps BES already fulfills this role, and EERE should just get involved in the evaluation of fuel-cell-related BES projects (if it is not already).

Manufacturing R&D Program Comments

1. Was the program, including overall strategy, adequately covered?

- Yes, the strategy was clear on how to address identified barriers within the broader framework of goals and objectives.
- Yes, the presentation comprehensively presented the problems, strategies for work, and outcomes.
- Having a single manufacturing program for very different technologies, ranging from fuel cell membranes to pipelines, makes gaining a clear focus difficult. The small budget also hampers progress.
- Yes (two responses).

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- Yes. In particular, it was encouraging to hear about the exchanges with industry for some of the defect-detection technologies. The engagement with manufacturers and eye toward technology transfer is important and was well addressed to keep the near-term focus that is necessary in this program.
- Because of the nature of manufacturing, the program has a short-term focus, which is appropriate for this program.
- The main focus appears to be on near-term research and development (R&D) to serve long-term manufacturing goals (production at the scale of 500,000 units).
- Yes (two responses).

3. Were important issues and challenges identified?

- The program has identified several critical issues for manufacturing and is addressing them appropriately, given the funding available. If more funds were available for the area, the work could be expanded, but the program has chosen appropriate challenges.
- Yes. However, if there is more significant demand from original equipment manufacturers (OEMs) and/or polymer electrolyte membrane (PEM) fuel cell manufacturers for fuel cell material/component products, the areas of "quality control in production" and "supply chain maturity and U.S. opportunities" can be expected to improved rather rapidly—unless this has been established outside of the United States (but this was not made clear).
- Yes. However, some of these projects have been focusing on similar challenges in manufacturing during the past couple of years. The program likely needs to start expanding to tackle other issues. As a prime example, compressor manufacturing should be investigated (especially the highest-pressure compressors at fueling stations) to determine what role, if any, manufacturing may have in the high rate of downtime as demonstrated by the National Fuel Cell Technology Evaluation Center (NFCTEC) project.
- Yes (two responses).

4. Are plans identified for addressing issues and challenges?

- Yes, the translation of program goals to actionable projects and efforts was well presented.
- Yes (four responses).

5. Was progress clearly benchmarked against the previous year?

- This was briefly presented in some of the project presentations, but in general, the program overviews do not address this question well, as was the case here.
- Comparison to the previous year could have been better.
- No. However, this could be understood from reviewing the 2015 presentation and comparing it to the 2016 accomplishments.
- Yes (two responses).

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 potentials for cost reduction and growing scale of deployment of fuel cells address key barriers. Also, the supply chain technical exchange project will be valuable not only for developers but also for other stakeholders who may be looking to assess the technology from a regulatory or economic analysis perspective.
- Largely, yes. Some of the efforts represent benefits that will likely not materialize in the near term but could materially reduce costs.
- The broad problems and barriers were mostly addressed. However, pipeline material manufacturing research is challenging to justify because it is not clear whether there is a good understanding of the barriers and challenges of implementing more hydrogen pipelines in the United States (this includes the cost of pipeline right of way).
- Yes (two responses).

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes. Given funding limits, the program is well directed and focused.
- Yes (four responses).

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- The program strengths seem to lie in the innovative developments that come out of the program as products/deliverables. The weakness is the limited scope in some sense (increasing the number of fuel cell components and station components will need to be introduced into the program). The really strong projects seem to be the technical exchange centers and the quality control (QC) diagnostics. None of the particular projects is weak.
- The National Renewable Energy Laboratory's efforts related to QC methodologies have been a strong project for some years. The other current projects are higher-level assessments and development of supply chains that have the potential to lead to measureable improvements.
- A strength is the use of appropriate funding instruments, e.g., Small Business Innovation Research (SBIR) and funding opportunity announcements, to address relevant issues.
- A weakness is possibly that major membrane electrode assembly (MEA) manufacturers, such as Johnson Matthey, do not appear to be directly involved with the MEA QC project.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Yes, there are examples of first-of-their-kind efforts among the sub-program's projects.
- Yes, the Mainstream SBIR Technology Transfer Opportunity project represents a novel approach. Others, such as assessments and facilitated networking of industry suppliers, are not novel, but they are necessary and good for addressing barriers.
- It does not seem that they are highly novel or innovative.
- Yes.

10. Has the program engaged appropriate partners?

- It appears to have engaged appropriate partners. However, involvement of larger industry players may be beneficial.
- Yes, the program has engaged partners quite extensively. This should continue, especially for the technical exchange.
- There is good work with OEMs and Tier 1 suppliers. The regional technical exchange center collaboration work is yielding valuable information.
- Yes, the projects generally have good collaborators from industry to academia.

11. Is the program collaborating with them effectively?

- Yes, cross-cutting activities provide good leverage for existing funding.
- Yes (three responses).

12. Are there any gaps in the portfolio for this technology area?

- With the available funding budget for this topic area, it appears properly addressed.
- There are not any significant gaps at this time. The program is in a bit of a transition, and current higher-level projects should identify more discrete future efforts.
- Type 3 and Type 4 fully overwrapped storage tubes need to be used in ground storage applications for hydrogen fueling station use.
- QC of manufacturing is generally a focus of the program, but the projects so far are limited to a single component of fuel cells. This focus needs to be expanded to other fuel cell components and station components. On the station side in particular, there may be an opportunity to help identify issues that are affecting stations today.
- Yes, but insufficient funds are available to address them.

13. Are there topics that are not being adequately addressed?

- Gaps are being addressed adequately with available funds.
- There may be opportunities in QC inspection methods of steel vessels for stationary hydrogen storage.
- Type 3 and Type 4 fully overwrapped storage tubes need to be used in ground storage applications for HFS use.
- This is potentially a cross-cutting effort, but it seems that there should be some risk/economic analysis on the manufacturing side for situations in which equipment providers “switch” to a new or future technology. For example, if alkaline becomes predominant instead of PEM in vehicles, or cryocompressed or solid-phase hydrogen storage becomes predominant, it is not clear what potential manufacturing equipment, lessons, and other investments could be leveraged in the transition to these proposed future technologies. It is not clear what the potential for stranded assets is, especially because the market may scale significantly before some of these future options become commercially viable.
- No.

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- The program should consider investigating composite hydrogen tank wrapping methods (manufacturing) to determine whether this can be sped up while delivering the same quality. The current manufacturing speed (tank/unit of time) does not align with mass volume production of cars (10,000 fuel cell electric vehicles [FCEVs]/year is about 30 FCEVs/day, requiring ~60 composite tanks/day, and 100,000 FCEVs/year is about 300 FCEVs/day, requiring ~600 composite tanks/day).
- Type 3 and Type 4 fully overwrapped storage tubes need to be used in ground storage applications for HFS use.
- Yes, assuming that they are not addressed in other sub-programs.
- No, no other areas should be considered at the moment.
- No.

15. Can you recommend new ways to approach the barriers addressed by this program?

- It is recommended that the program assess state-of-the-art manufacturing capabilities and quality assurance methods in Europe, Japan, and China and assess (in addition to specific methods) what other quality assurance processes play an important role to warrant product quality of supplied product over the years.
- For reducing costs, it may be worthwhile to have not just a supply chain exchange but also a lessons learned or best practices exchange, at least to the extent that manufacturers are willing to participate.

Development of something like "best practice guides" for manufacturing various components may catalyze expansion and new entries into the component and equipment supplier markets, which could bring costs down through competition.

- No.

16. Are there any other suggestions to improve the effectiveness of this program?

- Given its funding, the program is functioning effectively.
- It is recommended that the program assess FCTO projects in other sub-programs (such as the Hydrogen Production and Delivery program) in which manufacturing is part of the project and finding solutions for manufacturing issues is part of the project scope. Some of the solutions found or paths taken to find solutions could be beneficial for Manufacturing R&D program projects (cross-pollination of expertise).
- No (two responses).

Technology Validation Program Comments

1. Was the program, including overall strategy, adequately covered?

- Yes, the program was adequately covered with identification of the project areas, emphasis, verification, and risk mitigation.
- Yes, there was a clear discussion and slide for the strategy of the Technology Validation Program.
- Yes, the program was well covered.
- Yes (two responses).
- The objectives are reasonable but should be more inclusive of the technologies being tested. For instance, it is not clear what the objectives of testing cryogenic vessels and high-pressure liquid hydrogen (LH2) pumps are. It is not clear what the goals are. The strategy overall is adequate, but the program was not adequately covered during the presentation. During the poster session, it was observed that there are more projects under this program than were presented during the program overview.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- The presentation provided a clear set of objectives (targets) for the near-, mid-, and long-term goals and objectives. The balance was demonstrated by comparing several of the slides that identified accomplishments and new projects.
- Yes, there seems to be good communication with the laboratories to ensure that after technology is verified at the laboratory scale, it evolves into the technology validation area before moving to market transformation. The projects adequately cover technologies that range from near- to long-term.
- Yes, the program seems to have a major focus on near-to-mid-term projects. The longer-term electrolyzer grid integration project provided a good balance for the portfolio of projects.
- Generally yes, the program is balanced with identification of laboratory analysis and measures for risk management.
- Yes (two responses).

3. Were important issues and challenges identified?

- Yes.
- Generally yes, the important issues/challenges were identified with project selection, metering/ measurement, and reporting. Many of the other presentations supported the overview.
- Important issues and challenges were identified, though the specifications for some areas seem like they need to be updated. In particular, the hydrogen station goals appear to address too narrow of a subset of the specifications that are being established or determined right now by stations being put in place in the United States and around the world. Necessary specifications such as back-to-back-fill sequencing and timing and simultaneous fueling capability are all issues that need to be addressed, and this program could help validate these more advanced capabilities of station equipment. Additionally, the targets that are set (capacity and fueling rate) need to be updated. The target for 2019 is already met or nearly already met by stations being built today. The targets for the program need to look to the next generation of stations and be set appropriately.
- The presentation focused on accomplishments and failed to mention issues for each of the projects. The individual project presentations did mention challenges. It would be informative to list challenges on the slides of the program assessment.
- There did not appear to be a chart identifying specific challenges; however, under the accomplishment charts, there were targets given that were assumed to be challenges for the projects.
- No, the issues/challenges were not identified in the presentation.

4. Are plans identified for addressing issues and challenges?

- Yes, the Request for Information issued by the Fuel Cell Technologies Office for truck targets is a particular example of a necessary and well-designed step for addressing an upcoming and urgent challenge.
- Range, efficiency, and operation were all appropriately identified for refueling. The connection with grid modernization, grid simulation, and energy storage is of value, but the connection was not fully assessed for actions and remedies.
- Data collection appears to be the approach for addressing the issues and challenges, combined with working closely with industry.
- Challenges were not identified in the main presentation, but individual project presentations did mention challenges in most cases and how they are being addressed.
- Some issues/challenges need help.
- No.

5. Was progress clearly benchmarked against the previous year?

- Yes.
- Yes, sufficient detail was available for the light-duty fuel cell electric vehicles (FCEVs), fuel cell electric buses (FCEBs), and fuel cell electric trucks (FCETs).
- Data collection and analysis for both FCEVs and FCEBs were presented for 2015 and 2016. While it was clear which parameters improved, it would have been informative to get additional information about the challenges that resulted in lower-than-expected performance and the plan to bridge the gaps between the current number and the target. The differences between 2015 and 2016 were explained during the project presentation (FCEV Data Collecting and Analysis slide), as was the cost difference between fuel cell vehicles, compressed natural gas, and diesel vehicles. This cost comparison slide should also have been included in the main presentation to provide a more complete picture of the status of the technologies. For the United Parcel Service (UPS) vans project, it is unclear what the progress was between 2015 and 2016. For the performance validation and contaminant detection project, it was unclear when the project started and whether any data have been collected from compressor manufacturers. The progress of other projects not presented during the program overview was explained during other sessions.
- Progress was not fully benchmarked, but the goals were adequate to show the intent and intended progress. Other presentations supported the U.S. Department of Energy (DOE) directive to benchmark progress.
- This was not very clear from the program overview presentation, but it was included in some of the individual project presentations.
- 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?

- Yes.
- Yes, this program is critical to evolving technologies from the laboratory to the field and to demonstrating their feasibility at scale.
- The projects are collecting real-world data that will provide guidance for future developments to move FCEVs to commercialization.
- Yes, the project to address the ability of hydrogen to enable more renewable electricity was particularly good.
- Generally, yes, they are. There was one particular project that seemed like its applicability could be limited, though. The station equipment power and energy demand project appears to be built around a single station. There seems to be significant potential that any conclusions or lessons learned from that project will be highly specific to that station design or possibly even just that particular station. This project should keep an eye toward how its findings can be applied broadly once the project is complete.
- Yes, but an improved connection between the vehicle refueling and grid modernization with energy storage would have been helpful.

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes (two responses).
- Yes, this program is addressing needs that are particularly well timed to ongoing developments, even outside of FCTO.
- Yes, both areas of the presentation were focused and well managed, but next steps should increase the connection between the refueling and grid storage topics.
- The focus is a little unclear, given that some of the technologies being tested do not fall within the objectives listed for 2019, 2021, and 2023. The projects do fall within the strategy, though. It is also unclear how some of the projects transition from the Technology Validation Program to the Market Transformation program; for instance, the UPS (Technology Validation program) and the FedEx (Market Transformation program) projects are very similar. The data analysis and reporting project is particularly strong and well underway to providing useful data.
- Yes, the program is focused on the very important transportation applications. The infrastructure development of a hydrogen station needs to be emphasized. It did not appear that DOE had access to the data developed by industry for the hydrogen stations used for forklift and backup power applications, although the reviewer believes DOE has this information.

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- The data collection, management, and reporting projects of the program are a particular strength. Going forward, it would be helpful for the program to coordinate with its stakeholder "customers" for composite data products to ensure that the information being provided through the program is relevant to the stakeholders' needs. Some data presented have at times seemed like they would be more useful if expressed or provided in a different metric, visualization, etc. Gaining this feedback directly from stakeholders will maximize the impact of a program that is already highly effective and very necessary during today's rapid acceleration of fuel cell deployment. Hydrogen Station Equipment Performance (HyStEP) is another strength, especially for its collaborative nature and ability to help stakeholders solve problems right now. The generalizability of the power and energy use project is a little suspect. A similar concern exists for the Washington, D.C. station, as it is not clear how lessons learned from what may be a highly local set of circumstances (permitting agencies, etc.) could be easily translated for other stations in other regions. However, this is not as mission-critical as it is for the power and energy use project.
- Two good topics (FCEV hydrogen refueling and grid modernization with energy storage) with opportunity for coordination is an obvious strength. A weakness would be the lack of strategic coordination to connect these two strengths.
- Key strengths are large-scale grid storage and FCETs (to open up new markets such as the forklift truck market). A weakness is the continued development of onboard LH2 storage in passenger vehicles. (LH2 will continue to be a valuable pathway to transport hydrogen to the station, but as long as no original equipment manufacturer [OEM] is developing FCEVs that store LH2, it seems to be a poor use of taxpayer money to continue automotive onboard LH2 storage, since all OEMs have settled on 700 bar storage.)
- The strongest project right now is data collection and analysis for both FCEVs and FCEBs. The hydrogen-at-scale project is also very important, but not at its current scale; it is necessary to partner with a utility to scale up the project and produce hydrogen off-peak. There is great potential value in this application if integration challenges can be addressed, although the applications may be limited by geographical location (sources next to customers); ability to provide hydrogen to ensure availability in, for instance, hydrocracking units; long-term hydrogen contracts; and others. Developing targets for medium- and heavy-duty trucks is important, but it is unclear why that responsibility falls under this program.
- The key strength is the close collaboration with industry. The weakness is the long timeline for the development of FCEVs and FCEBs. Validation of the technology without a reduction in cost of the technology may not be beneficial.
- The National Renewable Energy Laboratory's project on hydrogen station data collection and analysis needs attention.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Yes.
- Yes, HyStEP and the National Fuel Cell Technology Evaluation Center are examples of projects within this program that generated tools that simply did not exist and are proving their value to stakeholders.
- Yes, the notion to bring energy storage into grid operations with vehicles and vehicle fueling is not well accepted but would be of high value.
- They are not necessarily novel, but this is not the program where novelties are expected. New technologies are generated in the research and development (R&D) side and then tested in Technology Validation program. The approach is appropriate.
- Novel and innovative are not descriptors for this effort. Hard work, incremental improvements, and close collaboration with industry better describe these projects.
- Nothing particularly novel stands out.

10. Has the program engaged appropriate partners?

- Yes (three responses).
- Yes, the partners were identified in the presentation.
- For the most part, yes, but it would be good if the program brought in more international partners for data gathering and analysis and also for cryogenic hydrogen testing— BMW and Shell are doing a lot of work in this area. It was good to see during the poster session that there is a collaboration with Air Products and Chemicals, Inc. to test dispensing from tube trailers.
- Partners appear adequate, but additional feedback from utility partners, regulators, and transportation planners would be of value.

11. Is the program collaborating with them effectively?

- Yes (two responses).
- There appears to be good collaboration.
- Collaboration appears adequate, but additional collaboration between automakers, fuel providers, utility regulators, state transportation planners, and grid managers would be of value.
- Yes, collaboration is adequate.
- Mostly yes, although some need improvement.

12. Are there any gaps in the portfolio for this technology area?

- It is unclear why the Technology Validation program and Safety, Codes and Standards program are not working together on a cross-cutting project to address inline fuel quality testing at hydrogen stations. Combined with the work presented last year by Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST), it seems that such a cooperation could help accelerate the development and validation of sensor technology that is needed now but does not exist in a plug-and-play component.
- Correction or normalization of performance data from the buses as they age is a gap. This was, however, addressed in one of the project presentations.
- None are apparent (2 responses).
- There are no projects addressing stationary fuel cells. It is not clear whether there is any new R&D in this area that needs validation. Although a tube trailer refueling project is underway, that was not mentioned during the overall program presentation. Also, the hydrogen-at-scale project needs to be scaled up and tested at large scale in partnership with a utility.
- It is not clear how the cost of the fuel cell systems and the hydrogen stations fits in with Technology Validation.

13. Are there topics that are not being adequately addressed?

- No (three responses).

- More integration of cost numbers with the projects is needed.
- More precise goals are needed to coordinate topics for energy storage with both transportation and grid management.
- Several topics were not adequately addressed: (1) scaling up renewable energy power to hydrogen with added storage, (2) partnering with international stakeholders (e.g., Shell, BMW) to gather additional data on cryogenic hydrogen dispensing, and (3) comparing data gathered from domestic stations against data from Germany and Japan (aggregated).

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- No.
- The program addresses the important applications.
- The program may be able to help investigate the viability of co-locating medium-/heavy- and light-duty fueling at the same location. While there are a couple of examples existing, their designs were more one-offs, and lessons may not be generally applicable. It would helpful for the program to discern the special considerations of combining purposes and help determine whether there are sufficient gains to be had for either application and the extent to which similar components/designs/etc. could actually be used in one station to meet two needs. The benefit or penalty of increasing system complexity is also uncertain.
- Increased coordination with utility regulators for grid management and transportation planners that control transportation budgets should be considered.

15. Can you recommend new ways to approach the barriers addressed by this program?

- No (two responses).
- Better management review by some is recommended.
- The approach should address how technology validation and cost reduction interact.
- The program should increase coordination with state transportation officials and utility regulators, and with vehicle refueling, vehicle OEMs, and utilities.
- It would be good to remind the program manager of the importance of providing cost information on all of the projects.

16. Are there any other suggestions to improve the effectiveness of this program?

- Overall, the program is thoughtful and well managed. Increased coordination and collaboration among transportation fueling and grid stakeholders may produce valuable partnerships that connect transportation with energy storage and grid performance.
- It is not clear whether the program addresses distributed generation using solid oxide fuel cells. If it does not, it is not clear why.

Safety, Codes and Standards Program Comments

1. Was the program, including overall strategy, adequately covered?

- Yes, there was a good overview of the overall strategy with goals and objectives. It is clear that safety, codes and standards (SCS) enable introduction of hydrogen infrastructure and adoption of fuel cell electric vehicles (FCEVs).
- Yes, the strategy was presented well and made logical sense. The explicit inclusion of feedback to regulations, codes, and standards (RCS) is key and a highlight of the program.
- The program was clearly presented with a clear strategy of defining near-term and long-term objectives and efforts, and of how the research and development (R&D) approach will be an enabler to achieve the main goals of the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program).
- Yes, there are many projects targeted at key areas and the broad barrier of "lack of data to inform development of performance-based codes and standards" to support vehicle/infrastructure deployment.
- The strategy is clear, well set, and thorough.
- Yes, thanks to the initial presentation that provided a general overview—although it had strong emphasis on only three out of five areas of activity, followed by several presentations devoted to the specific activities performed by projects—the SCS program seems to perform as planned, adequately covering the different activity areas proposed in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan: (1) safety management, (2) R&D, (3) test measurement protocols and methods, (4) development and harmonization of RCS, and (5) dissemination of data, safety knowledge, and information.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- Yes, the near-to-mid-term focus of many of the projects in the program is appropriate. The needs in this area are very "boots-on-the-ground," and the program does a good job of developing projects that focus on this characteristic.
- The near- and long-term balance seems appropriate for this program. This can be can be seen through the fact that there are significant efforts on near-term activities, such as fuel quality and gaseous separation distances, and at the same time, the program is allocating significant resources to longer-term efforts, such as the ongoing efforts on liquid hydrogen activities.
- Yes, there is a combination of near-term testing with actual hardware (hydrogen sensors and pressure relief devices [PRDs]) and longer-term (materials compatibility) projects. All of these can provide good data for current specific code development and outreach/education work.
- Yes, the SCS program shows a balanced portfolio of activities with goals in the near, mid, and long terms.
- There was no unbalance between the different time spans of the program. The activities are multiannual, and the projects are integrated in the overarching strategy.
- It is not completely clear whether balance is needed, given the limited budget. There is sufficient balance as is.

3. Were important issues and challenges identified?

- Yes, many of the major challenges facing stations and fuel cell applications today are being addressed by the program.
- The program manager has clearly identified the main issues and challenges associated with hydrogen station rollout. This can be documented with the emphasis on some of the critical areas, such as fuel quality and station footprint, and by using a science-based approach to solve these.
- Yes, very good examples in PRD testing and material compatibly testing show potential problem areas with current industry approaches. The project will need to keep an eye on opportunities to get new/additional data and identify emerging risks/opportunities because both vehicle and infrastructure deployment are ramping up fast, so the project should get plenty of data.
- Yes, the activities within the program are inherently challenging by their own nature, but in the main, the challenges appear known and seem to be under control. Nevertheless, there are no references within the

information provided to potential new challenges that may appear or have already appeared during the course of the activities and the associated plans to address them.

- Yes.
- No.

4. Are plans identified for addressing issues and challenges?

- The program portfolio is very well-rounded with very clear objectives and a clear strategy for addressing the main challenges being identified. The science-based approach is definitely key to the success of this program.
- Most have identified plans. Several of the projects need to make sure that plans for future work are well/better focused on the mission of generating clear data to inform standards to support deployment.
- Yes (two responses).

5. Was progress clearly benchmarked against the previous year?

- Yes, this was well documented by the program manager on slide 12. The reviewer really wants to highlight the significant progress being made on the liquid hydrogen activities, such as the liquid hydrogen release behavior experiments. Other activities that demonstrate significant progress include the development of a prototype for in-line fuel quality monitoring, the release of the Hydrogen Risk Assessment Model (HyRAM), and the materials compatibility efforts.
- Yes, this was one program in which this was clearly covered in the program overview, and it was appreciated. A good deal has clearly been accomplished since the previous review.
- Yes, the progress is clearly shown in the R&D activities, but this could be extended to the rest of the activity areas.
- The progress shown, obviously incremental, was very convincing.
- Yes, this was done very clearly on slide 12.
- Generally yes, but the reviewer has not personally reviewed these projects before and therefore cannot make a direct comparison. However, in most cases, there is a clear account of deliverables/ accomplishments in the past year. It is not always as clear for multiyear projects whether the project is on track with the original plan in the long run, so some summary of the projects over their full lives might be helpful for ongoing projects.

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, without any doubt, the projects within the program are complementing the activities of the rest of the sub-programs within the FCTO, providing valuable information and knowledge to facilitate the deployment and commercialization of fuel cells and hydrogen technologies. The efforts to promote international collaboration, which strengthen the excellence of the Program while avoiding addressing issues that may already be addressed in other places in the international landscape, are commendable.
- Yes, the program is definitely addressing some of the main issues associated with initial rollout (fuel quality and footprint issues) as well as addressing the main long-term issues (mainly footprint) associated with liquid hydrogen stations.
- Yes, SCS program projects are clearly tied in to the need for enabling deployment in terms of generating data to inform code/standard work, education, and outreach.
- Yes, these projects are helping to ensure uptake of hydrogen and fuel cell technologies.
- This technology area is an enabler for the deployment of hydrogen technologies.
- Overall yes, but it is very focused on light-duty FCEVs. Expansion into medium- and heavy-duty FCEV technology application areas may be beneficial for newly supported FCTO areas of interest.

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- One of the key aspects for the success of the program is the fact that the program manager and his team are very engaged with the scientific community, the main standardization bodies, and relevant hydrogen stakeholders, both domestic and international.
- Yes, the SCS program management shows professionalism and a high degree of competence.
- It is clear that there is a strong interface with the other technology areas, aiming at identifying and prioritizing all cross-cutting issues.
- Yes, the projects taken as a whole present some very good work on developing test facilities, test rigs, and testing procedures that will, in the future, support ongoing research and data gathering. The program should make sure each project has clearly focused goals/deliverables for future work.
- Yes (two responses).

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- The strength of this program lies in its focus on addressing real-world barriers that need solutions today. Additionally, many of the outreach and safety efforts have been well maintained and continued over many years. It is essential to keep doing this, as the need for outreach and education across all the states is immense, and building the scalability of the market will require building very widespread acceptance of the technologies. The program should be encouraged to keep these efforts in place.
- In general, the projects have a very integrated approach, and projects are complementary and comprehensive. It is, however, difficult to understand the very complex national standardization system, and therefore a simplification of it is desired.
- The following work stands out at the positive end of the spectrum:
    - Polymer and materials compatibility work
    - Setback distances work
    - International collaboration
- The strengths include the nature of activities, balanced portfolio of activities, international collaboration, comprehensive coverage of safety-related issues, and multidisciplinary and competent partnerships. The weakness is that industry participation could be widened.
- The strengths are the program management, very strong scientific resources, and international collaboration. A weakness is that the program could benefit more with an increased budget.
- The key strength is that the projects mostly have key practical applications in advancing performance-based standards. A weakness is that the future work for some of the projects is not always clearly focused—it would help to try to define/prioritize future key work items with deliverables whenever possible.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Yes, they do. New tools and devices have come out of the program that are not available anywhere else and that required innovative design and application of existing technology on a very limited timescale.
- This program is thinking out of the box compared to other sub-programs with regard to the extent of international collaboration.
- The science-based approach clearly demonstrates novel/innovative ways to approach the main barriers.
- Yes, the projects are of high quality and seem to address efficiently the identified barriers.
- Most of the projects are narrowly targeted (which is good in this case) on specific needed data, so it is not clear how much "novel and innovative" applies. The development of specific test apparatus and test methods was a strong point of these projects and could be classified as innovative in that perhaps similar tools/methods did not exist previously.
- Yes.

10. Has the program engaged appropriate partners?

- Yes, the program involves a variety of partners, which provides the perception that the challenges are being covered by an appropriate and well-balanced number of stakeholders from the private and public sector.
- This program is very integrated and complete, involving all national stakeholders and players. Also, internationally, it plays on all the tables deserving attention.
- The program manager and his team are very engaged with the scientific community, the main standardization bodies, and relevant hydrogen stakeholders, both domestic and international.
- The projects have generally engaged the national laboratories, industry partners, and codes and standards (C&S) groups in both the United States and internationally and received good support. It might be helpful to consider opportunities for more collaboration with academic institutions. There was not much reference to such partners, and it seems that the projects might be missing significant potential expertise and resources.
- Yes (two responses).

11. Is the program collaborating with them effectively?

- Yes, the program appears to be engaged effectively with the partners noted.
- There is no evidence that the program is not collaborating with them in an efficient and timely manner.
- Yes (four responses).

12. Are there any gaps in the portfolio for this technology area?

- There is no evidence of gaps or areas that are not being addressed appropriately. Perhaps additional initiatives focused on safety training devoted to the general public or at least to first consumers could reinforce the program.
- Perhaps, with an increased budget, more work could be done on refueling protocols.
- There are a few gaps: understanding of C&S adoption cycles and patterns in California and the Northeast, underground hydrogen storage assessment and footprint benefits, and safety standards for medium- and heavy-duty vehicles.
- No (three responses).

13. Are there topics that are not being adequately addressed?

- The projects the reviewer reviewed covered the subject topics well.
- Fueling protocols could be an area in which the program could allocate some resources.
- Non-proprietary medium- and heavy-duty hydrogen vehicle fueling protocol development is not being addressed.
- No (two responses).

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- The program should consider funding the assessment and development of safety standards for medium-and heavy-duty FCEVs and the transfer of lessons learned from natural gas.
- Perhaps initiatives focused on increasing the general public’s acceptance and awareness of fuel cell and hydrogen technologies could be considered as a reinforcement of the outreach activities.
- International collaboration on standards development appears to be an area that is missing from the program. It is only recently developing, but as the conversation shifts to increasing volume, the need to capitalize on the potential worldwide nature of the market (especially on the fueling technology) is becoming even greater. There will likely soon be a need to evaluate and even attempt to reconcile various standards and best practices being developed by different organizations around the world. This is an area that seems like it would fit well with the sub-program’s goals.

- In view of the increased interest in another part of the FCTO dedicated to power-to-gas issues, it is suggested that the possibly related safety aspects are taken into account.
- Work on hydrogen storage system materials/designs that can either allow original equipment manufacturers to package 5 kg in a vehicle at lower pressure than 700 bar or that would relax the Type IV tank constraints (temperature) that drive the refrigeration requirements of J2601 could provide major cost benefits in hydrogen fueling infrastructure. As an example, obtaining real-world data on tank temperature during/after fueling events (thousands of them, one hopes) would help understand how often -40°C precooling is really needed to complete a fast (<5-minute) fill.

15. Can you recommend new ways to approach the barriers addressed by this program?

- The project should support underground burial of a liquid hydrogen storage tank project to assess challenges of applying "business as usual" (of conventional fuel industry burying fuel tanks underground at gas stations) on hydrogen fuel.
- The lack of harmonized standards at an international level is still considered one of the main barriers for the commercialization of fuel cell and hydrogen technologies, and therefore, new ways to collaborate at an international level could be analyzed in order to avoid overlaps; to detect synergies and gaps; and to distribute, in an efficient and strategic manner, the efforts among the international actors, maximizing the overall progress in this area. The International Partnership for Hydrogen and Fuel Cells in the Economy Working Group on RCS is a good example, but perhaps there are other stakeholders that could contribute in this field. In line with the previous comment, a specific forum devoted to safety-related issues, such as the International Conference on Hydrogen Safety, could promote the safety culture among the diversity of stakeholders and provide guidance on next steps to be taken in this sense.

16. Are there any other suggestions to improve the effectiveness of this program?

- The program should call out specific areas that could be sped up to make progress toward goals/objectives faster and get results swifter with more funding. Some areas are bound by code adoption cycles, while others may not be. If R&D results are achieved faster, this may result in swifter adoption of revised/created C&S. The program should also hand off the "Hydrogen Tools App" to a volunteer entity that thinks it can benefit from using the application and continuing its use/availability in the market for business purposes.
- The international collaboration shall be a keystone for maximizing the impact of this program by finding synergies and gaps on the different activity areas.
- Given the highly localized nature of permitting projects such as hydrogen fueling stations, it is a little unclear just how widely applicable the permitting guidebook and video will turn out to be. The program should make an effort to reach out to those who have accessed the material and evaluate how effective the material was for their specific applications and needs. If gaps are identified, then there should be consideration of whether they can be addressed through further development of the guiding materials.
- The program should have an increased budget.
- It is not clear whether there are government agency/department counterparts to DOE in other countries that are/could be effectively engaged as collaborators and to compare work and results. In addition to possibly supporting harmonization of standards, it would be beneficial to avoid duplicate efforts.

Market Transformation Program Comments

1. Was the program, including overall strategy, adequately covered?

- Generally, yes, with a good summary of objectives, partnerships, and projects. Other reviews followed the theme(s) presented here with consistency.
- Yes, given the broad range of possible projects/topical areas, the projects covered a good sample. Participation in education/outreach support is an excellent tactic in support of the strategy of the Market Transformation program.
- Yes, the program and overall strategy were covered in detail and easily understood. The strategy was outlined with a statement of the objectives. New strategies were identified, but it was unclear how these strategies would be funded or implemented by the U.S. Department of Energy (DOE).
- Demonstrations and deployments were sufficiently covered. There did appear to be a good deal of analysis work that was not covered as well in the overview presentation.

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- There is a good balance of near-, mid-, and long-term research and development (R&D). The demonstrations of the airport cargo tractor fleet results were given (near term). The demonstration of maritime pier-side power was completed (near term). The FedEx project is under development. Analysis of the business case scenario for idle fuel cell lift trucks provides a mid-term peak shaving project. Business cases for a renewable hydrogen station in Hawaii and the site-specific technical economic analyses for Hawaii, Connecticut, Massachusetts, and New York look to the future for the long-term case. Educational seminars are a needed addition.
- Yes, the focus for the program seems to be appropriately weighted toward the near-to-mid-term. This should be maintained.
- Yes, this balance was generally described through the projects and challenges. Other presentations were consistent with the program overview and themes.
- Generally, yes, although at this point all of the auxiliary power unit, range extender, truck, aerospace, backup power, etc. applications seem to be near-term R&D. It is hoped that the H2-at-Scale focus will inform us of additional high-priority mid- and long-term opportunities.

3. Were important issues and challenges identified?

- Yes, important issues were discussed. In particular, the work shown on developing business cases for early market adoption will likely become increasingly important in the coming years. There should likely be some anticipation of developing the necessary programmatic infrastructure to continue to meet this growing need.
- Yes, particularly in the area of increasing/ensuring demand for fueling infrastructure by the addition of multiple applications, such as range extenders and airport equipment.
- The use of technology readiness levels (TRLs) provides a measurement of the status of the technology and identifies entry points for the technology. It is recommended that manufacturing readiness levels be included in the challenges.
- Yes. However, some of the challenges to identify targets and clusters with the existing and proposed budgets will be difficult to resolve. Other presentations generally followed the issues and challenges.

4. Are plans identified for addressing issues and challenges?

- Yes, in particular, the extensive collaboration with stakeholder working groups seems like it has been, and will continue to be, a valuable resource for the program to continue working on pertinent projects.
- Generally yes, but it would be good to see more detail for deliverables for the projects and any thoughts on future projects that support high-usage infrastructure and H2-at-Scale strategies.
- Yes, but the work will continue to be stretched thin because of budget constraints. Oversight, management, and measurement of individual projects by DOE will continue to be necessary.

- Testing of new technologies at user operating conditions provides a methodology for addressing the issues and challenges, but it was not clear how the testing referenced back to the TRLs.

5. Was progress clearly benchmarked against the previous year?

- Yes, although the key successes (materials handling and backup power), while certainly successful, happened a while ago now. It is good to see the progress/benefits of the Northeast outreach. It would be good to see more detail on what was learned and what might be recommended from the Federal FCEV Fleet Analysis.
- Targets, impacts, and analyses were identified. Explicit benchmarking from previous year(s) was not highlighted; however, such highlighting would have provided limited value. Other presentations provided appropriate benchmarking, indicating appropriate oversight by DOE with a goal for progress.
- The Market Transformation Deployments referenced the previous year's results and identified the increase in deployments for both lift trucks and backup power. The 2015–2016 deployments were done without DOE funding, although the bar charts contained DOE appropriations and combined DOE/industry data. The bar charts could be confusing, and it could be interpreted that funding was supplied by DOE during the current reporting period. Dates on the bar charts would have made the data clearer.
- This did not seem to be addressed in the overview 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?

- Yes, in particular, it is hoped that the range of applications that are being explored by the program will help to develop potential for deployment of fuel cells at large scale, bringing costs down.
- Yes, the projects are of high value and consistent with market needs and trends. Overall, the program successfully accomplishes its goals for market transformation. Other presentations generally followed the theme with consistency.
- Yes, in terms of increasing the number of commercial applications of fuel cells and in demonstrating examples of increased usage of infrastructure. It would be helpful to come up with ideas, maybe through a request for information, on how some of these projects could support gathering real-world data to inform codes and standards.
- Hydrogen stations are critical to implementing the current deployments and the future strategies. The cost of the hydrogen stations and the distribution of the hydrogen stations were not discussed in the charts.

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes, there is clear focus, but at the same time, the program is still innovative. This combination appears to make the program particularly effective.
- Generally, yes, the program appears focused with well-selected targets, intended partnerships, and analysis. Additional reporting regarding the outcomes for market penetration would have been helpful. Overall, the program, with consideration of other relevant presentations, was focused, well managed, and effective in addressing market and industry issues.
- Generally, yes, although it would be helpful to categorize the various projects under some major subheadings (such as Mobility and Renewable Hydrogen) and prioritize them to clarify how projects are chosen. In this area, there are several subheadings and hundreds of possible projects, so it would be good to tie projects back to higher-level enablers.
- The program appears to be well organized, and successful market transformation activities were identified.

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- The great strength of the Market Transformation program projects is the partnerships with private industry. Technology validation under DOE guidance has established a high level of confidence in the future success of industry, which provides a basis for the Market Transformation program. The development of safety, codes and standards is a significant benefit that is necessary for the transition to industry of the fuel cell applications.
- Selection of appropriate topics and projects for analysis to test market transformation is of high value and should continue. Some of the ancillary areas, including state training for codes and standards, cash flow, and fleet identification, were good areas of interest. The direct impact and relationship with the targets was not clearly proven during the presentation but was shown through other presentations that are under the guidance of DOE. Overall, the presentation provided a good overview of program direction.
- A key strength is the gaining of real-world experience in the various projects and the "stretching" of potential fuel cell and infrastructure applications. An example of this "stretch" is how the FedEx trucks are going to get hydrogen (which did not seem very well defined).
- Strengths are in the real-world applicability of the sub-program's projects. A weakness of the program may be the limited number of projects it can take on with its given budget. There are many developing issues as fuel cell deployments accelerate now, and many more will likely appear. While this program addresses a good number, there are already some issues that could benefit from more work, so the future potential for work in this program could be even greater. For example, there is a clear need for business cases or alternative business models not only for fleet operators but also for fueling network operators.

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Yes, the projects were well selected. Additional work to promote the findings for increased market transformation would be of value. This work was described during many of the other related presentations under the guidance of DOE.
- Yes. The reviewer would like to reiterate the benefit of supporting the Northeast outreach efforts as an innovation compared to funding hardware demonstrations.
- Novel and innovative are not necessarily terms for market transformation. Well organized and dedicated to hard work better describe this program.
- Yes.

10. Has the program engaged appropriate partners?

- Collaboration with partners was discussed and is expected to be pursued. Other presentations also provided strong intent to collaborate with stakeholders.
- Yes, collaborations are listed and demonstrated in the presentation.
- Yes. It is suggested that the program consider some additional industries to include because the H2-at-Scale opportunities (utilities, wind, solar, etc.) are meant to build on the current infrastructure projects.
- Yes.

11. Is the program collaborating with them effectively?

- It is expected that the partnerships will continue for effective collaboration. Other presentations also provided information on intent for effective collaboration.
- Yes, based on the success of the deployments, collaborations are effective.
- Yes, the program might consider focusing work with infrastructure providers on how core high-usage fueling infrastructure can support multiple types of vehicles/applications from single sites (consider how one airport site would support cars, trucks, tugs, buses, etc.).
- Yes.

12. Are there any gaps in the portfolio for this technology area?

- No, but the program should consider how to "put together" the various applications so that they support high-usage infrastructure such as at an airport or port. For example, the future look at car-sharing might consider this aspect and examine whether a high percentage of Uber/Lyft trips originate or end at an airport.
- It is perhaps a little bit early, but there did not appear to be any discussion of anticipated work for following and reporting on the light-duty vehicle (LDV) market transformation. Given that the vehicles are starting to come onto the roads, it seems like there will soon be plenty of information that will need to be tracked, assessed, and reported similar to the information presented in the overview for the materials handling and backup power markets.
- Increased focus on transformation targets with stronger reporting on results would be of value. Many of the other presentations provided information to fill gaps consistent with comprehensive program management.
- A discussion of the availability of hydrogen fuel was missing. A discussion of the cost of hydrogen delivery is necessary.

13. Are there topics that are not being adequately addressed?

- No, to the extent that the Market Transformation program is focused (as it should be) on markets that utilize compressed hydrogen gas at the infrastructure-to-product interface.
- The topic of identifying a cluster strategy was identified, but specifics were not provided. Other presentations provided a great amount of information demonstrating the diversity and connection of topics being addressed.
- This may be a cross-cutting project with the Hydrogen Storage program, but it appears that there may be a need for a risk/cost assessment of a potential future shift in the hydrogen storage medium used for vehicles. Public and private entities are currently investing significant amounts of money in compressed hydrogen storage. The Hydrogen Storage program has identified this as only a short-term solution, and none of the mid-to-long-term solutions seem to have much in common with compressed gas. An analysis may be necessary to determine the risk of increased costs or even stranded assets in a hypothetical future shift to one of the other storage media.

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- The analysis of car-sharing/ride-sharing opportunities is good. It is not clear whether there are any other emerging LDV usages or fleet opportunities that might lend themselves to supporting high utilization of infrastructure.
- Market entry and potential growth for electrolyzers seem to be accelerating at the current moment. This may be a technology area that the program should consider adding to its portfolio. In particular, economic and business analyses seem to be a key area for potential research in the technology.
- A reproducible strategy to identify clusters with a focus on market transformation may be helpful for the targeted promotion of the results to stakeholders, including partners. Many other presentations provided substantial information on the vast array of topics being managed by DOE. Overall, the topics are appropriate, connected, and effective in addressing market and industry issues.
- It is not clear when the commercial fuel cell markets will no longer need the tax credit and will be self-sufficient.

15. Can you recommend new ways to approach the barriers addressed by this program?

- The current methods are appropriate.
- Overall, the program is doing a good job of keeping a broad view of opportunities in the space. The program should keep a close eye on the H2-at-Scale initiative, because that may open up some new project areas with potential.

- Increased promotion of findings to stakeholders for reproduction of successful projects is suggested. The sub-program overview was very helpful to identify barriers, but a full understanding of all barriers was only possible after review of several other presentations.
- No.

16. Are there any other suggestions to improve the effectiveness of this program?

- The program should continue its close collaboration with the other FCTO sub-programs and with industry and look at whether there are similar agencies/investigations going on internationally that could inform or support U.S. strategies and projects so that the United States is not duplicating efforts.
- As somewhat of an aside, it seems like there may be an important opportunity for DOE to coordinate with the U.S. Department of Transportation regarding the Fixing America's Surface Transportation Act. It seems like the Market Transformation program would be the appropriate program to accomplish this.
- An increased budget would seem to be appropriate to move some of the R&D to commercialization. While difficult, the program overview might have been used to highlight some additional success stories presented by others.
- The program should provide a cost analysis demonstrating that the cost of the fuel cell power plants is meeting commercial, non-subsidized targets.

Systems Analysis Program Comments

1. Was the program, including overall strategy, adequately covered?

- The program manager did an excellent job defining and presenting the overall strategy of the Systems Analysis program and how it fits in addressing the main targets and challenges of the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program).
- The projects within the program covered a wide range of modeling and analyses required to evaluate hydrogen and fuel cells. Models for customer preference, cost, and greenhouse gas (GHG) emissions cover hydrogen production, hydrogen infrastructure, and vehicles. Models elucidate the benefits of the DOE investments in terms of employment, deployments, and GHG emissions. One area that was lacking was the analysis of criteria emissions or petroleum reduction. It may be useful to develop a graphic showing what space each of the models works in, how they differ, and where there are still gaps. The team's purpose and assumptions should be mapped out as well. There appears to be some overlap between the various models. The work of Melaina, Levinson, and Vijayagopal, although different in some respects, does some of the same analysis. One advantage of the overlapping analysis is that the conclusions of these models can be compared. In many cases, the results are consistent. This is encouraging.
- Yes, it was adequately covered in slide 3 by listing each segment of the strategy and the projects that fall within each category. The only omission was the water life-cycle analysis (LCA) work that is being conducted by Argonne National Laboratory, which is a very important area of study. Fuel Cell Technologies Office (FCTO) barriers being addressed by this program were not presented.
- Yes, maintenance and development of modeling tools were covered well.
- Yes (two responses).

2. Is there an appropriate balance between near-, mid-, and long-term research and development?

- The sub-program's portfolio has an excellent mix of near-, mid-, and long-term analyses. Some examples include the following: for the near term, the low-volume hydrogen production and delivery analysis; for the midterm, the GHG emissions analysis for the emerging hydrogen production pathways; and for the long term, the fuel cell electric vehicle (FCEV) cost-of-driving analysis, comparing it with conventional vehicles up to the 2030 timeframe.
- Yes, one of the key issues is understanding how to build out the hydrogen infrastructure. The analyses consider near-term issues such as effective policies for its growth, mid-term evaluation of the costs with models like the Hydrogen Financial Analysis Scenario Tool (H2FAST), and long-term projections with the work of Rosner.
- By mentioning the 2020 cost target, it is obvious how all the near- and mid-term projects are geared toward reaching that target. It is not clear where technologies need to be in terms of GHG reductions in the long term, so perhaps a cost-per-gram GHG target would be appropriate to gauge the effectiveness of technologies and what needs to be done to attain that target. Alternatively, a longer-term target (beyond 2020) would be helpful to compare status vs. target and guide the development of new projects. In the longer term, it is obvious that the more exotic hydrogen production alternatives are in scope to try to reduce the cost.
- Yes, each timeframe is clearly defined.
- Yes, analysis of fueling at multiple station capacities is an example of projecting long-term costs.
- This is not an appropriate evaluation measure for the Systems Analysis program.

3. Were important issues and challenges identified?

- The work carried out under this program has enabled the FCTO to identify targeted areas for reducing costs, GHG emissions, and petroleum use.
- The program manager clearly identified the main challenges, which are understanding the future market behavior and the very limited availability of data for these type of analysis work.
- Yes, they were accurately reflected on slide 4.

- The primary issues of hydrogen and FCEVs are being addressed with the projects in this program. One area that could use more work is that of hydrogen distribution. The issue that FCEVs will reach a premature saturation due to limitations in gaseous hydrogen delivery to refilling stations should be addressed.
- The issues were mostly identified. Results were well presented, but the volume of data partially obscures highlighting of the key issues and challenges.
- Yes.

4. Are plans identified for addressing issues and challenges?

- Yes, these include significant engagement with relevant stakeholders as well as the utilization of excellent modeling capabilities and leveraging resources from the top experts at the national laboratories, which support these analysis efforts.
- It is good to see that the barrier of inconsistent data, assumptions, and guidelines is being addressed by multiple projects using the same models to develop their analyses. Even more integration of existing software within new models is encouraged so that the assumptions used throughout the program are the same, or if not, the differences are starkly highlighted. For example, it is not clear whether the information from Autonomie is being used in H2FAST.
- Yes, but the plans could have been more directly linked to the challenges.
- It appeared that none of the projects address consumer choice/behavior. Perhaps this is being addressed under a different program to later be integrated into the Systems Analysis program. The program is doing a good job at consolidating data from different models under, for instance, the Macro-System Model and the cradle-to-grave (C2G) models. There is good collaboration with national laboratories to ensure that high-quality data are sourced or generated.
- Plans are not explicitly identified.

5. Was progress clearly benchmarked against the previous year?

- Yes, very significant accomplishments in this year for this program were clearly presented, such as the C2G analysis and its recently published report, the update to H2FAST, the GHG emissions analysis on emerging hydrogen production pathways, the employment study, and the sustainability workshop with relevant stakeholders.
- It was an improvement from last year to see actual selling prices of hydrogen at the station, which was a gap from last year. Looking at the 2015 slides and comparing them against the 2016 slides, it is notable how much progress has been made in terms of patents and jobs creation. It is also interesting to see that there are new projects and new results from ongoing projects (e.g., C2G).
- Yes, obtaining consensus and publishing a C2G study is a significant accomplishment.
- The information shared in the Systems Analysis program presentation appears to be new and does not repeat what was done in previous years. As a result, it is hard to know what capabilities already exist in the models. It may be helpful to provide a short description of the purpose of the models rather than just describing the delta from one year to the next.
- No, analysis results are shown for the current year with updated projections for future years. This is reasonable and in keeping with the analysis theme of the entire effort, but year-to-year progress or change was not tracked.
- 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 program manager has done an excellent job in balancing the project portfolio to address the main FCTO barriers. All of the projects have significant interactions with the industry, academia, national laboratories, and relevant stakeholders, which will be a key enabler to providing very robust analysis work. In addition, the analysis projects are leveraging the use of very well-established modeling tools and top researchers, which will provide clear direction and results for this program.

- Yes, analysis is critical to ensure that the FCTO is working on relevant issues that will enable cost-effective implementation of hydrogen vehicles in a way that addresses national needs. The H2FAST model, which addresses critical issues for investors, is a great addition to the program.
- Yes, the program is assessing the right benefits and evaluating current performance against targets. Systems analysis is needed to assess the potential of new technologies (e.g., solid oxide electrolyzer cell, BDL) on a techno-economic and environmental basis to guide DOE funding on research and development of new hydrogen production technologies.
- Yes, although sometimes the models are not pushed to address the really big questions of the FCTO and instead just provide additional data.
- Yes, in general, the projects are addressing the broad problems/barriers.
- Yes.

7. Does the program appear to be focused, well-managed, and effective in addressing FCTO's needs?

- Yes, the program is focused on assessing the right technologies and fuel/vehicle pathways. Yes, the program is well managed and effective.
- Yes, the program is working well with DOE, original equipment manufacturers (OEMs), and fuel providers to provide relevant, accurate analyses.
- During the presentation, the program manager and his team demonstrated that they are clearly focused on addressing the main barriers and targets of the FCTO.
- With FCEVs now coming into the market, the analysis should focus on the issues of hydrogen infrastructure and back off on the vehicle analyses. Low-cost hydrogen production will continue to be a major issue and should continue to be addressed.
- Yes (two responses).

8. What are the key strengths and weaknesses of the projects in this program? Do any of the projects stand out on either end of the spectrum?

- Strengths are the management, modeling tools, and the researchers/leads from the national laboratories, external stakeholder engagement, and balance of near- and long-term activities. Perhaps the limited budget is a weakness.
- It is great to see that many of the models are trying to validate their results by using available data from California infrastructure, hybrid electric vehicle sales, and the like. It is not clear what work will be done to evaluate sustainability and the utility of this project. It would be beneficial to provide concrete details of what information will be gathered and how it will support the overall program.
- A weakness is that the cost of battery electric vehicle (BEV) charging equipment per BEV is much higher than the cost of hydrogen infrastructure per FCEV by a factor of at least three, but slide 10 shows that charging equipment costs are similar to or even less than hydrogen station costs per vehicle; this needs to be corrected. A strength is showing GHG reductions due to fuel cell technology deployments (slide 18); this is very useful.
- The electric vehicle and hydrogen infrastructure cost analysis results are misleading. Using dollars and miles to compare gasoline vs. electric vehicle charging stations is very misleading. A different metric is needed. The impact of FCTO targets on the vehicle cost project is very weak and not very material. Given that targets for FCEVs are set to be competitive with hybrids, modeling the competitiveness of gasoline internal combustion engines (ICEs) against FCEVs is not particularly relevant. It would be more useful to assess when FCEVs will become competitive with gasoline ICEs on their own merits without assuming that a particular target is met, but rather, using technology assessment as a way to gauge progress to the target. It is hard to make a case for the sustainability framework projects. There are very few cases in which sustainability metrics are required to commercialize a product.
- Job estimates and return on investment calculations are difficult to make. The reviewer is suspicious of the values and thus generally considers them "weak."

9. Do these projects represent novel and/or innovative ways to approach these barriers as appropriate?

- Yes, researchers involved in the Systems Analysis program are well informed of cutting-edge technology and innovations, which they integrate in their analysis when/if the technology shows promise, at least at the laboratory scale.
- The program is fortunate to have a very capable group of analysts who are knowledgeable in the economics and science of hydrogen fueling.
- None of the analysis work seems particularly novel and/or innovative. However, the analysis appears to be focused, on-target, needed, and professionally conducted.
- The projects do not appear particularly novel.

10. Has the program engaged appropriate partners?

- Yes, and this is probably one of the strongest areas of the program; it has extensive interactions and engagement with a broad set of key stakeholders.
- The Systems Analysis program relies on collaborations with third parties to acquire data and targets and to validate models. Collaboration with external stakeholders has been adequate, and the right people have been involved.
- The program has worked well with OEMs and fuel providers to ensure credibility and relevance.
- It may be beneficial to analyze the niche markets that are being evaluated by the Market Transformation program and Technology Validation program. Most of the work done so far has been on mid-size vehicles. Work should be done to evaluate these other markets.
- Yes.
- It appears that it has, but it is difficult to tell from the presentation.

11. Is the program collaborating with them effectively?

- Yes, it appears that collaboration has been effective with external parties and even across offices within DOE (e.g., the Bioenergy Technologies Office). Further, the Annual Merit Review is a good opportunity for researchers working on similar projects to communicate results and share best practices. It is good practice to run preliminary results of the projects by the Fuel Pathway Integration Technical Team and other U.S. DRIVE Partnership technical teams.
- The evaluation of the DOE targets and their long-term impact on the market is very beneficial. This information helps to justify these targets and demonstrate a pathway to successful fuel cell implementation.
- A great example of this is the C2G analysis, which resulted in a publication with significant collaboration between the government, industry, and national laboratories.
- Yes, the program manager has been an effective coordinator.
- It appears so.
- It appears so, but it is difficult to tell.

12. Are there any gaps in the portfolio for this technology area?

- None were detected.
- There is a limited budget.
- It would be good to see Systems Analysis program tools be expanded and used for the H2@Scale Big Idea.
- The Systems Analysis program should use a new metric, for instance, cost per GHG emissions abated. The program should also add low-volume and near-term assessments to the models, including Hydrogen Analysis (H2A). It is useful to assess the future potential cost and technology advancement of technologies, but it is more useful to know where those technologies are now in terms of cost and how the gap between today and the future will be bridged, particularly when future values for parameters such as cost and efficiency are based on DOE target goals. The program should add short-term climate forcers to the technology LCA assessment.
- DOE should be taking a longer and wider analytical view of GHG reduction by broadening the scope of the analyses. In order for the nation to achieve its GHG reduction goals, GHGs must be reduced in both

transportation and electricity generation. While the FCTO is focused on transportation, hydrogen as an energy carrier is uniquely qualified to provide a major contribution to GHG reductions in both electricity generation and transportation. For example, consider the impact of the introduction of coal gasification with carbon capture and sequestration (CCS) to produce hydrogen. Coal reserves are much greater than oil or natural gas reserves in most parts of the world. The United States could, in theory, replace all gasoline and diesel fuel used in transportation with hydrogen made from coal by coal gasification, thereby dramatically improving our national security (a Toyota executive, for example, has suggested that Toyota may stop producing ICEs by 2050, presumably relying entirely on FCEVs). An integrated gasification combined cycle (IGCC) and CCS would significantly cut GHGs in the electricity generation sector compared even to natural gas combined cycle (NGCC) plants. Taking a holistic, societal view of hydrogen use in both transportation and electricity generation could dramatically reduce GHGs while simultaneously decreasing threats to national security. The Office of Energy Efficiency and Renewable Energy should take the lead in conjunction with fossil fuels in analyzing and promoting the use of coal in IGCC+CCS plants to provide both low-carbon electricity and low-carbon transportation via FCEVs, with hydrogen as the main energy carrier.

- Predicting consumer demand for hydrogen FCEVs remains a challenge. Predicting consumer behavior does not appear to be part of the DOE skill set.

13. Are there topics that are not being adequately addressed?

- No.
- Perhaps one of the areas that was not being addressed was related to the sustainability analysis, but the program has taken the right steps to move forward with this (by organizing the sustainability workshop), and it was clearly presented as one of the efforts for upcoming activities.
- While GHG emissions and water usage are being calculated by the models, how this information can be integrated with cost needs to be evaluated. Several different scenarios (carbon taxes, credits, etc.) may need to be looked at. It is hard to couple these less tangible areas with cost.
- Using infrastructure cost in dollars per mile to compare gasoline refueling stations vs. electric vehicle chargers is misleading. This metric does not provide a fair representation of reality. It is difficult to understand the value of some models, such as H2FAST. It would be helpful to present a report of who is using these models and whether they have resulted in investment considerations.

14. Are there other areas that this program should consider funding to meet overall programmatic goals?

- No.
- The Systems Analysis program should use a new metric, for instance, cost per GHG emissions abated. The program should also add low-volume and near-term assessments to the models, including H2A. It is useful to assess the future potential cost and technology advancement of technologies, but it is more useful to know where those technologies are now in terms of cost and how the gap between today and the future will be bridged, particularly when future values for parameters such as cost and efficiency are based on DOE target goals. The program should add short-term climate forcers to the technology LCA assessment.

15. Can you recommend new ways to approach the barriers addressed by this program?

- The program could also benefit by engaging with international stakeholders and their efforts on this space.
- The FCTO should consider the use of agent-based models for looking at consumer behavior, using hybrids, BEVs, and plug-in hybrid electric vehicles to calibrate.
- The program should continue to assess the readiness level and potential of hydrogen technologies throughout the value chain of hydrogen production, transportation, and delivery. The program should continue to engage with national laboratories and academia to integrate results from laboratory tests into models to help provide early guidance to FCTO on funding alternatives. The program should continue

to partner with industry to vet model assumptions and results and to help transition technologies that show promise to commercialization.

16. Are there any other suggestions to improve the effectiveness of this program?

- The funding level for this subject area is low. Additional analysis would be beneficial. Further/more coordination among analysis, vehicles, storage, and production sub-topics would enhance effectiveness of the analysis.
- No, it is a very robust and well-managed program.
- No.

Research and Development Project Evaluation Form

This evaluation form was used for the following Hydrogen and Fuel Cells Program review 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 review 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-112	Reformer-Electrolyzer-Purifier (REP) for Production of Hydrogen [CO2 Pump]	Fred Jahnke	FuelCell Energy, Inc.
PD-117	High Temperature, High Pressure Electrolysis	Cortney Mittelsteadt	Giner, Inc.
PD-118	New Metal Oxides for Efficient Hydrogen Production via Solar Water Splitting	Yanfa Yan	University of Toledo
PD-119	National Science Foundation/U.S. Department of Energy Solar Hydrogen Fuel: Engineering Surfaces, Interfaces, and Bulk Materials for Unassisted Solar Photoelectrochemical Water Splitting	Tom Jaramillo	Stanford University
PD-120	Accelerated Discovery of Advanced Redox Materials for Solar Thermal Water Splitting to Produce Renewable Hydrogen	Charles Musgrave	University of Colorado Boulder
PD-121	Tunable Photoanode-Photocathode-Catalyst Interface Systems for Efficient Solar Water Splitting	G. Charles Dismukes	Rutgers University
PD-128	2014–2016 H2 Refuel H-Prize	Jeff Serfass	Hydrogen Education Foundation
PD-129	Novel Hybrid Microbial Electrochemical System for Efficient Hydrogen Generation from Biomass	Hong Liu	Oregon State University
ST-014	Hydrogen Sorbent Measurement Qualification and Characterization	Phil Parilla	National Renewable Energy Laboratory (NREL)
ST-093	Melt Processable PAN Precursor for High Strength, Low-Cost Carbon Fibers	Felix Paulauskas	Oak Ridge National Laboratory (ORNL)
ST-101	Enhanced Materials and Design Parameters for Reducing the Cost of Hydrogen Storage Tanks	David Gotthold	Pacific Northwest National Laboratory (PNNL)
ST-110	Optimizing the Cost and Performance of Composite Cylinders for Hydrogen Storage Using a Graded Construction (Small Business Innovation Research Phase II)	Andrea Haight	Composite Technology Development
ST-128a	Hydrogen Storage Materials Advanced Research Consortium: Sandia Effort	Mark Allendorf	Sandia National Laboratories (SNL)

Project ID	Project Title	Principal Investigator Name	Organization
ST-129a	HyMARC: Hydrogen Storage Materials Advanced Research Consortium	Brandon Wood	Lawrence Livermore National Laboratory (LLNL)
ST-130a	HyMARC: A Consortium for Advancing Solid-State Hydrogen Storage Materials	Jeffery Urban	Lawrence Berkeley National Laboratory (LBNL)
ST-131a	H2 Storage Characterization and Optimization Research Efforts	Thomas Gennett	NREL
ST-134	Investigation of Metal and Chemical Hydrides for Hydrogen Storage in Novel Fuel Cell Systems	Ted Motyka	Savannah River National Laboratory (SRNL)
ST-135	H2 Storage Characterization and Optimization Research Efforts	Thomas Gennett	NREL
BESH-2001	Metal Oxide-Supported Platinum Monolayer Electrocatalysts for Oxygen Reduction	Radoslav Adzic	Brookhaven National Laboratory (BNL)
BESH-2002	Catalysis and Electrocatalysis for Advanced Fuel Synthesis	Jose Rodriguez	BNL
BESH-2003	Control of Reactivity in Nanoporous Metal/Ionic Liquid Composite Catalysts	Jonah Erlebacher	John Hopkins University
BESH-2004	Multifunctional Catalysis to Synthesize and Utilize Energy Carriers	Tom Autrey	PNNL
BESH-2005	Modeling Catalyzed Growth of Single-Wall Carbon Nanotubes	Perla Balbuena	Texas A&M University
BESH-2006	Room Temperature Electrochemical Upgrading of Methane to Oxygenate Fuels	Bill Mustain	University of Connecticut
BESH-2007	Nanostructured, Targeted Layered Metal Oxides as Active and Selective Heterogeneous Electrocatalysts for Oxygen Evolution	Eranda Nikolla	Wayne State University
BESH-2008	Controlling Structural, Electronic, and Energy Flow Dynamics of Catalytic Processes through Tailored Nanostructures	Talat Rahman	University of Central Florida
BESH-2009	Development of Physically Transparent, Predictive Structure-Performance Relationships for Rational Design of Multi-Component Catalytic Materials	Suljo Linic	University of Michigan

Project ID	Project Title	Principal Investigator Name	Organization
BESH-2010	Computational Design of Graphene-Nanoparticle Catalysts	Ashwin Ramasubramaniam	University of Massachusetts
BESH-2012	Atomic-Scale Design of Metal and Alloy Catalysts: A Combined Theoretical and Experimental Approach	Manos Mavrikakis	University of Wisconsin
BESH-2013	Catalyst Screening and Common Design Principles for HDO: A DFT investigation on pure and promoted MoO3(010)	Lars Grabow	University of Houston
BESH-2014	Fundamentals of Catalysis and Chemical Transformations	Steve Overbury	ORNL
BESH-2015	Nanostructured Catalysts for Hydrogen Generation from Renewable Feedstocks	Yong Wang	PNNL
BESH-2016	Element Specific Atomic Arrangement of Binary and Ternary Alloy Nanosized Catalysts in As Prepared and Active State	Valeri Petkov	Central Michigan University
BESH-2017	Oxide Perovskites as Protonic Conductors: Thermodynamic Stability and Protonic Conductivity	Anil Virkar	University of Utah
BESH-2018	Computer Simulation of Proton Transport in Fuel Cell Membranes	Greg Voth	University of Chicago
BESH-2019	Fundamentals of Hydroxide Conducting Systems for Fuel Cells and Electrolyzers	Bryan Pivovar	NREL
FC-105	Novel Structured Metal Bipolar Plates for Low Cost Manufacturing	C.H. Wang	TreadStone Technologies, Inc.
FC-115	Affordable, High Performance, Intermediate Temperature Solid Oxide Fuel Cells	Bryan Blackburn	Redox Fuel Cells, Inc.
FC-117	Ionomer Dispersion Impact on PEM Fuel Cell and Electrolyzer Durability	Hui Xu	Giner, Inc.
FC-150	Dimensionally Stable High Performance Membranes	Cortney Mittelsteadt	Giner, Inc.
FC-151	Low-Cost Proton Conducting Membranes for PEM Fuel Cells	Hongxing Hu	Amsen Technologies LLC
FC-152	Novel Hydrocarbon Ionomers for Durable Proton Exchange Membranes	William Harrison	Nanosonic Inc.

Project ID	Project Title	Principal Investigator Name	Organization
FC-153	Novel Nanocomposite Polymer Electrolyte Membranes for Fuel Cells	Runquing Ou	NEI Corporation
FC-154	Regenerative Fuel Cell System (SBIR Phase II)	Paul Matter	pH Matter, LLC
MN-015	Continuous Fiber Composite Electrofusion Coupler	Brett Kimball	Automated Dynamics
MN-016	In-line Quality Control of PEM Materials	Paul Yelvington	Mainstream
MT-019	2016 HEF Hydrogen Student Design Contest Winning Project		University of Waterloo
ARPAE-017	A Novel Intermediate-Temperature Fuel Cell Tailored for Efficient Utilization of Methane	Meilin Liu	Georgia Tech
ARPAE-018	Nanocomposite Electrodes for a Solid Acid Fuel Cell Stack Operating on Reformate	Tom Zawodzinski	ORNL/UT-Knoxville
ARPAE-019	Low Temperature Solid Oxide Fuel Cells for Transformational Energy Conversion	Bryan Blackburn	Redox Power Systems
ARPAE-020	Solid Acid Fuel Cell Stack for Distributed Generation Applications	Calum Chisholm	SAFCell
ARPAE-021	Fuel Cells with Dynamic Response Capability Based on Energy Storage Electrodes with Catalytic Function	Yunfeng Lu	UCLA – University of California, Los Angeles
ARPAE-022	A Novel Intermediate-temperature Bifunctional Ceramic Fuel Cell Energy System	Kevin Huang	University of South Carolina
ARPAE-023	Development of an Intermediate Temperature Metal Supported Proton Conducting Solid Oxide Fuel Cell Stack	Dave Tew	UTRC
ARPAE-024	Intermediate Temperature Hybrid Fuel Cell System for the Conversion of Natural to Electricity and Liquid Fuels	Ted Krause	Argonne National Laboratory (ANL)
ARPAE-025	Dual Mode Intermediate Temperature Fuel Cell: Liquid Fuels and Electricity	Carl Willman	FuelCell Energy
ARPAE-026	Intermediate-Temperature Electrogenerative Cells for Flexible Cogeneration of Power and Liquid Fuel	Greg Tao	MSRI

Project ID	Project Title	Principal Investigator Name	Organization
ARPAE-027	Intermediate Temperature Proton Conducting Fuel Cells for Transportation Applications	S. Elangovan	Ceramatec
ECS-001	FC Catalysts	Ted Krause	ANL
ECS-002	User Facilities: What they are and how to access them	Karren More	ORNL
ECS-003	Small Business Voucher Pilot (and CAP) / SBV	Christopher Ainscough	NREL
ECS-004	Performance Evaluation and Testing / Tech Assistance	Bryan Pivovar	NREL
ECS-005	FC Durability / FCPAD	Rod Borup	Los Alamos National Laboratory (LANL)
ECS-006	H2 Production	Kev Adjemian	Idaho National Laboratory (INL)
ECS-007	Manufacturing	Michael Ulsh	NREL
ECS-008	Sensors	Rangachary Mukundan	LANL
ECS-009	Hydrogen Storage and Delivery	David Wood	ORNL
H2REFUEL		Darryl Pollica	SimpleFuel
SCS-010	R&D for Safety, Codes and Standards: Hydrogen Behavior	Ethan Hecht	SNL
TV-016	Stationary Fuel Cell Evaluation	Genevieve Saur	NREL
TV-020	Validation of an Advanced High Pressure PEM Electrolyzer and Composite Hydrogen Storage, with Data Reporting, for SunHydro Stations	Larry Moulthrop	Proton OnSite
TV-021	Material Handling Equipment Data Collection and Analysis	Christopher Ainscough	NREL

Project ID	Project Title	Principal Investigator Name	Organization
TV-024	CSULA Hydrogen Refueling Facility Performance Evaluation and Optimization	David Blekhman	California State University Los Angeles (CSULA)
TV-038	Overview of an Integrated Research Facility for Advancing Hydrogen Infrastructure	Michael Peters	NREL

2016 Annual Merit Review Questionnaire Results Summary

Following the 2016 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	19	9%
National/government laboratory, private sector, or university researcher whose project is under review	49	23.2%
Non-government institution that received funding from the office or program under review	53	25.1%
Non-government institution that does not receive funding from the office or program under review	40	18.9%
Government agency (non-federal, state, or foreign government) with interest in the work	3	1.4%
National/government laboratory, private sector, or university researcher not being reviewed	25	11.8%
Other	15	7.1%
No Responses	7	3.3%
Total	211	100%

"Other" Responses

- *From two respondents:* DOE contractor
- *From two respondents:* Industry
- Foreign company
- Foreign university researcher
- Private company
- Academia
- Advanced carbon fiber company
- Foreign public laboratory
- Private-sector contractor
- Independent consultant
- Retiree

1.2. The Joint Plenary Session was valuable in providing an overview, including the purpose and scope of the Annual Merit Review (answer only if you attended the Joint Plenary on Monday).

The top number is the count of respondents selecting the option. The bottom number is the percentage of the total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	2	20	66	54
1%	1%	14%	46%	38%

23 Comments

- The joint plenary was excellent.
- It provided a very good top-level review from the Secretariat level to the program level that tied the DOE mission and goals to the programs for each office.
- It was very useful. I am funded through Basic Energy Sciences/x-ray scattering; I, made important connections and expect fruitful future collaborations.
- There were big improvements over previous years' joint plenary sessions in which participants sometimes had to miss some presentations.
- Reviewers were great. They asked tough questions in front of everyone, which was good.
- It was good to see what technology DOE will fund for future development.
- It provided a good overview.
- It was very helpful for a first-time attendee.
- It helped to identify the overall priorities.
- Listening to Senator Dorgan was enjoyable.
- No issues.
- The presentations are needed before the meeting. Some revisions are okay.
- There were good speakers, but it was too long—this set a bad precedent for the rest of the meeting.
- There was too much material for the time available.
- The means and strategy to achieve the vision were not clearly articulated.
- *From eight respondents:* I did not attend the joint plenary session.

1.3. The two plenary sessions after the Joint Plenary Session were helpful to understanding the direction of the Hydrogen & Fuel Cells Program and/or Vehicle Technologies Office (answer only if you attended the Hydrogen & Fuel Cells and/or Vehicle Technologies plenary sessions on Monday afternoon and/or Tuesday morning).

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	3	18	73	57
1%	2%	12%	48%	37%

24 Comments

- Great presentations from each office with good details on the research being funded in each technology area.
- The information was helpful in understanding technology trends because some presentations showed future technologies.
- It was good to get a "big picture" of where the Vehicle Technologies Office (VTO) is and where it is planning to go.
- The plenary talks provided good information and vision on VTO programs.
- They showed that a wide breadth of research is presently being performed.

- More specific subsections would be helpful. The reviewers were absolutely great. These sessions should not be parallel. It forced participants to choose one or the other.
- These are probably more useful than the main plenary session for regular attendees.
- The reviews were helpful in understanding what each program does and what it focuses on. There was a lot of material for some programs to cover in 30 minutes.
- These are useful for identifying priorities. The downside was that they did not necessarily allow time to identify projects that were important, but were not mentioned because they are be smaller or outside of large priority areas.
- No issues.
- It was a great overview, but the progress presented was a bit oversold.
- The rooms for the plenary could have been slightly bigger. It was difficult to get a seat. All the parallel sessions had adequate seating.
- The VTO plenary sessions on Tuesday morning ran faster than scheduled. Since I had to alternate between the FCTO and VTO plenary sessions, I essentially missed the first half of the VTO sessions. This was caused by the first presenters finishing in 20 minutes and handing the floor over to next speaker right away.
- Need to revise the presentation format to include the following sections: role; tangible objectives; accomplishments; and vision forward (near-, mid-, long-term).
- A shockingly high number of funded projects seem to have very little potential for commercialization. The focus was on past accomplishments vs. future direction.
- There is no overlap.
- *From seven respondents:* I did not attend either plenary session.

1.4. The program overviews were helpful to understanding the research objectives (answer only if you attended one or more program overviews).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	1	7	92	73
1%	1%	4%	53%	42%

16 Comments

- These overview presentations are extremely helpful for the audience to understand all the programs, including mission, interests, and research foci.
- Great presentations from each agency conducting research with good details for each technology area.
- The overviews do set the stage for what participants will see and why the research was done.
- The reviews were helpful to understand what each program does and what it focuses on. There was a lot of material for some programs to cover in 30 minutes.
- No issues.
- I attended several of the overview presentations. It would be good to highlight the overview presentations in the AMR program, perhaps with bold lettering.
- For major priorities, yes, this was useful. For additional, smaller research areas, this was not as useful.
- Objectives were stated; the strategy was not presented.
- These presentations are largely reviews of objectives with which I am already familiar.
- Most reviews provided an adequate amount of data, but more was available that was not disclosed. It is understandable that companies are trying to protect their intellectual property. It is not clear how to solve this.
- Content on future direction was minimal. There was too much focus on past accomplishments.
- The room where the plenary sessions were held was not ideal. There were many seats with obstructed views, and I was not able to see the presenters. The extra video monitors were helpful in viewing the presentations, though.

- *From three respondents*: I did not attend the program overviews.

1.5. The working lunch at the 2016 AMR was significantly different than in previous years. It included multiple brainstorming working lunches, a lunch poster session, and a bag lunch as opposed to a sit-down lunch. The awards were not held during lunch. Please indicate your preference for the AMR lunch program, and share your comments relative to the new lunch format.

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

	Not Interested	Neutral	Somewhat Interested	Highly Interested
Brainstorming Sessions	33	52	62	36
	18%	28%	34%	20%
Poster Sessions	17	45	57	64
	9%	25%	31%	35%
Bag Lunch	39	54	51	37
	22%	30%	28%	20%
Other (please specify under Comments)	4	19	8	12
	9%	44%	19%	28%

59 Comments

- *From three respondents:* the working lunches were a great way to have time to talk with colleagues, brainstorm, and network. This format was good, so it should be kept.
- *From three respondents:* Holding the awards during lunch was good.
- *From four respondents:* The bagged lunch did not seem to be a very good idea. It forced people to stand while eating and also did not provide enough food for everyone.
- The brainstorming session was a much better use of the lunch hour. It facilitated networking and discussion much better than the lunch presentations from previous years.
- The group lunches are a good opportunity to network and have detailed discussions with various colleagues. Arrangements that facilitate this are good (e.g., there should be plenty of tables for different groups of colleagues to mingle at lunch).
- The blended networking and brainstorming opportunities were fantastic.
- The networking opportunities were immensely enjoyable.
- Simpler lunches are good. In the past, the award lunches and various presentations were of interest to only a few. It is much better to have time for discussions and interactions.
- The brainstorming approach provided good cross-talk at the tables and was better than someone trying to give a presentation while people are eating and talking. The only drawback would be that the results of the brainstorming at each table may not have been recorded on the feedback sheets provided.
- The brainstorming working lunch was preferred over the previous years' awards. However, a bit more instruction during the lunch regarding the group brainstorming would have been appreciated. Also, more specific questions would have made for easier discussion. The generalness and open-endedness of the questions left the exercise a bit too vague.
- These sessions were much more useful for discussions of research and research needs with colleagues. This lunch was much better than the awards, which serve only a few, and much better than having one person on stage trying to get the participants to all be quiet.
- The poster lunch was pretty good, but the overall schedule felt somewhat tight. There were a lot of talks and poster sessions running one after the other with few breaks.
- It was good to be able to network and talk to researchers over lunch to discuss projects. It is difficult to find time otherwise. It was difficult to focus on answering the brainstorming questions during lunch, but it is good that it was only one question per lunch.
- Really enjoyed the format of the lunch, but during the boxed lunch session they did run out of sandwiches.
- Thanks for not having the awards over lunch.

- No issues.
- The brainstorming sessions worked well, but it would have been beneficial to have a speaker or even a slide announcing that there was a card at the table to fill out. The bag lunch presented administrative challenges, particularly with seating arrangements. If bag lunches are held again for future reviews, including more seating close to the posters is suggested.
- Holding the awards during lunch is not my preference; the format this year was good. Lunch breaks are a great time for interaction with other attendees.
- While I liked the awards at lunch, presenting awards at the plenary is better because people could not be quiet during the presentation of awards during the lunch.
- The working lunches did not seem to stay on topic; the poster session lunches were beneficial.
- The poster sessions at lunch were a good idea. However, the execution of the bag lunch was poor. Either the number was way off, or the initial people took more than they were allotted, and many people were left searching for food. Since the items were not in bags or boxes, it is possible people did not know what one serving was supposed to include. If they were pre-boxed or bagged, it probably would have gone better.
- The brown bag lunch was not well organized, as food ran out before everybody arrived. Having the posters with the lunch was good, but this should be extended, as there was not enough time to attend poster sessions and to eat lunch (even if not reviewing the posters).
- The bag lunch/poster session is a reasonably good idea, but it was poorly executed with insufficient lunches and seating. It would be nice to sit and eat during such an event.
- I do not have a good reference for previous years, since I have not attended in the past. The bag lunch was fine, but there was no designated area to eat it.
- It was a good idea to hold the awards separately and not during lunch. The concept of having a poster session at lunch was good, but the bag lunch process was not well organized.
- Perhaps people should be assigned to tables based on an interest that they showed during registration.
- Perhaps attendees can pay for lunch instead. Most people have not seen each other in a while, and they use lunch time to catch up since most of the day is packed full of information. Actual work/brainstorming has occurred at the poster sessions. At a sit-down lunch, people are fixed and cannot freely mingle and brainstorm.
- The poster session presenters need to know they need to bring water and it is difficult to get a snack.
- The poster session during lunch was not interesting. A sit-down lunch is better because it gives the opportunity to talk to people. Brainstorming is not very well organized. A professional is needed to run these activities.
- The brainstorming session did not work at my table (i.e., no one paid attention). The poster sessions were fine, but some people did not receive any of the bag lunch. The Wednesday evening poster session had very meager meals; some participants left early to go to restaurants.
- I suggest that there be no bag lunch next year. Many folks came late because they were busy doing networking at/after the presentations and ended up with no food or paying for their own lunch at hotel restaurant.
- The bag lunch had some major flaws: (1) no details were given regarding the food, and many people took more food than they should have; (2) there were not enough tables (high and low); (3) there were not enough chairs, so many people had to sit on the floor, even people in their 60s; and (4) there was not enough food, and many people left without any. This concept may work if it is better organized and thought out. The brainstorming lunches were okay, but the questions were too general.
- The brainstorming topic should be communicated earlier (e.g., at the overview session) so that the attendants have some time to digest and think about it. It is hard to get something meaningful just in the 20–30 minutes during lunch. It might be a good idea to create a website for the brainstorm questions. The lunch poster session was good for additional social time, but it also makes a long day even longer. It is not clear why this was not done at an evening poster session. There were not enough tables and seats during the lunch poster session.
- I got no lunch Tuesday or Wednesday and know a bunch of other people who did not either. The room for the plenaries was way too small the second day—for the second year in a row. It would have been much better, to have the plenaries the second day in the big room and the lunch downstairs that day.
- Too many people took multiple sandwiches and salads, filling their bags to capacity. The lunches should have been pre-bagged and labeled to avoid this chaos.

- It was very difficult to eat the bag lunch while standing around. There were nowhere near enough chairs and tables. I ended up returning to my hotel room to eat, which completely defeated the purpose of the bag lunch poster session.
- It is not a good idea to have a poster session during lunch for two reasons: (1) some people have to present at the poster session and again right after the poster session, which makes it impossible for them to have lunch, and (2) most people are more interested in having lunch and interacting with folks than going through the posters.
- Lunch was not well organized. There was not enough seating on the first day and not enough sandwiches on the second day.
- The Tuesday brainstorming session was not effective because the brainstorming question were too broad. It was hard to come up with a meaningful answer.
- The bag lunch was too chaotic. Since people moved around between locations, it was difficult to balance the amount of food for each distribution location. Also, there was not enough seating for the bag lunch, or at least it was not obvious where to find seating.
- Lunch was chaotic with no directions. It was spread across two rooms, and few tables and chairs were available. Finding associates with whom to have discussions was difficult.
- Lunch was crowded and chaotic. It is not clear how to fix this.
- There was too much plastic for the bag lunch. It was not environmentally friendly.
- The lunch room was noisy.
- The bag lunch was a disaster. In addition to running out of food, putting the buffet tables near the posters did not have the desired effect. People just put food in their bag, walked into the adjacent rooms, sat down, and ate their lunch, since it is difficult to stand and discuss a poster while holding a salad or sandwich and a drink. I was not aware of the brainstorming working lunches.
- I had to go outside to get lunch, which meant I missed out entirely on the posters.
- I only attended Tuesday and Thursday lunches, so missed the bag lunch session.
- *From three respondents:* I did not attend the working lunches.

1.6. 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	109	51.6%
Presenter of a project	54	25.5%
Peer Reviewer	41	19.4%
No Responses	7	3.3%
Total	211	100%

(Continued next page)

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 number is the percentage of total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Presentations	1	1	7	54	39
	1%	1%	7%	53%	38%
Question and answer periods	2	1	9	50	38
	2%	1%	9%	50%	38%
Answers provided to programmatic questions	1	0	17	59	22
	1%	0%	17%	60%	22%
Answers provided to technical questions	2	2	16	55	25
	2%	2%	16%	55%	25%

7 Comments

- It would have been nice to have more time, because in some cases the reviewer questions took all the time in the question and answer period.
- For some projects, the presenters were simply unprepared to respond to the questions presented to them.
- It seems sometimes criticisms were misunderstood most likely because of the differences in the backgrounds of the presenter and reviewer. For example, chemists and physicists (or theoreticians and experimentalists) tend to see and explain things in different ways.
- The general consensus of the people I spoke with seemed to be that the fuel cell work is way too focused on the same few companies/teams with the national laboratories The funding focus should have pivoted more to those who are working on manufacturing fuel cell materials today. It is still too research-focused. Too many materials are moving offshore.
- Not enough information was released by presenters to justify the funding. Some bigger programs should be given more time for presenting. Reviewers were nicer to big original equipment manufacturers than they were to small startups. Anonymous questions should be sent by reviewers online spontaneously.
- Far too many presentations contained "obscured" methods and results. This is especially egregious when the obtained performance is referred to only as "greater than the milestone" and numeric estimates are not provided.
- Not enough data were presented.

2.2. Enough time was allocated for presentations.

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	4	9	56	33
0%	4%	9%	55%	32%

7 Comments

- This was very good: the time per presentation and question-and-answer session (Q&A) was nearly always strictly respected.

- There was good overview in the presentations and lots of time afterwards to follow up. The flash drive with the materials set was awesome.
- The monitors showing the time remaining worked very well, and both presenters and DOE session managers were cognizant of the clocks.
- While many presentations I attended ran over their time allotment, the issue was probably with presenters trying to provide too much information, rather than insufficient time.
- There was enough time for the presentations, but presenters consistently tried to include too much in the slides and cover too much. It should be reinforced that they should cover a few key technical points that fit in the twenty minutes and point to any additional information in back-up slides.
- Maybe five more minutes in some cases would have helped.
- Fifteen minutes would be better.

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	1	13	60	23
2%	1%	13%	61%	23%

10 Comments

- Some questions were very appropriate, and others were rude and unhelpful.
- It varied. Some presentations had few reviewers and few reviewer questions.
- There should be more reviewers from industrial practitioners.
- There was not enough time for reviewer questions in almost all of the talks I attended. Perhaps reviewers should pre-submit questions based on the presentation so that these can be addressed in a timely manner in the oral presentation.
- The reviewers tended to ask harder (and better) questions of external presenters (non-DOE). An equal level of rigor should be expected.
- Occasionally, reviewer questions were not detailed enough for the presenter to understand; however, it most likely occurred because they came from different fields. Trivial basics for a chemist are not trivial for a physicist. This has to be addressed in order to have a fair evaluation. The meaning of rigorous tends to be different between a physicist and chemist, or an engineer for that matter.
- Often the reviewer questions were already addressed in the presentation or were outside of the project scope.
- There was too much restricted information.
- Some reviewers were a bit harsh.
- Only once or twice did I hear questions such as “Okay, so what?” or “What problem are you solving?” Reviewers were trying to let everyone know how smart they were. And not once did I hear “Where did all the rest of the money go?” or “Are these results as fast as you can go?” The presenters got off too easy sometimes.

(Continued next page)

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	94	44.5%
Too frequent	4	1.8%
Not frequent enough	1	<1%
No opinion	4	1.8%
No Responses	108	51.1%
Total	211	100%

1 Comment

- It is hard to get everyone together for a week, so once a year is good.

2.5. Logistics, facilities, and amenities were satisfactory.

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	2	4	48	48
0%	2%	4%	47%	47%

15 Comments

- *From two respondents:* The hotel is very expensive.
- Very well organized and well run.
- It was nice to have a bit more space than in the Crystal City location.
- Room temperature was not cold as usual, so it was very comfortable. But sometimes the room capacity was not enough.
- There was only one set of rooms that was difficult to find.
- Some space should be left between chairs in the meeting rooms.
- A venue with Wi-Fi would be good.
- A bigger room for the second session of plenaries was needed, as was more food (the food ran out for lunch two days in a row—for the same people).
- Although the hotel seemed to be a good venue for the AMR, there was no Internet available in the guest rooms, which made working in the hotel difficult. This issue needs to be overcome in the following years to make sure that guests can use the Internet in their rooms without additional charge.
- It was too cold. Reducing the air conditioning would save energy.
- I got sick because of the occasionally too-strong air conditioning.
- Car parking fees of $45 are too high.
- This participant could not get a government-rate room at the meeting venue. A colleague said that the sleeping rooms were subpar. The meeting rooms were fine, and the full-time audio/video (AV) and door monitor staff in the room helped the sessions go smoothly. The sit-down lunch was crowded and claustrophobic with 10–12 settings per table and little space to navigate between tables.

(Continued next page)

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	11	11	48	30
1%	11%	11%	48%	30%

24 Comments

- *From seven respondents:* Many presentations were too hard to read from the rear of the room because of small font, driven by presenters trying to put too much data/information on each slide.
- *From two respondents:* Having copies of the presentation on a thumb drive was very helpful. I mainly followed along on a laptop during presentations.
- *From two respondents:* The projection screens need to be higher for everybody in the technical presentations to see.
- Most rooms had adequate visual quality, but one room was very long and narrow, and it felt like the screen was really far away.
- In the lower rooms, the screen was small for the size of the rooms. This can be easily fixed for the following years.
- A lot of material was packed into a few slides and it was a little tough at times to see, so I brought my laptop and followed along quite well. The hotel should have had Internet connectivity everywhere for free.
- There is too much information crammed onto each slide, making it difficult to see/read.
- The plenaries were the worst, with half the top banner cut off from the top. Smaller rooms had fewer issues. There is a simple fix: running through the slides ahead of time in the room where they will be shown. Also, the stage lights in the plenary room on the first day were blinding for the speakers.
- Some of the rooms were long and set up with seating far away from the screen. It made it difficult to see the details of the presentation and hear the presenter speak.
- The template requires slides that have far too many words, which leads to small fonts, graphs, and photographs. The information is often duplicative.
- Some presenters do not know how to make a captivating presentation. A lot of slides were filled with bullets and had no visuals to soften the view and engage minds. If the viewers are not 100% interested in the topic presented, it can be a struggle to follow along.
- A few rooms are long and narrow, but there is only one screen. It was very hard to see the slides from the back. Adding screens or suggesting a minimum font size to presenters is recommended.
- I had to look around the person in front of me to see the screen while trying not to block the person behind me.
- Bigger screens would be beneficial.
- Some rooms had columns blocking the view.

(Continued next page)

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	3	50	47
0%	0%	3%	50%	47%

7 Comments

- *From two respondents:* Good job. No issues.
- The full-time AV helper was quick to make sure microphones were working and available for speakers.
- The AV team did a great job.
- The public address system could be better.
- Some of the reviewers did not speak loud enough.
- Some of the rooms were long and set up with seating far away from the screen. It made it difficult to see the details of the presentation and hear the presenter speak.

2.8. The meeting hotel accommodations (sleeping rooms) were satisfactory.

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	5	21	23	38
1%	6%	24%	26%	43%

19 Comments

- *From six respondents:* I did not stay at the hotel.
- *From two respondents:* I was not able to stay at the hotel because there were not enough rooms.
- *From two respondents:* Costs were too high, and I decided to stay elsewhere.
- Excellent hotel and location.
- Good hotel but expensive.
- The rooms were satisfactory, but Internet was not available.
- Rooms booked up very quickly this year, and many had to use less convenient lodgings.
- I could not get a government-rate room at the meeting venue. A colleague said that he saw a mouse in his room at the Marriott and that the rooms were subpar.
- The hotel conference area was good, but the sleeping rooms were terrible. There was a mouse in my room, and the bathtub clogged during showers. The televisions are 15 years old.
- There was a mouse in the room on the first night. The new room’s air conditioner had a large temperature fluctuation, and the room got very dry.
- The hotel room was a bit dated and way overpriced.
- Too expensive.

(Continued next page)

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	3	5	46	44
0%	3%	5%	47%	45%

3 Comments

- The website was great.
- Based on the information I received when registering, I thought there was going to be a meeting app to download and use for scheduling, but it turned out it was only for signing up to an account with the meeting host service. The Portable Document Format (PDF) schedule and information provided on the AMR website were more helpful.
- Internet connection was poor from various personal computers. Once in a while, it connected. The reason for this is not clear.

2.10. What was the most useful part of the review process?

59 Responses

- *From eight reviewers:* The presentations.
- *From four respondents:* Networking.
- *From three respondents:* Questions and answers.
- *From two respondents:* Plenary overviews.
- All slides are available to the attendees.
- Electronic copies of the presentations prior to the review.
- The presentations and the availability of the presentations via the Internet and USB.
- The breadth of information that was covered, as well as the ability to connect across the agencies. With technology at this stage of development, it is crucial to bring everyone together on a regular basis to understand everyone's overlap as well as "underlap." It is great that it is an open forum.
- Hearing reviewer feedback and questions about the project. The experienced perspective was helpful in understanding and analyzing the projects.
- Ability to hear from DOE leaders and principal investigators (PIs) directly about research projects and ask questions.
- The presentations were quite clear and precise and the networking and discussions in the breaks were useful.
- The AMR is an excellent time to see the progress in the field, a motivation to continue to make progress in this participant's own program, and an excellent time to discuss projects with peers.
- Great and relevant content. Great exposure to see the work in progress. The quality of the reviewers was impressive.
- Learning about the state of technology and networking with colleagues.
- Seeing new information.
- Hearing about the direction of the programs and meeting with people.
- The opportunity to see the latest projects and network with fuel cell personnel.
- The presentation of the technical progress of each PI.
- Concentrated session organization (for example bundled by motors or power electronics).
- Gaining insight on the advancements of hydrogen technology and the future possibilities.
- Opportunity to meet with DOE and attendees to discuss the projects' approach and progress.
- Technical presentations, poster session, and ability to talk to people.
- Opportunity to learn about many projects in progress at DOE.

- Information sharing and networking driven after/prompted by the reviews.
- Compact update with key findings and status of the projects.
- Learning about projects.
- Political insights. Networking.
- The breadth of topics and the chance to choose.
- Getting to know the programs that DOE is sponsoring.
- Seeing the program's projects and gaining a better understanding of the program's direction and interests.
- Presenters got real feedback from experts and the broad public.
- Understanding the big picture, the shared goal, and the role of individual projects.
- The review discussions, content, and networking.
- The announcements regarding new request for proposals that are burgeoning and interacting with new colleagues who may become collaborators.
- The condensed format with rigid time control facilitated exposure to the broad set of projects.
- Poster sessions and technical review presentations.
- Gathering all stakeholders in the DOE program in the same forum.
- Networking and getting quality feedback on the projects.
- The presentations, the Q&A, the posters, and the breaks to network were the most useful.
- The overview recap at the beginning of the sessions was really enjoyable.
- To learn the scope and the breadth of work, and to meet stakeholders.
- The technical reviews were the most relevant part.
- The presentations and following discussions during the poster sessions (at night).
- Open discussions.
- Interacting with others.

2.11. What could have been done better?

42 Responses

- *From two respondents:* The title of each of the presentations did not always accurately reflect the content. It would be helpful if a better description of the content or scope of the project was provided in a listing ahead of time.
- *From two respondents:* This participant was very satisfied with the entire meeting.
- Nothing. Everything was great.
- The viewers could ask more questions.
- Give input opportunities to and by practitioners.
- More time for Q&A by the audience, after official reviewers were finished.
- More questions or comments about the performers should be collected through the website and be taken into consideration in the final review.
- Perhaps have a standing microphone for the audience to ask questions.
- Try to group similar program presentations on the same floor of the hotel (e.g., VTO presentations on the one floor and Hydrogen and Fuel Cell presentations on another).
- Keeping similar technology presentations closer. This was done, but even closer could be better.
- Last year it seemed that the sessions for transportation and hydrogen were more separated, so it was a bit easier to meet people of the same group. This year, it was more mixed (this has, of course, advantages as well).
- Meeting room locations could be better arranged/organized. It was hard to find some of the meeting rooms and some meeting rooms were far away from each other.
- On the second day, a bigger room for plenaries was needed. There should be sufficient food for attendees or else tell them to buy their own lunches so they don't waste time looking for lunch.
- The food in the lunch bag was good. It was just awkward to eat it. Perhaps have a shorter sit-down lunch with a poster session.

- I was always able to find a seat, but not everyone was, and it seemed like there was a too-wide aisle in some session rooms that could have accommodated another chair in each row. It is great that meals were provided, but the lunch sessions were chaotic and crowded.
- When there is a major dispute between presenter and reviewer, there should be a formal procedure for the presenter to respond. The review should not focus on grading the project but on giving constructive feedback in order to re-direct the project. The former seems to lead to competition over resources (funding), not outcome.
- The reviews are too cursory, almost collegial, given the amount of taxpayer money spent on these programs. Reviewers need to take a critical look at whether the money is being spent wisely and whether the approaches are appropriate to the objective and will lead to useable outcomes.
- Reviewer quality seemed quite low. At times it appeared that it was the same group of reviewers. Most presenters were also reviewers, and they kept supporting each other. Reviewers that are independent and knowledgeable of the subject matter are needed. Reviewers should be selected by someone other than program managers.
- Internet access could be better. A little less focus on the national laboratories would be better. Money goes in, but very little comes out, and even less gets to production. The process needs to be streamlined at national laboratories. The voucher program does not help get money to startups—it just propagates the status quo at the laboratories. More focus on economies of scale manufacturing is also needed.
- Further separate the VTO and the Hydrogen and Fuel Cell sessions/conference rooms.
- The size of the meeting rooms was too small during the two first days of the event.
- Presenters should be allowed to add more recent data, since most had to submit slides two months in advance.
- Multiple presentations on the deep dive programs could have been consolidated into one concise presentation.
- The opening standard slide—the one with budget and timing, etc.—is boring. Maybe AMR needs a standard template that is more exciting.
- Maybe this participant is just not tuned into them, but the poster sessions seemed distributed and unclear.
- Poster networking.
- The bag lunch during the Wednesday poster session; several people were not able to eat.
- Lunches seemed disorganized.
- The projection screens need to be higher for everybody in the technical presentations to see.
- Follow the schedule; do not start early. This will allow attendees to bounce between rooms.
- The font size for affiliation on the nametags should be the same as for the name.
- Visual quality.
- Make slides more readable (font is too small).
- More standing tables for the bag lunch.
- Seating at the bag lunch.
- The lunch poster session should either be eliminated or organized better.
- More breaks.

2.12. Overall, how satisfied are you with the review process?

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
2	1	8	53	36
2%	1%	8%	53%	36%

2 Comments

- This was a very good AMR, and attendees were generally pleased.

- The concept of open review of federal funding is very good and would work, provided the reviewers are independent. Reviewers should not be chosen by the program managers who are allocating projects and funds.

2.13. Would you recommend this review process to others?

	Number of Responses	Response Ratio
Yes	93	44%
No	5	2.3%
No Responses	113	53.5%
Total	211	100%

6 Comments

- Already recommended this process in Europe.
- These events are needed. Industry benefits enormously from the learning and networking.
- It is a great chance for an overview of projects.
- It might be better to have smaller, individual merit reviews for Hydrogen and Fuel Cells and VTO.
- Some of the reviewers' comments were harsh.
- Too much of the material is repetitive year after year: same milestones, collaborators, background, justification, etc. There is not enough time for accomplishments.

2.14. Please provide comments and recommendations on the overall review process.

17 Responses

- *From two respondents:* Great event.
- It was fantastic.
- This was my first trip to AMR, and I was a non-funded participant. The AMR was very informative and offered great networking. I will be back and will recommend it to others in the new technology, advanced materials, and processing fields, as well as in heavy truck efficiencies.
- The registration check-in process went extremely smoothly.
- This event is always very impressive. It is the all-in-one shop for energy technology, to meet researchers and hear/see progress. The organizers should charge a fee (a few hundred dollars) to help defray the cost of provided meals.
- The distribution of all presentations upfront is extremely helpful and a good contrast too many meetings.
- Looking forward to attending future events. This was an excellent meeting.
- It was good that the hotel had more entrances and exits open so it was easier to move around than at prior meetings. The easier access to the Metrorail with a simple ramp was also appreciated.
- It is not clear whether there is a less expensive hotel alternative.
- On the nametags, the font for the affiliation should be the same size as for the name.
- More focus on U.S.-based manufacture of fuel cell system components, membrane electrode assemblies, and station components. Getting to economies of scale is needed now, and the technology can be refined in the process. I do not want to see another study on where stations need to go. Ten years of street model funding and planning is enough.
- Hold the AMR meeting closer to the completion of the fiscal year in which the presenters are reporting. Ensure that presenters hold to the DOE presentation format.
- A simple approach is suggested: (1) reviewers should be chosen based on their knowledge of the subject, (2) reviewers should be selected by program managers not allocating projects, and (3) reviewers should be from customers, i.e., the industry that is intended to benefit from the project.
- There may be room to improve communication between DOE and reviewers. Some reviewers do not seem to have a good idea about the new systems that DOE is introducing (e.g., EMN, consortium), which operate

in a different way. Also, presenters should be given an opportunity to respond to reviewers outside of the AMR.
- The bag lunch was a terrible idea.

3. Responses from Reviewers

3.1. Information about the 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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	1	19	17
0%	3%	3%	50%	45%

7 Comments

- Guidance was very good. Training and presentations to be reviewed were provided well in advance.
- Having both the 2015 and 2016 presentations was very valuable to determine actual progress (or slippage).
- No issues.
- As always, the information was provided in advance. The only issue was the timing of the overview presentation availability—these were available largely last-minute (as usual).
- This was my first time to review the projects. In the future, I will print the presentations out and review them prior to the meeting. I would want at least two weeks to review them prior to the meeting, especially given that I had eight projects to review.
- PDF documents cannot replace face time with the PI, and although the quality of the slides was good, they left many questions.
- Too much information—too many emails. Reviewers just need to know when they are reviewing and in what room. The details of all the sessions are of some interest—but not much.

3.2. Review instructions were provided in a timely manner.

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	0	0	17	20
0%	0%	0%	46%	54%

7 Comments

- The written and oral training sessions were made available well in advance.
- The instructions were excellent, as always.
- No issues.
- The webinars that were held in advance were appreciated, as they helped me prepare for the meeting. I did not understand how soon the presentations were made available, and how much preparation time I would need to go over them prior to the meeting.
- The reviewer webinars were held at reasonable times.
- Too much information was provided.
- I have reviewed before so did not attend the instruction sessions.

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	2	5	21	9
0%	5%	14%	57%	24%

15Comments

- One could always say more time is needed, but it is not practical given the large number of presentations and four-day duration. The 20+10 format (and allowing the presenter a few minutes grace) should be continued. Presentations were technical and informative. Overall, nice job.
- The organization of the presentations was very good for the time allowed and allowed for the presentation of all aspects of the project.
- All presentations were consistent in their construction, making evaluations and comparisons much easier. The effort by DOE and the PIs is appreciated.
- As always, it was well done.
- In most cases this was true, and the presenters did a good job of answering questions.
- In most cases, yes. A few were lacking in detail.
- No issues.
- Yes, except for the case of models, in which case it is impossible for the reviewer to validate the assumptions and calculations.
- In one or two instances, presentation content could not be understood without a presenter explaining it.
- Some of the information was truncated, particularly with posters.
- Some had better information than others.
- Many presentations were very short on data. This is especially troubling in national laboratory and academic talks. While the need to keep proprietary data secret is understandable, some company work was egregiously void of content. It is suggested that the team or person paying for or monitoring the work review the slides and demand content at an appropriate level as a condition of further funding of the project.
- The short length limits information.
- The time that the investigators spent on the slides is appreciated, but in some cases the information was inadequate. This may be due to the fact that some projects had just begun, while others had some data to report. Projects that did not have clearly stated goals and metrics were difficult to review, as the approach did not make much sense.
- Some of the presenters spoke in terms and at a pace that only those involved in the project could understand.

(Continued next page)

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 number is the percentage of total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance/Potential Impact	0	1	2	19	16
	0%	3%	5%	50%	42%
Approach	0	1	2	16	19
	0%	3%	5%	42%	42%
Technical Accomplishments and Progress	0	1	2	16	19
	0%	3%	5%	42%	50%
Collaboration and Coordination	0	1	4	16	16
	0%	3%	11%	43%	43%
Proposed Future Research	0	2	1	18	16
	0%	5%	3%	49%	43%
Resources (for Vehicle Technologies Office Projects)	0	0	9	13	10
	0%	0%	28%	41%	31%
Strategy for Technology Validation or Deployment (for Market Transformation and Technology Validation Projects)	0	3	10	7	7
	0%	11%	37%	26%	26%
Alternative Fuel Market Expansion and/or Petroleum Reduction Potential (for Technology Integration/Clean Cities Projects)	0	2	12	8	5
	0%	7%	44%	30%	19%

11 Comments

- This set of criteria will provide a good review of the project in most cases and was documented adequately in most presentations.
- The criteria make sense.
- Good structure for time-limited reviews.
- The criteria are correct. Interpretation and response relative to the criteria need be communicated to the researchers.
- No issues.
- Criteria largely made sense. The only issue was with criteria for overviews—at least three of the questions were asked multiple ways and should just be combined. In particular, the collaboration questions and the gaps in the program questions should be combined.
- The difference between “Outstanding” and “Excellent” ratings is not clear. It seems that a project can be improved upon, no matter how good it is.
- Some investigators used the future work section of the presentation to propose future work beyond the scope, and others used it to provide guidance on the future work of the project. The guidance for the investigators is unclear, and it should be clarified.
- More emphasis on the specific measurement of each project against technology deployment and market expansion would be beneficial.
- It is hard to tell whether there are enough resources on a project just from the presentations. It would take a more in-depth assessment to make that call.
- In the past, DOE did a better job of screening slides; here there were some clear mistakes, and sometimes metrics or goals were ambiguous.

3.5. The evaluation criteria were adequately addressed in the presentations.

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance/Potential Impact	1	2	5	20	7
	3%	6%	14%	57%	20%
Approach	0	1	3	21	11
	0%	3%	8%	58%	31%
Technical Accomplishments and Progress	0	1	2	18	14
	0%	3%	6%	51%	40%
Collaboration and Coordination	0	1	5	17	13
	0%	3%	14%	47%	36%
Proposed Future Research	0	3	3	22	8
	0%	8%	8%	61%	22%
Resources (for Vehicle Technologies Office Projects)	0	2	7	14	4
	0%	7%	26%	52%	15%
Strategy for Technology Validation or Deployment (for Market Transformation and Technology Validation Projects)	0	6	8	9	2
	0%	24%	32%	36%	8%
Alternative Fuel Market Expansion and/or Petroleum Reduction Potential (for Technology Integration/Clean Cities Projects)	1	6	8	7	2
	4%	25%	33%	29%	8%

11 Comments

- No issues.
- Generally true. About half of the presentations I reviewed did not stick to this format, so I had to read between the lines.
- Some of the presenters did not adequately present the information that was requested in the guidelines; however, most did.
- This is a difficult question to answer because unfortunately not all the presentations had the same elements, and each criterion was addressed differently by different presenters.
- There is not enough time for real presentation of technical results unless the reviewer is already pretty familiar with the work.
- Judging the approach is more difficult than judging the technical progress, collaboration, relevance, and future plans.
- Relevance was merely a listing of DOE barriers. What is badly needed is a statement of the state of the art when the project began, the quantitative goals it is aiming for, the current status toward those goals, and why it matters—that is to say, what will be better about the world if the project succeeds.
- Projects need to be more clearly structured to support the overall objectives of technology deployment. Not all of the projects were clear on how they were relevant to the end goals.
- Some investigators used the future work section of the presentation to propose future work beyond the scope, and others used it to provide guidance on the future work of the project. The guidance for the investigators seems to be unclear, and should be clarified.
- Resources are rarely addressed by presenters.
- The collaboration category rewards large teams, which are almost always a waste of money.

3.6. 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 number is the percentage of total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance/Potential Impact	0	2	1	22	12
	0%	5%	3%	59%	32%
Approach	0	2	1	23	11
	0%	5%	3%	62%	30%
Technical Accomplishments and Progress	0	1	2	19	13
	0%	3%	6%	54%	37%
Collaboration and Coordination	0	3	4	22	8
	0%	8%	11%	59%	22%
Proposed Future Research	0	1	5	22	9
	0%	3%	14%	59%	24%
Resources (for Vehicle Technologies Office Projects)	1	0	6	15	5
	4%	0%	22%	56%	19%
Strategy for Technology Validation or Deployment (for Market Transformation and Technology Validation Projects)	0	4	9	7	4
	0%	17%	38%	29%	17%
Alternative Fuel Market Expansion and/or Petroleum Reduction Potential (for Technology Integration/Clean Cities Projects)	0	2	10	7	4
	0%	9%	43%	30%	17%

10 Comments

- All had reasonable weightings, with accomplishments/progress placed properly as the priority.
- Weighting is appropriate for the type of research being funded.
- Weighting appeared to make sense.
- Criteria are correct; the weighting factors are probably okay.
- No issues.
- Technology deployment should be weighted higher.
- Collaboration is not needed in some cases, and teams should not be marked down. This also drives bad behavior, such as listing suppliers as collaborators. No one talks about resources other than to list the payment plan, which is not very useful; presenters need to discuss whether the resources are adequate.
- Collaboration needs to be managed but not made a requirement. The need for collaboration with universities and federally funded research laboratories should be project-specific vs. encouraged.
- Not enough choices were provided for the "resources" criteria.
- This reviewer was unaware that the criteria were weighted.

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	2	18	17
0%	3%	5%	47%	45%

10 Comments

- Great opportunity to meet with the researchers—they are in the room.
- Poster presenters were much better about being present this year.
- This was the highlight of this form of a review.
- No problems.
- Reviewers were given the first opportunity to ask questions.
- Most presenters finished in the allotted time or were stopped to allow for questions so the session could finish close to on time.
- No issues; however, others need reminding that this is a peer review, not a conference. Some of the questions were not germane to the project being reviewed.
- It would be good to have an email address to send questions to before and after the AMR.
- Some presentations went long, which did not allow for a full Q&A session. Keeping presenters to the 20-minute slot is suggested.
- All reviewers typically got for Q&A was a few minutes at the end of the presentation. Usually that was enough, but if it was not, it was not clear that there was any recourse.

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	2	1	17	18
0%	5%	3%	45%	47%

9 Comments

- The AMR coordinating organization did an outstanding job of getting the information to the attendees well in advance of the AMR.
- Nicely organized. Easels with the schedule were very handy.
- Excellent layout.
- Highly organized, as usual (except for the interim program organized by time slot—that one really did not work).
- Please just send the when/where details of the session the reviewer is reviewing. All of the other details can be picked up once the reviewer arrives in the District of Columbia.
- Participants still have to choose between similar programs sometimes.
- The program is pretty dense and took a while to figure out.
- It is hard to find the time and location of each presentation since the number in the presentation sequence was random.
- The website with the agenda was not functioning the week prior to the Review.

3.9. The number of projects I was expected to review was:

	Number of Responses	Response Ratio
Too many	2	<1%
Too few	5	2.3%
About right	30	14.2%
No Responses	174	82.4%
Total	211	100%

12 Comments

- This year the workload was well balanced.
- There were no problems.
- Three is about right.

- I had six but could have done eight or even ten. The lighter workload was appreciated, though.
- I was able to complete the 13 reviews that I was assigned.
- Only two.
- I was pleased to see the automatic extension to June 20 for reviews—not that it was critical, but the close of business Friday date had always seemed unnecessary.
- I did four reviews, all on Thursday. The reviewer could have done more reviews (if needed by DOE staff).
- I only had two; three to five would have been doable.
- I only received 4 to review initially, which I did not think justified the expense of coming. After I complained, it was increased to nine, which was just about right.
- It was very difficult to justify the trip expenses to do a single review.
- I would have liked to not have had back-to-back sessions to review so that I could finish my comments from each session before the next session. Also, since I had back-to-back sessions, I was running between rooms on different floors and at times missed the first 10 minutes of the next session. If the AMR has reviewers perform back-to-back reviews, please make sure that they are in the same session room for those reviews. That would be more conducive for quality.

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
0	1	4	19	14
0%	3%	11%	50%	37%

9 Comments

- For the number of days allotted and for the number of projects being reviewed, there was adequate depth of the subject matter for a meaningful review.
- For most presentations, this worked well. There were a few where the time for the Q&A session ended up being insufficient, either because of a presentation that ran long or significant questions.
- For the topics I reviewed, which were multi-year projects, it was adequate. For a new reviewer or a new project, more time would be nice.
- It was sufficient for some, but insufficient for others.
- Yes, but I learned more time should be spent before the meeting reviewing the presentations and preparing questions. I'll make sure to do that next time.
- There could be a longer presentation period. Most presenters packed too much into their slides for the time available.
- It is recommended that the AMR allow more time after each presentation.
- A 20-minute talk is not enough time to describe the major points in a year of work to those not closely following the work already. But with several hundred projects, it is not clear what else can be done.
- Not in the case of models. One cannot evaluate a model based on a presentation in which the PI does not even get to run the model.

3.11. Please provide additional comments.

14 Responses

- Together, with many other researchers, I rate the DOE AMR as the most informative meeting in the energy area in the world in terms of quality of presentations and the attendance.
- The DOE team has been running the AMR for over a decade, and it shows. It runs very smoothly and professionally. The only issues were with the meals (more and better selections are required). The actual facilities were quite nice.

- The Wi-Fi for reviewers was flawless this year. There was a delay in getting on but only a minute or two, and it never dropped out—not once—while I was reviewing. This is a nice improvement. The ORAU staff were polite and helpful. There was sufficient room in the poster sessions (that has not always been true).
- The annual DOE AMR is always useful not only for the reviews but also for the opportunity for on-site collaboration in many areas. This year's AMR had more than usual.
- As always, it was an interesting, enjoyable, and valuable session.
- It would be beneficial if the researchers were required to show how their activity is value-added. At this point in the Hydrogen and Fuel Cells Program, the research should be in applied, not pure, science. Also, the support from ORAU, in the peer review office, was fantastic as always.
- The venue was good and reasonably sized for the number of participants, but the rooms tended to be on the cool side.
- It would be great if the reviewers could have some freedom to pick the projects they would like to review and specify the projects that they do not want to review.
- Please consider reworking the overview questions and combining several areas into single questions. In addition, the instructions for individual projects indicated a score should be left blank for Future Work for projects that were over (or ending). When doing so, no overall score was provided in the project list.
- Some presenters followed the review template (Approach, Technical Progress, Future Work, etc.), but some did not.
- One PI indicated he assumed reviewers have access to reports provided outside of the review process. This is not the case. It is recommended that the instructions for the preparation of presentations make it very clear that evaluation will be made solely on information provided in the presentation. Reviewer-Only slides should be used to communicate additional information that the presenter feels is important to the project's evaluation.
- The fruit and coffee service should not be taken away after 9:00 a.m.
- DOE must make an effort to better define primary and secondary objectives for the Program and to provide better tools for measuring progress. A good example would be the preparation, distribution, and management of gap charts for the appropriate research areas.

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	1	2	16	33
1%	2%	4%	30%	62%

5 Comments

- Yes, the timing was adequate, and follow-through was beneficial.
- The time is sufficient. However, projects might be evolving, so a balance is of value.
- Perhaps the presentation was provided too far in advance.
- The presenters all know when to expect the request, so a month to prepare it is probably adequate. However, requiring the slides two months in advance of the review is not reasonable. Researchers who try to comply with that deadline are placed at a disadvantage. Some Researchers did not receive their fiscal year 2016 funding until March. The projects would look better if the researchers had more time to work before the slides are assembled and if they could present more current information at the review.
- The deadline to submit the presentations is too early (over two months before the meeting). By the time of the actual presentation, the results being presented are over three months old, which makes for an awkward discussion.

4.2. Instructions for preparing the presentation were sufficient.

The top number is the count of respondents selecting the option. The bottom number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
1	1	1	22	29
2%	2%	2%	41%	54%

4 Comments

- They were very useful—and I had no problem putting my presentation together.
- The detailed instructions are very valuable, but some presenters attended last year and would benefit from a shorter summary of the changes from last year.
- This was my first AMR meeting, and I don't think the instructions really captured how to prepare a poster correctly. The instructions were orientated toward an oral presentation format. More visual examples, e.g., actual pictures, would help.
- It was overly sufficient. The AMR presentation format is bad and leads to a boring, confusing presentation.

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 number is the percentage of total respondents selecting the option.

Highly Disagree	Disagree	Neutral	Agree	Highly Agree
3	2	3	16	28
6%	4%	6%	31%	54%

6 Comments

- They worked great.
- For the presentations in the Washington rooms, it was difficult to see the presenter due to large pillars in the room, and if one was in the back, the noise from people outside the room was a distraction.
- Generally, the clarity of the projections was a little bit low. In many cases, presentations had small print that was difficult to make out clearly. Also, the projected colors were often a bit different from the colors in the digital version. The presenter was difficult to see, depending on one's positioning (in the Washington 4 room). The audio worked generally quite well.
- Washington 4 had big pillars. It was hard to use a laser pointer with the projection screens in line with the podium.
- The room was poor. Columns forced the presentation to be split on four screens. The pointer could point to only one screen. There was no mouse, so pointing and talking was awkward, as the mouse pad wanted to move the mouse and flip slides.
- The layout in the large room (Washington 4, bottom floor) for Energy Storage was very poor. Speaking from the podium, the presenter could not see almost 20% of the audience because of the large pillars throughout the room. Seeing the speakers was equally frustrating. Walking around the pillars was not convenient. Several people clipped their shoulders and heads on the flat panel monitors mounted on the pillars while trying to get around them.

(Continued next page)

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 number is the percentage of total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance/Potential Impact	1	2	2	24	23
	2%	4%	4%	46%	44%
Approach	1	2	2	26	21
	2%	4%	4%	50%	40%
Technical Accomplishments and Progress	1	1	2	23	25
	2%	2%	4%	44%	48%
Collaboration and Coordination	1	3	4	26	18
	2%	6%	8%	50%	35%
Proposed Future Research	1	1	6	28	15
	2%	2%	12%	55%	29%
Resources (for Vehicle Technologies Office Projects)	1	5	5	23	10
	2%	11%	11%	52%	23%
Strategy for Technology Validation or Deployment (for Market Transformation and Technology Validation Projects)	1	3	12	20	6
	2%	7%	29%	48%	14%
Alternative Fuel Market Expansion and/or Petroleum Reduction Potential (for Technology Integration/Clean Cities Projects)	1	3	13	16	5
	3%	8%	34%	42%	13%

3 Comments

- This presenter was never made aware of the evaluation criteria used for the review. It may have been addressed in a general session. It is recommended that this information be emailed to presenters in the future.
- Some of these criteria are not applicable for my project.

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 number is the percentage of total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance/Potential Impact	1	2	2	28	20
	2%	4%	4%	53%	38%
Approach	1	1	4	28	19
	2%	2%	8%	53%	36%
Technical Accomplishments and Progress	1	1	3	26	22
	2%	2%	6%	49%	42%
Collaboration and Coordination	1	3	4	26	18
	2%	6%	8%	50%	35%
Proposed Future Research	1	1	6	30	15

	2%	2%	11%	57%	28%
Resources (for Vehicle Technologies Office Projects)	1	3	7	22	13
	2%	7%	15%	48%	28%
Strategy for Technology Validation or Deployment (for Market Transformation and Technology Validation Projects)	1	2	11	18	10
	2%	5%	26%	43%	24%
Alternative Fuel Market Expansion and/or Petroleum Reduction Potential (for Technology Integration/Clean Cities Projects)	2	2	10	17	9
	5%	5%	25%	43%	23%

1 Comment

- The criteria are questions that are very subjective—it is up to the reviewers to interpret what is Outstanding vs. Excellent vs. Good. The collaboration criterion seems to be especially subjective.

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 number is the percentage of total respondents selecting the option.

	Highly Disagree	Disagree	Neutral	Agree	Highly Agree
Relevance/Potential Impact	1	1	5	26	19
	2%	2%	10%	50%	37%
Approach	1	1	5	26	19
	2%	2%	10%	50%	37%
Technical Accomplishments and Progress	1	1	5	25	20
	2%	2%	10%	48%	38%
Collaboration and Coordination	1	2	7	23	18
	2%	4%	14%	45%	35%
Proposed Future Research	1	2	8	26	15
	2%	4%	15%	50%	29%
Resources (for Vehicle Technologies Office Projects)	1	3	9	18	13
	2%	7%	20%	41%	30%
Strategy for Technology Validation or Deployment (for Market Transformation and Technology Validation Projects)	1	3	10	18	9
	2%	7%	24%	44%	22%
Alternative Fuel Market Expansion and/or Petroleum Reduction Potential (for Technology Integration/Clean Cities Projects)	1	4	9	15	8
	3%	11%	24%	41%	22%

0 Comments

4.7. Please provide additional comments:

9 Responses

- Having DOE program managers chair the oral sessions was excellent and well received.
- Overall, it was a good AMR.
- The four-day format was good. There should be more posters and fewer presentations. Reviewers should show up at the posters. The awards process is very unclear. DOE should open it up to make it transparent.

- Overall, the meeting went very well, but there was a lot of focus on the academic and review side of the work, and there were missed opportunities for interactions and meetings between attendees. The meeting should try to increase opportunities for individuals to interact.
- Please have regular sit-down luncheons.
- A whole day should be dedicated to allow serendipitous meetings between people.
- This presenter was never made aware of the evaluation criteria used for the review. It may have been addressed in a general session. It is recommended that the presenters are emailed that information in the future.
- It is hard to find good reviewers, but the process really needs to be more selective. During a Q&A session this year, a reviewer was trying to promote their own research program, and I had a reviewer contact me shortly after the review to inquire about having funding diverted to their own company.
- There are still concerns about the open access nature of the review, e.g., open meeting and slides on the Internet.