
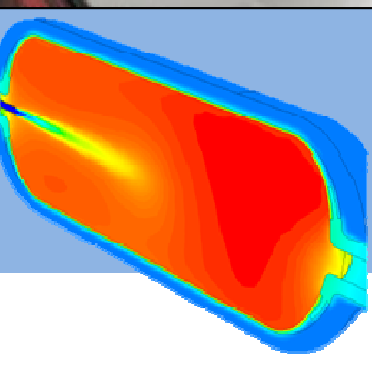


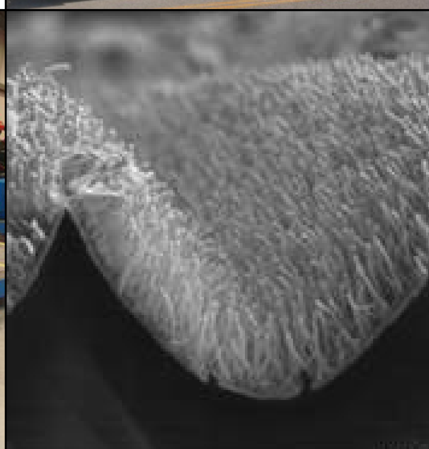

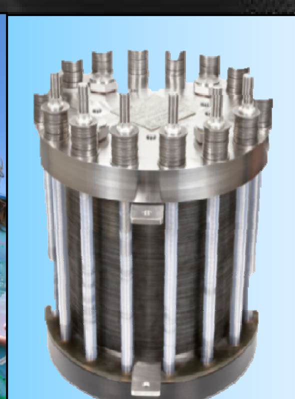

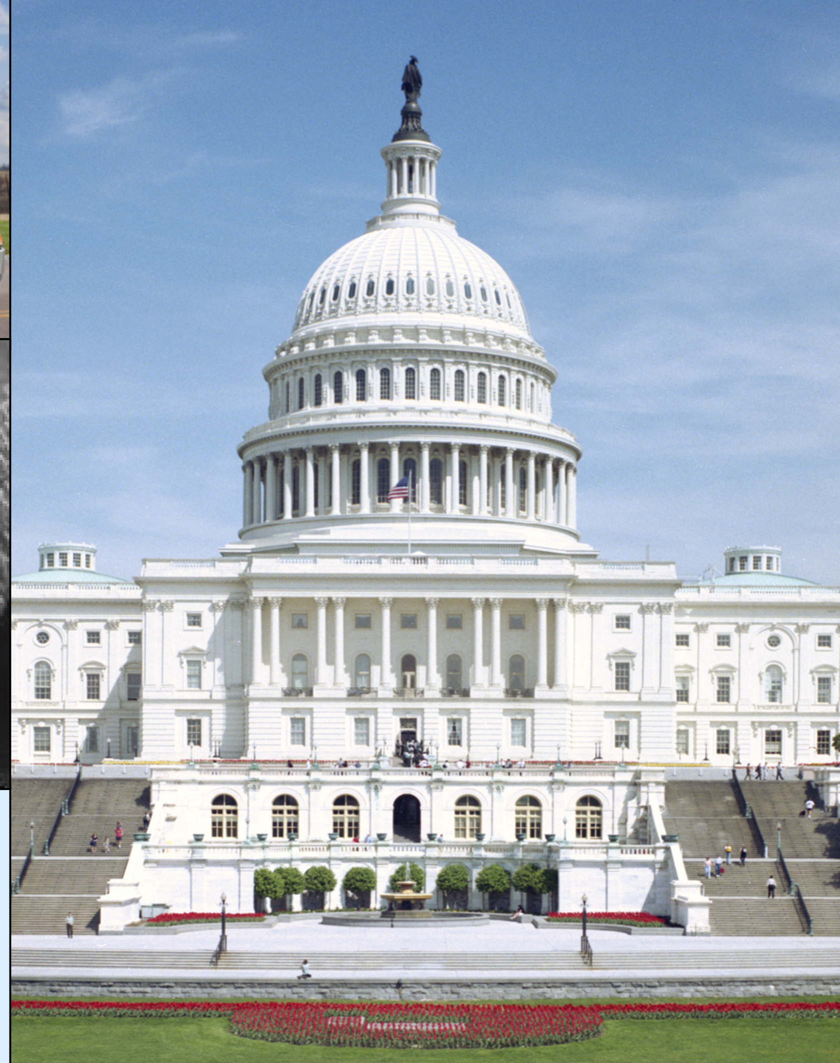

U.S. DEPARTMENT OF ENERGY

# Hydrogen and Fuel Cells Program

# 2012 Annual Merit Review and Peer Evaluation Report

*May 14–18, 2012*
*Arlington, Virginia*

DOE/GO-102012-3695 September 2012

***About the Cover***

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

*A close-up of hydrogen refueling of a fuel cell vehicle. (NREL PIX 16640)*

*Computational fluid dynamics modeling of the temperature distribution inside a Type IV hydrogen storage tank 80 seconds into the fill. This work is being performed in 2012 at Sandia National Laboratories under the Safety, Codes and Standards sub-program and an international exchange program with Zhejiang University. Photo courtesy of Sandia National Laboratories. (NREL PIX 22263)*

*The Delphi solid oxide fuel cell auxiliary power unit seen on this Peterbilt truck provides power to the truck's hotel loads. (NREL PIX 20085)*

*Fuel-cell-powered material handling equipment at the Sysco Houston distribution facility in Houston, Texas. The greenfield site received funding through the American Recovery and Reinvestment Act to deploy 98 lift trucks powered by fuel cells and has been operating since early 2010. (NREL PIX 18347)*

*A scanning electron microscopy image of a cross-section of a 3M nanostructured thin film (NSTF) electrode. Photo courtesy of Oak Ridge National Laboratory.*

*Solar race judge and National Renewable Energy Laboratory employee David Ginley at the 2011 Jr. Solar Sprint and Hydrogen Fuel Cell Car Competition with members of the winning hydrogen design team, "Larry the Leaf." The all-girls team also captured the Spirit Award for exhibiting good sportsmanship, including fairness and respectful behavior. Team members are Grace Simpson, Nani Ciafone, Naia Tenerowicz, and Sam Henry. (NREL PIX 19195)*

*A low-cost polymer electrolyte membrane (PEM) electrolyzer stack from Giner, Inc. Photo courtesy of Giner, Inc. (NREL PIX 22260)*

*A Lincoln Composites Titan Tube Trailer capable of storing approximately 616 kg of hydrogen at 3,600 psi. Photo courtesy of Lincoln Composites. (NREL PIX 22261)*

*Photo on right:*

*U.S. Capitol Building. Photo courtesy of U.S. Government, Architect of the Capitol.*

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

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## Prologue

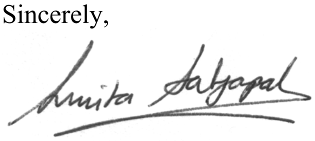
Dear Colleague:

This document summarizes the comments provided by peer reviewers on hydrogen and fuel cell projects presented at the fiscal year (FY) 2012 U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program and Vehicle Technologies Program Annual Merit Review and Peer Evaluation Meeting (AMR), held May 14–18, 2012, in Arlington, Virginia. In response to direction from various stakeholders, including the National Academies, this review process provides evaluations of the Hydrogen and Fuel Cells Program's projects in applied research, development, demonstration, and analysis of hydrogen, fuel cells, and infrastructure technologies. A joint plenary session opened the meeting with a presentation on "Transforming Transportation Toward a Very Efficient, Low-Carbon Energy Future," followed by overview presentations from the DOE Office of Basic Energy Sciences, Hydrogen and Fuel Cells Program, and Vehicle Technologies Program. A plenary for Hydrogen and Fuel Cells Program participants included overviews on each of nine sub-programs: Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Market Transformation; Technology Validation; Safety, Codes and Standards; Education; and Systems Analysis. An overview of American Recovery and Reinvestment Act (Recovery Act) activities was also presented.

DOE values the transparent, public process of soliciting technical input on projects from relevant experts. The recommendations of the reviewers are taken into consideration by DOE technology development managers in generating future work plans. The table that follows lists the projects presented at the review, evaluation scores, and the major actions to be taken during the upcoming fiscal year (October 1, 2012–September 30, 2013). The projects have been grouped according to sub-program and reviewed according to appropriate evaluation criteria. This year's AMR featured the third annual review of hydrogen and fuel cell projects funded under the Recovery Act. The weighted scores for all of the projects are based on a four-point scale. 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 2013 plans.

In addition to thanking all participants of the AMR, I would like to express my sincere appreciation to the reviewers. You make this report possible, and we rely on your comments, along with other management processes, to help make project decisions for the new fiscal year. We look forward to your participation in the FY 2013 AMR, which is presently scheduled for May 13–17, 2013, in Arlington, Virginia. Thank you for participating in the FY 2012 AMR.

Sincerely,



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

## Hydrogen Production and Delivery

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-002 | Biomass-Derived Liquids Distributed (Aqueous Phase) Reforming<br>*David King;*<br>*Pacific Northwest National Laboratory* | 2.4 | | X | | According to reviewers, this project is technically sound and effectively identified optimal catalysts for use in reforming the aqueous phase of biomass- derived liquids. However, they expressed concern over the high cost of feedstock for hydrogen production and lack of a clear pathway to meeting U.S. Department of Energy (DOE) cost targets. Recommendations were made to complete the project and document results, and to consider economically feasible niche applications for this method of hydrogen production. |
| PD-004 | Distributed Bio-Oil Reforming<br>*Stefan Czernik;*<br>*National Renewable Energy Laboratory* | 3.1 | | X | | Reviewers noted the progress of the project and commended the team's focus on feedstock preparation and reformation. Reviewers noted the lack of a clear path forward to meet cost targets based on feedstock cost and catalyst cost and durability. It was recommended that future work focus on specific methods to improve hydrogen yield, understanding differences in bio-oil composition and its effect on hydrogen production, and completing a lifecycle analysis for catalyst choices. |
| PD-013 | Electrolyzer Development for the Cu-Cl Thermochemical Cycle<br>*Michele Lewis;*<br>*Argonne National Laboratory* | 2.6 | | X | | Reviewers praised the project for its relatively simple three-step thermochemical process at a low temperature of 550°C and compelling concept which could produce hydrogen from waste heat without concurrent greenhouse gas emissions. Reviewers noted that while progress was made on the key issue of copper crossover, there was a lack of focus on the mechanisms of degradation. It was recommended that the project address degradation and barriers to implementation of the full cycle, while putting less focus on scaling up. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-014 | Hydrogen Delivery Infrastructure Analysis<br>*Marianne Mintz; Argonne National Laboratory* | 3.6 | X | | | According to the reviewers, this project is extremely important to the identification of potential cost reductions in the delivery pathways. Reviewers felt that this is a strong project which has effectively identified current technological challenges and ways to resolve them. Reviewers recommended that prioritization of analysis topics, industry input and vetting of data, and validation of predicted costs be addressed in future. |
| PD-016 | Oil-Free Centrifugal Hydrogen Compression Technology Demonstration<br>*Hooshang Heshmat; Mohawk Innovative Technology, Inc.* | 3.4 | X | | | Reviewers commented that this project has made good progress toward an essential facet of hydrogen delivery through thoughtful design and good collaborations with outside organizations. It was recommended that a test with hydrogen be performed and capital cost estimates be refined prior to scaling up unit production. |
| PD-017 | Development of a Centrifugal Hydrogen Pipeline Gas Compressor<br>*Frank Di Bella; Concepts NREC* | 3.2 | X | | | Reviewers felt this project has made adequate progress and addresses a critical component for safe and cost effective hydrogen delivery. Reviewers suggested development of a one-stage prototype rather than a two-stage prototype to reduce cost and expansion of industry collaborations to ensure market relevance. |
| PD-021 | Development of High Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery<br>*Don Baldwin; Lincoln Composites* | 3.4 | X | | | According to reviewers, this project shows significant progress towards lowering the cost of hydrogen delivery. The reviewers appreciated the identification of the commercial barriers and the planning and execution of adjustments to address issues encountered. Reviewers encouraged continued development of this technology to the 350 bar level. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-022 | Fiber Reinforced Composite Pipelines<br>*Thad Adams;*<br>*Savannah River National Laboratory* | 3.4 | X | | | Reviewers praised the progress made in conducting tests and working in conjunction with partners such as ASME. Reviewers specifically cited identification of potential failure modes and materials. According to reviewers, further work is needed to clearly define, via a cost-benefit analysis, what can be learned from a large-scale pipeline demonstration and to characterize fiber-reinforced polymer (FRP) composite pipeline performance with test data. |
| PD-024 | Composite Pipeline Technology for Hydrogen Delivery<br>*Barton Smith;*<br>*Oak Ridge National Laboratory* | 3.2 | | | X | According to reviewers, this project team has well defined barriers and technical targets for the qualification of FRP pipelines, which is critical to the long-term goal of reducing hydrogen delivery costs. Reviewers cited good progress in crack resistance and permeability. It was suggested that future work focus on data needed to qualify composite pipeline for hydrogen service. |
| PD-025 | Hydrogen Embrittlement of Structural Steels<br>*Brian Somerday;*<br>*Sandia National Laboratories* | 3.4 | X | | | Reviewers noted that this project has a sound combination of experimental and modeling work. Reviewers encouraged review of the results by organizations that operate high pressure hydrogen pipelines to help validate the findings. Reviewers suggested performing temperature, pressure, and frequency cycles simultaneously. |
| PD-027 | Solar High-Temperature Water Splitting Cycle with Quantum Boost<br>*Robin Taylor;*<br>*Science Applications International Corporation* | 2.6 | | X | | Reviewers commended the progress on improved voltage and the use of modeling to understand efficiency of the thermochemical cycle, particularly in the electrolysis step. However, they expressed concerns regarding the complexity of this cycle and indicated that more attention should be given to defining targets for cell voltage and current densitites, assessing balance of plant costs, and analyzing the economics to define the overall value proposition of this cycle. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-028 | Solar-Thermal ALD Ferrite-Based Water Splitting Cycles<br>*Al Weimer;*<br>*University of Colorado* | 3.0 | X | | | Reviewers favorably cited the team's use of advanced fabrication and characterization methods for the reactant materials and their initial investigation of isothermal operation. Reviewers felt that the reactor design and potential for 24/7 operation should receive more attention. It was recommended that the team address issues of reactor mechanical strength and its relation to lifecycle and cycle time, pressure and temperature ranges to reduce process cost, and hercynite/alumina composition effects on redox performance and particle robustness. |
| PD-029 | High-Capacity, High Pressure Electrolysis System with Renewable Power Sources<br>*Paul Dunn;*<br>*Avalence LLC* | 2.6 | | | X | Reviewers appreciated the progress made despite the difficulties that were present. Reviewers noted that the work could benefit from further collaboration with an academic team. They expressed concern due to the lack of cost analysis. Reviewers also felt that investigators insufficiently addressed safety concerns with regards to the new multiple tube approach. |
| PD-030 | PEM Electrolyzer Incorporating an Advanced Low Cost Membrane<br>*Monjid Hamdan;*<br>*Giner Electrochemical Systems, LLC* | 3.7 | | | X | According to reviewers, investigators made excellent progress with a good blend of analysis, design, and experimentation. Reviewers highlighted the significant reductions in material and manufacturing costs as well as the team's work with industry partners. Reviewers recognized that electrolysis systems are limited by the cost of electricity. |
| PD-031 | Renewable Electrolysis Integrated System Development and Testing<br>*Kevin Harrison;*<br>*National Renewable Energy Laboratory* | 2.7 | X | | | Reviewers appreciated the importance of the team's test facility as an independent third party test site to validate technical and economic claims. They however expressed uncertainty about the project's impact on the stated barriers of cost and system efficiency. They also recommended a greater emphasis on integrated renewable electrolysis systems. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-033 | Directed Nano-scale and Macro-scale Architectures for Semiconductor Absorbers and Transparent Conducting Substrates for Photoelectrochemical Water Splitting<br>*Thomas Jaramillo; Stanford University/National Renewable Energy Laboratory* | 3.3 | X | | | Reviewers commended this project for its well-defined scope and novel approach to addressing key photoelectrochemical (PEC) issues. They highlighted that the high quality of the work and strong collaborations with the PEC working group have led to important progress in developing bandgap-tunable photocatalysts and stable support structures. Reviewers did recognize that this project is still at an early stage, noting that the PEC efficiency at present is too low to meet nearer term goals for efficiency, durability, and cost. They recommended more emphasis be placed on identifying specific losses mechanisms and limitations. |
| PD-035 | Semiconductor Materials for Photoelectrolysis<br>*John Turner;*<br>*National Renewable Energy Laboratory* | 3.3 | X | | | Reviewers noted that good progress was made in demonstrating high efficiency with improved durability in III-V PEC semiconductors. Specific project strengths cited included the demonstration of extended durability (>100 hours) of the PEC interface, standardization of testing and reporting protocols, and efficiency bookmarking under actual sunlight to validate laboratory results. Reviewers recommended continued efficiency and durability studies of the PEC interface under full-spectrum conditions, including atomic-level modeling. They also recommend that the cost effectiveness of nitride-treated III-V PEC cells be evaluated further. |
| PD-036 | Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures<br>*Tasios Melis;*<br>*University of California, Berkeley* | 3.6 | X | | | Reviewers praised the progress in identifying genes and proteins that determine algal chlorophyll antenna size to improve light collection efficiency, as well as the focus on potential application of the findings. Reviewers noted that the lack of details on future tasks prevents evaluation of likely progress and suggested the development of collaborations with experts for planned cyanobacteria experiments, to explicitly link the project to hydrogen production to quantify how changes to photosynthetic structure affects hydrogen yields. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-037 | Biological Systems for Hydrogen Photoproduction<br>*Maria Ghirardi;*<br>*National Renewable Energy Laboratory* | 3.4 | X | | | Reviewers approved of the decision to halt the targeted random mutagenesis work and commended the accomplishments of demonstrating light-dependent hydrogen production in an algal hydrogenase knockout mutant with an oxygen-tolerant hydrogenase (Ca1) gene insertion. Reviewers requested more information about future steps and interim goals so that progress could be better understood and evaluated. |
| PD-038 | Fermentation and Electrohydrogenic Approaches to Hydrogen Production<br>*Pin-Ching Maness;*<br>*National Renewable Energy Laboratory* | 3.3 | X | | | Reviewers commended significant steps towards scaling up by demonstrating that immobilization of microbes on the substrate allows easy separation of the growth medium from the acclimated culture. They also praised the progress in the compositional analysis for the microbial electrolysis cell project. Reviewers questioned some of the steps taken, such as making a *dcm-* strain when there are commercially available strains. Reviewers recommended the team establish more specific performance targets, provide more details on how the work will be accomplished, and focus on executing the counter screen for *pyrF* knockout. |
| PD-039 | Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacterial System<br>*Phil Weyman;*<br>*J. Craig Venter Institute* | 3.2 | X | | | According to reviewers, the well-designed approach of this project has allowed for commendable accomplishments. Furthermore, the principal investigator's decision to establish a go/no-go decision based on a definitive hydrogenase activity goal was praised. Reviewers raised concerns that the planned focus on increasing expression might overlook the effect of different ratios of the proteins. It was recommended that alternative approaches be addressed, and that work on engineering the cyanobacteria to increase the hydrogen production to viable levels be continued. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-048 | Electrochemical Hydrogen Compressor *Ludwig Lipp; FuelCell Energy, Inc.* | 3.1 | X | | | Reviewers commented that this project is focused on the right issues and has made good progress with 12,000 pounds per square inch (psi) cells. Reviewers pointed out the lack of a clear path to achieving DOE cost targets. According to reviewers, future work should include collaboration with parties who can help develop and provide commercial awareness of the technology as well as perform life cycle cost analyses. |
| PD-053 | Photoelectrochemical Hydrogen Production *Arun Madan; MVSystems/Hawaii Natural Energy Institute* | 3.2 | X | | | According to reviewers, this project exhibited logical organization and pursuit of the three PEC material classes under investigation. Reviewers specifically cited progress made in improving the a-Si system using surface treatments and in meeting milestones in durability tests for all three material systems. They emphasized the need for continued extended durability tests and for more extensive quantum efficiency measurements to gain a better understanding of material performance and limitations. |
| PD-065 | Unitized Design for Home Refueling Appliance for Hydrogen Generation to 5,000 psi *Timothy Norman; Giner Electrochemical Systems, LLC* | 3.3 | | | X | Reviewers commented that this project has made technical breakthroughs that contribute to significant progress toward accomplishing goals. Strenghs identified by reviewers included the emphasis on meeting codes and standards and the completion of a preliminary economic analysis of a commercial home refueling appliance system that falls within DOE targets. |
| PD-067 | Hydrogen by Wire – Home Fueling System *Luke Dalton; Proton OnSite* | 2.7 | | | X | Reviewers commented that this project is making good progress towards achieving its objectives. Reviewers noticed a lack of focus on meeting cost objectives and lack of technical detail released on component research and performance. Additionally, reviewers suggested a greater focus on safety. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-070 | One Step Biomass Gas Reforming-Shift Separation Membrane Reactor *Mike Roberts; Gas Technology Institute* | 2.3 | | X | | According to reviewers, the project's concept of a gasifier membrane to produce hydrogen is novel and particularly interesting. Reviewers noted that most of the progress has been concentrated on modeling and suggested that experimentation should be given more emphasis. Reviewers identified the need to characterize the status of hydrogen purity, selectivity, and membrane durability. Reviewers recommended that integrated gasifier tests be started to validate performance and cost estimates. |
| PD-071 | High Performance, Low Cost Hydrogen Generation from Renewable Energy *Katherine Ayers; Proton OnSite* | 3.1 | X | | | Reviewers praised the effective execution and use of collaborations with capable partners. Reviewers gave positive feedback on the team's stack cost reduction, prototype testing, and planned path for continued progress. It was recommended that the team emphasize meeting future cost and efficiency challenges and focus on scale up as well as manufacturing and test qualification. |
| PD-072 | Development of Hydrogen Selective Membranes/Modules as Reactors/Separators for Distributed Hydrogen Production *Paul Liu; Media and Process Technology Inc.* | 3.1 | X | | | Reviewers commended the progress made to date, but expressed concern regarding membrane durability during cooling cycles and did not see a clear path toward solving this issue. Reviewers also stressed the importance of validating lab results with a field test. It was further recommended that the team test the water-gas-shift reactor with cycling and that the team work with Ballard to understand how their system will operate during periods of stand-down. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-079 | Novel Photocatalytic Metal Oxides<br>*Robert Smith;*<br>*University of Nebraska at Omaha* | 2.7 | | | X | Reviewers commended the project for its innovative approach to PEC hydrogen production, including both computational and experimental components. They did express concern that the DFT-DOS calculations were unlikely to prove helpful in identifying new photoactive materials with improved PEC performance. Specific recommendations included expanded collaboration for assistance in characterizing photovoltaic/photoelectrochemical materials and better definition of the scientific and cost barriers to the technology. |
| PD-081 | Solar Hydrogen Production with a Metal Oxide Based Thermochemical Cycle<br>*Ivan Ermanoski;*<br>*Sandia National Laboratories* | 2.7 | X | | | Reviewers cited the project's well-balanced approach towards addressing difficult materials challenges and the progress in building the reactor, testing under ambient conditions, and successfully conveying powder particles. Reviewers suggested the project focus on developing the reactant materials prior to planning the scale-up process, state and test material life cycles, complete H2A cost analysis, and develop protocols for material characterization that includes clear metrics for success. |
| PD-088 | Vessel Design and Fabrication Technology for Stationary High-Pressure Hydrogen Storage<br>*Wei Zhang;*<br>*Oak Ridge National Laboratory* | 3.2 | X | | | Reviewers noted the project's potential to reduce the cost of hydrogen bulk storage and the progress made in designing vessels to meet pressure codes while detailing the manufacturing process and cost estimates. Reviewers recommended that design decisions be revisited when appropriate and that on-site fabrication be considered to avoid shipping challenges. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| PD-091 | Bio-Fueled Solid Oxide Fuel Cells<br>*Gokhan Alptekin;*<br>*TDA Research* | 3.3 | X | | | Reviewers cited specific strengths of the project including the development of a high-capacity sorbent for sulfur removal, field testing of the skid, and the start of the contracting process with fuel cell manufacturers. Reviewers commented that details on the test results of complex sulfides were not provided nor was a work plan to gauge progress and future work. Reviewers suggested that the team explore the applicability to other technologies for hydrogen generation. |

*Permanent consecutive project numbers were assigned for the 2010 AMR to make administration of the AMR more efficient. As projects are discontinued, the corresponding project number is retired. As new projects are added, new consecutive numbers are added. As a result, there are gaps in the consecutive project numbers. In addition, some projects were not selected for review at the 2012 AMR and are not shown in the Prologue Table (see Appendix D for the full list of projects not reviewed).

## Hydrogen Storage

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| ST-001 | System Level Analysis of Hydrogen Storage Options<br>*Rajesh Ahluwalia; Argonne National Laboratory* | 3.2 | X | | | The reviewers commented that the project has provided useful quantitative storage system performance estimates and important insights into the systems analyzed. The reviewers recommended calibration of the compressed tank model and validation of carbon fiber/composite properties to known tank systems, and that the project identify risk mitigation strategies and potential showstoppers for systems analyzed. Continued collaboration with the Hydrogen Storage Engineering Center of Excellence (HSECoE) was encouraged. |
| ST-004 | Hydrogen Storage Engineering Center of Excellence<br>*Don Anton; Savannah River National Laboratory* | 3.3 | X | | | This project is for the management of the HSECoE. The reviewers found the HSECoE to be well managed, with good collaboration and communication between partners and satisfactory progress to date. The HSECoE was praised for down-selects of concepts and approaches projected to not be able to meet DOE targets, such as discontinuing work on reversible metal hydrides. Reviewers expressed concern about the current lack of a material with the properties required for a complete system to meet all of the DOE on-board storage targets and the lack of efforts to improve regeneration processes for chemical hydrogen storage materials, although these were recognized to be out of scope for the HSECoE. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| ST-005 | Systems Engineering of Chemical Hydride, Pressure Vessel, and Balance of Plant for On-Board Hydrogen Storage<br>*Jamie Holladay; Pacific Northwest National Laboratory* | 3.5 | X | | | This project is part of the HSECoE. The reviewers found the project to be highly relevant and the Pacific Northwest National Laboratory team to be very capable with appropriate areas of expertise. They commented favorably on the work performed in the prior year, especially the tank modeling and ammonia borane slurry work. However, several reviewers felt that the ammonia borane slurry work should be carried out in more depth, especially through investigations of micro-scale properties and detailed comparison of slurries with ammonia borane-ionic liquids developed by Los Alamos National Laboratory (LANL). |
| ST-006 | Advancement of Systems Designs and Key Engineering Technologies for Materials Based Hydrogen Storage<br>*Bart van Hassel; United Technologies Research Center* | 3.1 | X | | | This project is part of the HSECoE. The reviewers praised United Technologies Research Center's (UTRC's) role in the analyses of reversible metal hydrides systems that contributed to the down-select of metal hydride systems. The reviewers were satisfied with progress on the gas liquid separator, ammonia removal, and UTRC's role leading the integrated modeling framework. It was recommended that UTRC better describe their collaborations with HSECoE partners and how duplication of efforts between partners is avoided. |
| ST-007 | Chemical Hydride Rate Modeling, Validation, and System Demonstration<br>*Troy Semelsberger; Los Alamos National Laboratory* | 3.1 | X | | | This project is part of the HSECoE. The reviewers found the LANL project to be highly relevant to the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program). While they found the work and progress to be satisfactory, they commented that they would prefer to be presented with more details on the work to support the claims and more comparisons between fluids containing ammonia borane and other potential exothermic and endothermic release hydrogen carriers to justify the narrow focus on ammonia borane. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| ST-008 | System Design, Analysis, Modeling, and Media Engineering Properties for Hydrogen Energy Storage<br>*Matthew Thornton;*<br>*National Renewable Energy Laboratory* | 3.4 | X | | | This project is part of the HSECoE. The National Renewable Energy Laboratory's (NREL's) modeling and simulation activities were thought by the reviewers to be valuable to the HSECoE and very well done. While the collaborations between NREL and the other HSECoE partners were found to be excellent, comments were made that efforts need to be carried out to avoid duplication between partners. The reviewers found that NREL's materials evaluation activities appeared disconnected from their modeling and simulation activities and recommended a review and re-evaluation of the materials efforts. |
| ST-009 | Thermal Management of On-Board Cryogenic Hydrogen Storage Systems<br>*Darsh Kumar;*<br>*General Motors* | 3.1 | X | | | This project is part of the HSECoE. Reviewers observed General Motors (GM) is doing a good job using models and material properties to evaluate heat and mass transfer in order to help determine the optimal adsorption based storage system design. Overall, the reviewers were happy with the progress on the models for both the heating coil and the pelletized storage material, but also recommended validatation of the models with experimental results as a near-term priority. Concern was raised that the models will not be able to accurately predict the thermal behavior of the system given the variability in gas flow patterns in a packed bed system. |
| ST-010 | Ford/BASF-SE/UM Activities in Support of the Hydrogen Storage Engineering Center of Excellence<br>*Mike Veenstra;*<br>*Ford Motor Company* | 3.4 | X | | | This project is part of the HSECoE. Reviewers commented that Ford has made good progress on their activities related to vehicle parameter modeling, absorbent characterization, and as absorbent system lead. The reviewers also commented favorably on the addition of the Failure Mode and Effects Analysis and believe this work should help guide the HSECoE by determining key issues still to be addressed. It was recommended that Ford clearly delineate their characterization work on MOF-5 from that of other partners in the HSECoE to avoid overlap. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ST-014 | Hydrogen Sorbent Measurement Qualification and Characterization *Phil Parilla; National Renewable Energy Laboratory* | 3.4 | X | | | The reviewers recognized NREL as one of the few laboratories that can produce credible isotherms for hydrogen sorption materials over a wide range of pressure and temperature conditions of interest. Reviewers also praised the project for providing a robust validation mechanism and for working with other researchers to identify potential sources of error in their measurements. It was recommended that these results be published and this work be the basis used to validate compacted cryo-absorbent media from the HSECoE. |
| ST-019 | Multiply Surface-Functionalized Nanoporous Carbon for Vehicular Hydrogen Storage *Peter Pfeifer; University of Missouri* | 2.5 | | X | | The reviewers were encouraged by Missouri's ability to increase the binding energy by methodically doping the carbon-based materials with boron without significantly reducing the surface area. The reviewers also noted Missouri's characterization work and the importance of their approach to develop materials from low-cost raw materials. However, the reviewers commented that the developed materials are still well below the DOE volumetric and gravimetric capacity targets. Independent validation of the sample results was recommended. This project will undergo a phase I/II go/no-go decision at the end of fiscal year (FY) 2012. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ST-021 | Weak Chemisorption Validation<br>*Thomas Gennett;*<br>*National Renewable Energy Laboratory* | 3.0 | | | X | In this effort to determine the viability of hydrogen capacity enhancement through spillover, the reviewers commented that the inclusion of both spillover "supporters" and "doubters" to the team, given the broad disagreement in the scientific community, was very appropriate and critical. It was considered a major accomplishment that the consensus opinion of the team is that the data supports the spillover phenomenon as real, even though the enhancement was small for the particular samples investigated. Additionally the reviewers commented favorably on the breadth of spectroscopic techniques used to investigate the phenomenon. However, the reviewers would have preferred investigation of the enhancement on materials with higher overall hydrogen uptake to avoid reliance on correlation of such small spectra peaks to spillover. The ultimate capacity for spillover materials needs to be established to determine if spillover offers a viable pathway to meet DOE goals. |
| ST-022 | A Joint Theory and Experimental Project in the Synthesis and Testing of Porous COFs for On-Board Vehicular Hydrogen Storage<br>*Omar Yaghi;*<br>*University of California, Los Angeles* | 2.0 | | X | | The reviewers commented favorably on the concept of utilizing both experimental and computational efforts to screen, synthesize, and optimize new sorbent materials with potential to meet the DOE targets. However, the reviewers are highly concerned that the empirical and computational approaches in this project are not well focused or complementary to one another. The reviewers felt there is a lack of collaboration/engagement between the theoreticians and experimentalists and that there was no clear methodology linking the two efforts. The empirical effort appeared focused on quantity of structures and linkers produced rather than material optimization to reach DOE hydrogen storage targets. The reviewers were not satisfied with the progress made this year, with most of the accomplishments presented being from previous years, and with nomaterials developed thus far with potential to meet DOE goals. This project will undergo a phase I/II go/no-go decision at the end of FY 2012. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ST-023 | New Carbon-Based Porous Materials with Increased Heats of Adsorption for Hydrogen Storage *Randy Snurr; Northwestern University* | 3.3 | | | X | The reviewers recognized the high relevance of this project which utilizes a combined experimental and computational screening approach, addressing both gravimetric and volumetric targets, to help in the discovery of higher capacity materials. The reviewers noted the project was well focused and excellent progress has been made on the theoretical initiative. The project was also commended for collaboration between researchers as well as with NREL for independent validation of $H_2$ measurement. However, the reviewers questioned the validity of applying a normalization factor to the NREL data. This project will be completed at the end of FY 2012. |
| ST-024 | Hydrogen Trapping through Designer Hydrogen Spillover Molecules with Reversible Temperature and Pressure-Induced Switching *Angela Lueking; Pennsylvania State University* | 2.5 | | X | | The reviewers commended Penn State for modifying its original approach to address reproducibility issues with spillover and for the collaborations with DOE’s Basic Energy Sciences program and the coordinated effort with the NREL-led spillover task group. The reviewers were unclear as to how this project would generate new materials capable of meeting DOE targets and had questions on the focus of the project going forward. This project will undergo a phase I/II go/no-go decision in FY 2012. |
| ST-028 | Design of Novel Multi-Component Metal Hydride-Based Mixtures for Hydrogen Storage *Christopher Wolverton; Northwestern University* | 2.5 | | X | | The reviewers noted that, in general, the project objectives comply with the DOE targets and goals and that the computational effort has predicted some promising multi-component hydrides. However, the reviewers expressed concern about the lack of focus in meeting DOE targets and an apparent disconnect between the theoretical and experimental components of the project. Furthermore, there appears to have been an unnecessary duplication of previously published work. The reviewers commented that the employed characterization methods are not satisfactory and are insufficient and that nuclear magnetic resonance spectroscopy should be included. Development of a detailed and focused plan forward was recommended. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ST-040 | Liquid Hydrogen Storage Materials *Benjamin Davis; Los Alamos National Laboratory* | 3.3 | X | | | The reviewers commended the project for close collaboration and coordination with the HSECoE and directly addressing the HSECoE's needs with respect to liquid phase ammonia borane. It was recommended that the project develop an approach that can narrow the search for an additive that enables a fluid phase spent fuel while maintaining the required storage capacity and meeting the hydrogen purity requirements. |
| ST-044 | SRNL Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Metal Hydride and Adsorbent Systems *Ted Motyka; Savannah River National Laboratory* | 3.4 | X | | | This project is part of the HSECoE. Savannah River National Laboratory's (SRNL's) technical work in support of the HSECoE was found to be highly relevant. The validation of the HSECoE models is critical and the data generated on adsorbent systems through SRNL and their collaborators were considered vital in this effort. Comments were made to ensure that the models and system concepts can accommodate incorporation of improved materials as they are developed. Also, system cost projections need to be made. |
| ST-045 | Key Technologies, Thermal Management, and Prototype Testing for Advanced Solid-State Hydrogen Storage Systems *Joseph Reiter; NASA Jet Propulsion Laboratory* | 3.2 | | X | | This project is part of the HSECoE. The reviewers commented favorably on the progress made by the Jet Propulsion Laboratory (JPL), in particular on the modeling of cryogenic tanks and the development of a cryogenic tank burst facility. Reviewers noted some inconsistencies and issues with the outgassing of composite overwrapped pressure vessels that need further work. (Note from DOE: As a result of several factors, including the down-select of reversible metal hydride systems from the HSECoE's efforts, JPL's project scope has changed and JPL will not be continued as a full independent partner of the HSECoE after FY 2012.) |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ST-046 | Microscale Enhancement of Heat and Mass Transfer for Hydrogen Energy Storage<br>*Kevin Drost;*<br>*Oregon State University* | 2.9 | X | | | This project is part of the HSECoE. The reviewers noted that using micro-channel technology to reduce system weight, size, and complexity while also enhancing charging and discharge rates was relevant to the goals of the Program. The reviewers also stated that the Modular Absorption Tank Insert (MATI) has seen significant improvements over the last year. They recommended that an absorbent system using the MATI be compared against a baseline system to help illustrate the potential benefits of this approach. They also cautioned it may be difficult to insert the MATI device into a pressure vessel and recommended Oregon State test their concepts soon to determine feasibility and identify remaining development needs. |
| ST-047 | Development of Improved Composite Pressure Vessels for Hydrogen Storage<br>*Norman Newhouse;*<br>*Lincoln Composites* | 3.3 | X | | | This project is part of the HSECoE. The reviewers commended the progress Lincoln Composites has made over the last year including the fabrication of 21 common test vessels for use within the HSECoE for component testing and improvements Lincoln has made in vessel weight, volume, and cost reduction. In addition to vessel materials development, the reviewers commented favorably that Lincoln was expanding its scope to include Type I all-metal and Type III composite pressure vessels with metal liners in addition to Type IV all-composite pressure vessels; reviewers also commented favorably that Lincoln was investigating the ability to incorporate sorbent materials into the pressure vessel and the potential of their operation at cryogenic temperatures. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ST-048 | Hydrogen Storage Materials for Fuel Cell Powered Vehicles *Andrew Goudy; Delaware State University* | 2.5 | | | X | The reviewers recognized the principal investigator's expertise in performing studies on the thermodynamics and kinetics of hydrogen reactions with metal hydrides. However, the reviewers felt the complex hydride systems being investigated have limited potential to meet DOE targets given the high temperatures required for solid state diffusion. The reviewers suggested no further consideration be given to systems such as $LiBH_4/MgH_2$ or $LiNH_2/MgH_2$ because of considerable literature already available. The reviewers are not confident that deeper insights will result by continuing this work. |
| ST-053 | Lifecycle Verification of Polymeric Storage Liners *Barton Smith; Oak Ridge National Laboratory* | 3.1 | X | | | Overall the reviewers considered the ORNL hydrogen permeation measurements of tank liner materials to be highly relevant to the Program and well executed. Reviewers did comment that planned testing of liner material incorporated with the carbon fiber composite layer is criticaland should be done soon. It was also suggested that consideration be given to having the test methodology promoted as an industry standard. |
| ST-093 | Melt Processable PAN Precursor for High Strength, Low-Cost Carbon Fibers *Felix Paulauskas; Oak Ridge National Laboratory* | 2.7 | X | | | Reviewers commented that the goal of reducing carbon fiber precursor costs, with potential carbon fiber cost reductions of about 30%, is highly relevant and important for the Program. While the researchers were considered to be very qualified, addition of other collaborators was recommended. Delays due to issues with winding of the precursor fibers on the laboratory equipment were noted and quick resolution is needed. It was recomemended that conversion of the precursors to carbon fiber be carried out soon to evaluate the status and potential of the project to meet it goals. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ST-098 | Development of a Practical Hydrogen Storage System based on Liquid Organic Hydrogen Carriers and a Homogeneous Catalyst<br>*Craig Jensen;*<br>*Hawaii Hydrogen Carriers, LLC* | 2.7 | | | X | This project is focused on the development of Liquid Organic Carriers (LOCs), which the reviewers acknowledged as one of the more promising options for low-pressure automotive hydrogen storage with the potential to exploit the existing gasoline infrastructure. The reviewers felt the work was significantly similar to that previously done by Air Products albeit with the use of a homogenous catalyst as opposed to a heterogeneous catalyst. The reviewers expressed concern about the high cost of the iridium pincer catalyst and the LOC's limited gravimetric capacity of ~ 7 wt. %, which will not be able to meet the DOE Ultimate Targets. Good collaboration with GM and progress on reactor design were noted and collaborating with the HSECoE was recommended. |
| ST-099 | Development of Low-Cost, High Strength Commercial Textile Precursor (PAN-MA)<br>*Dave Warren;*<br>*Oak Ridge National Laboratory* | 2.9 | X | | | The reviewers commented that the goal of this project in significantly reducing the cost of carbon fiber precursors is both relevant to the Program and critical for successful commercialization of high-pressure hydrogen storage. The research team, while limited to two partners, was considered highly experienced and suitable for the research effort. To date, the progress has appeared somewhat slow and the reviewers would like to see increased rate of production, especially in converting the precursors to carbon fiber and evaluating the status and potential of the polymer formulations. |

* Permanent consecutive project numbers were assigned for the 2010 AMR to make administration of the AMR more efficient. As projects are discontinued, the corresponding project number is retired. As new projects are added, new consecutive numbers are added. As a result, there are gaps in the consecutive project numbers. In addition, some projects were not selected for review at the 2012 AMR and are not shown in the Prologue Table (see Appendix D for the full list of projects not reviewed).

## Fuel Cells

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-006 | Durable Catalysts for Fuel Cell Protection During Transient Conditions<br>*Radoslav Atanasoski;*<br>*3M* | 3.6 | X | | | Reviewers commented on significant improvements in catalyst durability using start-stop cycling protocols, showing <10% loss in electrochemically active surface area on the cathode after 5,000 cycles simulating start/stop. They also noted three orders of magnitude reduction in the oxygen reduction reaction (ORR) current at the anode and 200 cycles of 200 $mA/cm^2$ simulating cell reversal with anode loadings of 0.045 $mg/cm^2$ platinum group metal (PGM) with the upper potential remaining <1.8 V. Also, this project demonstrated improved tolerance to cell reversal in stack testing using the oxygen evolution reaction modified nanostructured thin film anode at an original equipment manufacturer (OEM). |
| FC-007 | Extended, Continuous Pt Nanostructures in Thick, Dispersed Electrodes<br>*Bryan Pivovar;*<br>*National Renewable Energy Laboratory* | 2.8 | X | | | Reviewers remarked that the research team has good depth in the area of carbon-free (platinum) material, and good approaches to understanding limitations, such as the dispersion of these novel materials and adding carbon. Reviewers expressed concern that too much emphasis has been put on rotating disk electrode (RDE) to predict performance and membrane electrode assemblies (MEAs) come too late in the project. |
| FC-008 | Nanosegregated Cathode Catalysts with Ultra-Low Platinum Loading<br>*Nenad Markovic;*<br>*Argonne National Laboratory* | 3.4 | X | | | According to reviewers, Argonne National Laboratory (ANL) is carrying out outstanding, thorough work providing an important framework to guide others by identifying the most important factors in making more active ORR catalysts. The reviewers stated that the accomplishments presented were excellent and impressive. They noted that more fuel cell performance and durability testing would be good, which seems to be the plan for future work. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-009 | Contiguous Platinum Monolayer Oxygen Reduction Electrocatalysts on High-Stability, Low-Cost Supports<br>*Radoslav Adzic; Brookhaven National Laboratory* | 3.6 | X | | | According to reviewers, Brookhaven National Laboratory's (BNL's) approach provides one of the most promising pathways to reduce the use of expensive platinum (Pt) in fuel cells and thus could allow fuel cells to become cost-competitive with other power sources for a range of applications. They noted that BNL has made considerable strides in reducing total precious metal use, as well as reducing Pt. Reviewers also lauded the progress on extending the testing of activity and durability in MEAs, complementing the rotating disk electrodes used in the past. They suggested that the trend towards working with practical MEAs should be continued. |
| FC-010 | The Science and Engineering of Durable Ultralow PGM Catalysts<br>*Fernando Garzon; Los Alamos National Laboratory* | 3.0 | X | | | The reviewers noted that the stated objectives of the project will deliver a high-performing, durable catalyst; however, there appears to be no clear path to meet that objective half way through the project. They state that the modeling work has progressed well but the experimental reduction to practice has lagged behind. The reviewers suggested that Los Alamos National Laboratory (LANL) focus on development and analysis of Pt/ Y,Sc nanoplatelet catalysts as well as the Pt nanotube work, and that the other approaches have lower priority. |
| FC-012 | Polymer Electrolyte Fuel Cell Lifetime Limitations: The Role of Electrocatalyst Degradation<br>*Deborah Myers; Argonne National Laboratory* | 3.3 | X | | | Reviewers stated that understanding the effect of Pt and Pt alloy properties on voltage decay is a critical issue that must be addressed to enable fuel cell vehicle commercialization. They noted that ANL has done an outstanding job of collecting and analyzing an abundance of data to highlight the key factors that impact voltage loss in dispersed PGM catalysts and MEAs. The reviewers asserted that the approach could benefit from more thorough statistical analysis to better identify second and higher order effects. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-013 | Durability Improvements through Degradation Mechanism Studies<br>*Rod Borup;*<br>*Los Alamos National Laboratory* | 3.2 | X | | | According to reviewers, LANL's project is well executed with a focus on the key parameters and interactions needed for high fuel cell durability without added costs. The reviewers noted that the approach taken by this team is comprehensive, coordinated, and well organized. They were intrigued by interesting results that elucidate catalyst degradation including catalyst agglomeration as a function of loading and localized structural changes. They stated that the new findings lead to new durability perspectives and insight into the multi-dimensional problem of degradation. |
| FC-014 | Durability of Low Pt Fuel Cells Operating at High Power Density<br>*Scott Blanchet;*<br>*Nuvera Fuel Cells* | 3.1 | X | | | According to reviewers, this project has a good collaboration partners, consistent experimental data sets, and a balanced approach of in situ and ex situ measurement correlations. Reviewers expressed concern about the low level of detail included on the model results and lack of clarity on how the system characteristics (e.g., thermal gradients in the cell) will be taken into account. Also, they suggested that the model more accurately address commercial fuel cell systems. |
| FC-016 | Accelerated Testing Validation<br>*Rangachary Mukundan;*<br>*Los Alamos National Laboratory* | 3.1 | X | | | The reviewers applauded LANL's efforts to correlate accelerated stress tests with real-world experience as it provides valuable information for fuel cell developers. The reviewers were disappointed that LANL was restricted to evaluating post-mortem from only bus stacks since no automotive data were available. Reviewers questioned the development of an accelerated stress test for gas diffusion layers since that failure mode is rare. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-017 | Fuel Cells Systems Analysis *Rajesh Ahluwalia; Argonne National Laboratory* | 3.4 | X | | | The reviewers stated that the accomplishments and progress of ANL's system analysis project were significant this year and were well presented. Reviewers felt that the modeling results on optimal low Pt loading were interesting and offer an opportunity to validate the model by comparing with experiment. Reviewers suggested integrating the temperature evolutions during power variations into the $N_2$ dilution and purge strategy would be interesting. |
| FC-018 | Manufacturing Cost Analysis of Fuel Cell Systems and Transportation Fuel Cell System Cost Assessment *Brian James; Strategic Analysis, Inc.* | 3.4 | X | | | Reviewers believe the cost analysis of fuel cell systems is essential for assessing the state of the technology and guiding research. It was suggested that for MEAs, platinum alloy dispersed on carbon should be analyzed as well as nanostructured thin film options. Although progress slowed on the automotive analysis, reviewers were impressed with the rapid progress in the new stationary analyses. The methodology has led to high confidence in results and provides major insights into what the major cost drivers are for the fuel cell systems analyzed. Reviewers provided valuable suggestions, including a suggestion to analyze the cost of fuel cell systems for medium-duty trucks. |
| FC-020 | Characterization of Fuel Cell Materials *Karren More; Oak Ridge National Laboratory* | 3.3 | X | | | Reviewers state that this is a valuable, core project in the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program). It provides broad access to electron microscopy for projects studying fuel cell materials, in particular the structure and distribution of catalyst particles. This project has a continuing history of providing insight into fuel cell functions using the newest techniques of microscopy. Suggestions for future improvement included development of in situ liquid TEM techniques. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-021 | Neutron Imaging Study of the Water Transport in Operating Fuel Cells *Muhammad Arif; National Institute of Standards and Technology* | 3.3 | X | | | Reviewers noted that neutron imaging is a powerful tool for probing into an operating fuel cell and National Institute of Standards and Technology's (NIST's) neutron facility provides unique capability to DOE and industry researchers. NIST has clearly been successful in developing advanced tools to meet customer needs as seen by the numerous projects that have been effectively conducted at the facility. Reviewers would like to learn how such experiments advance the understanding of fuel cells and how neutron imaging will ultimately help reach DOE's technical targets. They noted that more extensive error and uncertainty analysis would be helpful. |
| FC-026 | Fuel-Cell Fundamentals at Low and Subzero Temperatures *Adam Weber; Lawrence Berkeley National Laboratory* | 3.1 | X | | | Reviewers state that this project is well designed to answer the complex question of durability mechanisms under freeze conditions. Recognizing the role of freezing in altering the nature of a 3-phase interface, as in the case of a conventional polymer electrolyte membrane (PEM) fuel cell, this effort combines advanced tools such as small angle X-ray scattering, dynamic scanning calorimetry, and high frequency resistance with modeling of transport and kinetics. Good progress has been made in implementing this strategy with good collaborative effort. Reviewers suggested adding studies of a larger variety of MEA configurations and of stack-level freeze cycle transients. |
| FC-028 | Transport Studies Enabling Efficiency Optimization of Cost-Competitive Fuel Cell Stacks *Amedeo Conti; Nuvera Fuel Cells* | 3.1 | | | X | Reviewers commented that the performance and modeling targets of the project have been met at low loadings. However, they expressed concern about durability, noting that data at higher temperatures and heat rejection findings should be reported. Reviewers also expressed concern about lack of published results and recommended disclosure of the model in a peer-reviewed forum. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-032 | Development of a Low Cost 3-10kW Tubular SOFC Power System<br>*Norman Bessette;*<br>*Acumentrics Corporation* | 3.1 | X | | | Reviewers commented that the project addressed several of the Program's goals focusing on critical barriers of performance, durability, and cost for solid oxide fuel cell power plants for stationary power applications. Reviewers commented favorably on the progress achieved by increasing power plant performance and reducing manufacturing cost. While the project is close to completion, proposed remaining efforts were deemed reasonable. |
| FC-036 | Dimensionally Stable High Performance Membranes<br>*Cortney Mittelsteadt;*<br>*Giner Electrochemical Systems, LLC* | 2.9 | X | | | This project aims to develop low-cost fabrication processes for new support layers with micro-fabricated openings for dimensionally stable membranes. Reviewers stated that the approach of pursuing three processes in parallel increases the chances of achieving both the performance as well as cost targets; and the progress and collaborations are good. Some reviewers questioned the need for better membrane supports as opposed to better membranes. Suggestions for improvement included down-selection to one process, testing membrane in MEA, use of current distribution modeling for designing the structure, testing MEA's with thinner supports, and providing details on potential cost reduction. |
| FC-044 | Engineered Nano-scale Ceramic Supports for PEM Fuel Cells<br>*Eric Brosha;*<br>*Los Alamos National Laboratory* | 2.8 | X | | | According to reviewers, overall good progress was made in electrochemical evaluation by RDE and MEA, even if the results were not that good. The reviewers noted that LANL showed that significant carbon content contributes to a lack of durability under severe carbon-corrosion tests. The reviewers also noted that LANL attempted incorporation into MEAs and MEA testing, although performance is poor due to integration issues. The reviewers suggested that more emphasis needs to be placed on direct measurements of conductivity after exposure to relevant conditions. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-048 | Effect of System Contaminants on PEM Fuel Cell Performance and Durability *Huyen Dinh; National Renewable Energy Laboratory* | 2.9 | X | | | Reviewers commended the extensive knowledge base about contaminants, the in-situ and ex-situ testing capabilities, and the collaborations with stack manufacturers. Some questions remain for each contaminant. Reviewers wondered what the mechanism of degradation is, if the degradation is recoverable at a lower concentration, if contaminants crossover to the anode, and what the mechanism of recovery is, if one exists. Reviewers suggested that more efforts should be directed towards understanding what the accelerating effects are for different species and towards identifying the critical levels for each type of contaminant. |
| FC-049 | Development of Micro-Structural Mitigation Strategies for PEM Fuel Cells: Morphological Simulations and Experimental Approaches *Silvia Wessel; Ballard* | 3.5 | X | | | The reviewers commented that progress shown has been very good, milestones have been met, and the model-based simulations have been experimentally validated. The reviewers expressed concern, however, about how specific the model is to Ballard technology, and the extent to which this model will be available for other fuel cell developers. |
| FC-052 | Technical Assistance to Developers *Tommy Rockward; Los Alamos National Laboratory* | 3.4 | X | | | According to reviewers, this project was more relevant to DOE goals than last year as LANL focused more on tasks that addressed essential questions related to fuel cell performance and durability. Reviewers noted that significant work was completed in many different areas including microporous layers, startup/shut down, and hydrophobic bipolar plate treatments. Reviewers also noted that it is a great idea to have one of the most credible teams in the fuel cell field helping developers meet the developmental targets for their fuel cell component technologies. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-054 | Transport in PEMFC Stacks<br>*Cortney Mittelsteadt;*<br>*Giner Electrochemical Systems, LLC* | 2.9 | X | | | The objective of this project is to improve understanding of the correlation between material properties and model equations of PEM fuel cell stacks. Reviewers agreed that the project relevance and team of collaborators are very good. However, there were divergent opinions on the approach and progress. Suggestions for improvement included increasing focus on the stack transport issues, instead of cells and components; conducting more sensitivity analysis using the model; reporting on thermal conductivity and catalyst layers; providing greater detail on the methodology behind the model treatment of water transport in gas diffusion layers; and publishing results and making them more readily available to the community. |
| FC-063 | Novel Materials for High Efficiency Direct Methanol Fuel Cells<br>*David Mountz;*<br>*Arkema* | 2.6 | | X | | Reviewers felt the project relevance for direct methanol fuel cells (DMFC) and the team were very good; however, the progress and results to date were lacking. The approach for membrane development is good, but the catalyst development work has not been sucessful. Reviewers recommended that the remaining resources should be shifted to focus on developing membranes with high conductivity and reduced methanol permeability. MEA performance testing and efficiency validation should be carried out using DMFC benchmark catalysts. |
| FC-064 | New MEA Materials for Improved DMFC Performance, Durability, and Cost<br>*Jim Fletcher;*<br>*University of North Florida* | 3.1 | | | X | The reviewers stated that this project had a strong team, had good success with water management issues, and demonstrated a DMFC system with long-term durability. The project took a practical engineering approach with strong linkages with an industrial catalyst partner to enable commercialization. Suggested improvements included further optimization of water management, increasing temperature while maintaining water balance, and optimization of system operating parameters and conditions. This project ended June 2012. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-065 | The Effect of Airborne Contaminants on Fuel Cell Performance and Durability *Jean St-Pierre; Hawaii Natural Energy Institute* | 3.0 | X | | | According to reviewers, the effect of air-borne impurities on fuel cell performance is important to study. Reviewers noted that the team's approach is good, that the results generated are reliable, and the models that result will be useful. Reviewers would welcome a more in-depth understanding of the causes of observed performance losses and were concerned that the data set is not sufficiently large to make the models as reliable as possible. Reviewers suggested that the University of Hawaii recommend tolerance limits for the contaminants. |
| FC-067 | Materials and Modules for Low-Cost, High-Performance Fuel Cell Humidifiers *Will Johnson; W.L. Gore* | 3.1 | | | X | Reviewers agreed that humidifiers are a key component for fuel cell system performance. The reviewers thought the approach was sound; however, some would like the project expanded to consider a larger set of membrane materials and some wanted to see emphasis on module design. Reviewers commended the work in understanding durability loss. |
| FC-070 | Development of Kilowatt-Scale Coal Fuel Cell Technology *Steven Chuang; University of Akron* | 1.8 | | | X | The reviewers commented that the project is not relevant to the Program's goals, as it is using coal as a potential fuel for fuel cells. Furthermore, they commented that while the direct coal utilization fuel cell concept offers potential for carbon capture, the project had only demonstrated modest progress. It was also stated that while the project has been completed, it would have benefited from the inclusion of a major solid oxide fuel cell developer. |
| FC-072 | Extended Durability Testing of an External Fuel Processor for SOFC *Mark Perna; Rolls-Royce Fuel Cell Systems (US) Inc.* | 3.2 | | | X | According to reviewers, the project pertained to the Program's objectives as it involved the development of fuel processing subsystems in support of solid oxide fuel cells for distributed power generation. Reviewers commended the project for having a good program plan and well-defined targets, and for the technical progress it has achieved. The project has been completed with subsystems developed and tested for durability. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-076 | Biomass Fuel Cell Systems<br>*Neal Sullivan;*<br>*Colorado School of Mines* | 2.8 | | | X | The reviewers noted that this project is a good combination of academic expertise and industry experience, and there is good correlation between modeling and hardware testing. However, reviewers thought that the project's progress was slow given the three-year project timeline. The reviewers suggested that the remaining time should be spent testing the long-term durability of the fuel cell stack using biogas. |
| FC-077 | Fuel Cell Coolant Optimization and Scale-up (plus work under SBIR III project)<br>*Satish Mohapatra;*<br>*Dynalene* | 2.9 | X | | | According to reviewers, the project's innovative approach may lead to a significant positive impact on the fuel cell system in terms of mass, weight, and maintenance by eliminating the resin filter. The reviewers expressed concern about the long term durability and identification/understanding of degradation mechanisms. The Small Business Innovation Research (SBIR) portion of the project is continuing; the congressionally directed project (CDP) has ended. |
| FC-078 | 21st Century Renewable Fuels, Energy, and Materials Initiative<br>*Joel Berry;*<br>*Kettering University* | 2.1 | | | X | The reviewers stated this project is unfocused, that it does not appear to have specific targets or milestones for performance, and much of the project is not aligned with the Program's goals. According to the reviewers, the fuel cell membrane work was able to improve on the conductivity they obtained with polybenzimidazole-phosphoric (PBI-phosphoric) acid by doping the PBI-phosphoric acid with sulfonated-polyhedral oligomeric silsesquioxane (SPOSS), but have not improved on the high-temperature conductivity of PBI-phosphoric acid reported in the literature. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-079 | Improving Fuel Cell Durability and Reliability *Prabhakar Singh; University of Connecticut Global Fuel Cell Center* | 2.4 | | | X | Reviewers identified this project as atypical, as it is comprised of 10 independent projects, with program objectives being addressed through a wide range of interactions with industry partners. According to reviewers, the project has a broad approach and improved coordination between the different tasks is required. They identified work covering some of the technical areas, such as molten carbonate fuel cells, which would otherwise be absent from the Program's portfolio. Reviewers felt that the project has demonstrated progress in at least some areas, and commended the project on its multiple collaborations between academic partners and industry. It was suggested that, for the remaining duration of the project, some of the project's tasks need to be reevaluated and prioritized. |
| FC-081 | Fuel Cell Technology Status—Voltage Degradation *Jennifer Kurtz; National Renewable Energy Laboratory* | 3.3 | X | | | The reviewers commended the break down of results from different platforms, test configurations, and test protocols, as well as break down among tests run using steady state, duty cycle, and accelerated protocols. The reviewers also commended the new website. The reviewers would like to see additional data and data types, as well as an enhancement of the accuracy of the decay prediction. |
| FC-083 | Enlarging Potential National Penetration for Stationary Fuel Cells Through System Design Optimization *Chris Ainscough; National Renewable Energy Laboratory* | 2.3 | | X | | Reviewers saw the potential value of the model as a high-level policy analysis tool and for providing a collection of building energy characteristics appropriate for combined heat and power systems. They were uncertain whether details of fuel cell systems in the model were sufficient. Reviewers highlighted that collaboration and leveraging NREL's buildings program is the right path and will be extremely important to the success of this project. Progress has been slow with the change in PI and the first useful results are expected in fiscal year (FY) 2013. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-084 | WO3 and HPA Based Systems for Durable Platinum Catalysts in PEMFC Cathodes *John Turner; National Renewable Energy Laboratory* | 2.7 | X | | | The reviewers commended the strength of the research team. According to the reviewers, some progress was shown in the synthesis and feasibility of new supports and increased durability using new supports, but no data was presented on reduced cost or increasing performance. The reviewers were concerned about the number of approaches being investigated. |
| FC-085 | Synthesis and Characterization of Mixed-Conducting Corrosion Resistant Oxide Supports *Vijay Ramani; Illinois Institute of Technology* | 3.0 | X | | | According to the reviewers, the project has shown the potential of using mixed proton electron conducting materials for improving support corrosion resistance, and it has shown good corrosion resistance for some model systems. The reviewers also noted the involvement of an automotive OEM as a strength. The reviewers expressed concern about the high Pt loading used. The reviewers recommended durability tests on the materials for downselection. Cost analysis was also recommended. |
| FC-086 | Development of Novel Non-Pt Group Metal Electrocatalysts for Proton Exchange Membrane Fuel Cell Applications *Sanjeev Mukerjee; Northeastern University* | 2.9 | X | | | Reviewers commended the approach and the level of collaboration. They noted the high ORR activity for 2–3 electrocatalysts and the improved stability, as well as the strength of the mechanistic studies. Reviewers cautioned against extrapolation of Tafel slopes using few points to project activity. Reviewers also recommended additional effort to verify the reaction mechanisms and more long-term stability tests for the catalysts. |
| FC-087 | High-Activity Dealloyed Catalysts *Fred Wagner; General Motors* | 3.4 | X | | | Reviewers commented favorably on the project's well-designed and well-focused approach and the good collaboration between all of the project's participants. The project was further praised for the good progress achieved to date toward meeting catalyst activity and durability targets. However, concern was expressed whether catalyst activity and durability targets could be met simultaneously by a single sample. Some reviewers suggested that focusing more on evaluating durability would benefit the project. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-088 | Development of Ultra-Low Platinum Alloy Cathode Catalyst for PEM Fuel Cells<br>*Branko Popov;*<br>*University of South Carolina* | 3.2 | X | | | Reviewers agree that this project is highly relevant to DOE objectives of platinum reduction in the fuel cell catalyst to reduce cost and enable commercialization. The outstanding approach, progress, and team collaborations were noted. Suggestions for improvement included developing more durability data; providing a more detailed explanation of methods, catalyst formulation, and test data; clarifying the effect of membrane on performance; and focusing on one or two high-potential catalysts. |
| FC-089 | Analysis of Durability of MEAs in Automotive PEMFC Applications<br>*Randy Perry;*<br>*DuPont* | 2.3 | | X | | The reviewers found the background work performed by Nissan to be the project's strength, and the details of the test protocols informative and helpful in interpreting the results. The reviewers were concerned that the accomplishments have been limited, specifically the contributions from IIT. Furthermore, the reviewers noted the development of the model based on experimental/characterization results and that the application of the suite of characterization techniques listed were not evident. Further review of this project and progress towards its go/no-go decision milestone is needed. |
| FC-090 | Corrugated Membrane Fuel Cell Structures<br>*Stephen Grot;*<br>*Ion Power* | 2.6 | X | | | The relevance and approach of this novel stack design concept was rated good by the reviewers; however, the reviewers had reservations about the likelihood for success due to many new issues associated with the innovative design. The reviewers rated the collaboration team highly. There is a lack of progress in demonstrating the feasibility of the concept due to delays in initiating a subcontract. A Go/No Go decision will be made in FY 2013. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-091 | Advanced Materials and Concepts for Portable Power Fuel Cells<br>*Piotr Zelenay;*<br>*Los Alamos National Laboratory* | 3.4 | X | | | Reviewers thought that the LANL project had a good balance between introducing new materials into direct methanol fuel cells and working on enabling technology for next generation fuels. Reviewers applauded the multi-directional approaches taken for the completion of all tasks and the impressive performance observed with multi-block copolymer membranes. Reviewers recommended increased interactions with SFC Energy to ensure practicality and progress against realistic goals and operating scenarios. |
| FC-092 | Investigation of Micro- and Macro-Scale Transport Processes for Improved Fuel Cell Performance<br>*Jon Owejan;*<br>*General Motors* | 3.6 | X | | | Reviewers rated this project as excellent in all aspects: Relevance, Approach, Progress, Collaboration, and Future Work. Suggestions for refinement included: develop the measurement techniques for the catalyst layer; extend the study to include durability/duty-cycle aspects; and include thin catalyst layers, such as nano-structured thin film. Reviewers felt that this project had outstanding synergy between experiment and model along with a strong team and that the results will be highly relevant and useful to the fuel cell community. |
| FC-096 | Power Generation from an Integrated Biomass Reformer and Solid Oxide Fuel Cell (SBIR Phase III)<br>*Quentin Ming;*<br>*InnovaTek* | 3.1 | X | | | According to the reviewers, good progress has been made, and most technical milestones have been met. Reviewers noted the initial test of bio-kerosene reforming has been successful in long term testing and operation on bio-fuels producing power is impressive. However, the reviewers noted the critical technical milestone to achieve 40% system efficiency has not been met (achieved only 27.5%). The reviewers also expressed concern about whether the second generation will achieve required durability. |

| Project Number* | Project Title<br>*Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| FC-101 | PEM Stationary Power Plant<br>*Tom Skiba;*<br>*UTC Power* | 2.5 | | X | | While the reviewers noted the technically strong team, they were very concerned about the limited progress based on the time and money spent on the project. The reviewers noted the number of stop-starts throughout the project's history resulting in a lack of continuity of results. The reviewers also noted the results do not give high confidence that the system proposed can meet the technical targets. Further review of this project and progress towards its go/no-go decision milestone is needed. |
| FC-102 | New High Performance Water Vapor Membranes to Improve Fuel Cell Balance of Plant Efficiency and Lower Costs<br>*Earl Wagener;*<br>*Tetramer Technologies, LLC* | 3.0 | | | X | This project was a nine-month Phase I SBIR project. According to reviewers, the project focused on one narrow aspect of fuel cells, membrane hydration, so the project is not as broad as some of the other DOE-funded activities. Reviewers found it difficult to evaluate the approach as it is proprietary, but lauded the good progress in the area of performance. Reviewers stated that significant challenges have to be addressed in the area of durability. |

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## Manufacturing R&D

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| MN-001 | Fuel Cell MEA Manufacturing R&D<br>*Michael Ulsh;*<br>*National Renewable Energy Laboratory* | 3.4 | X | | | According to reviewers, the results from the National Renewable Energy Laboratory's (NREL's) project are excellent. Specific project strengths cited include demonstrating the viability of a number of on-line detection tools under commercial operating conditions and expanding into other fuel cell technologies (e.g., solid oxide fuel cells). Reviewers suggested that a prioritized list of defect types and sizes from each supplier may be helpful for coordinating the segmented cell analysis. |
| MN-004 | Manufacturing of Low-Cost, Durable Membrane Electrode Assemblies Engineered for Rapid Conditioning<br>*Colin Busby;*<br>*W.L. Gore* | 3.4 | X | | | According to reviewers, W.L. Gore showed progress in the performance of direct coated cathodes. Reviewers felt that eliminating/reducing backer material will realize significant cost savings in membrane electrode assembly (MEA) production. In addition, they noted that reducing the number of coating passes will reduce MEA costs. |
| MN-005 | Adaptive Process Controls and Ultrasonics for High Temperature PEM MEA Manufacture<br>*Dan Walczyk;*<br>*Rensselaer Polytechnic Institute* | 3.4 | X | | | According to reviewers, the detailed program approach appears to be sound and well thought out and there has been reasonable progress in testing and validating ultrasonically bonded MEAs. They noted favorably that the team included Rensselaer Polytechnic Institute as the lead, BASF (MEA manufacturer), Ballard (stack manufacturer), UltracCell (system manufacturer) and NREL (national lab). Reviewers stated that the accomplishments have been outstanding and the payback potential appears huge. Reviewers suggested that the costing data be published. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| MN-006 | Metrology for Fuel Cell Manufacturing *Eric Stanfield; National Institute of Standards and Technology* | 3.2 | X | | | The reviewers noted that the non-contact dimension sensor and the scatterometry measurement systems can be very important in reducing manufacturing costs and enabling improved quality for both bipolar separators and catalyzed MEAs. While the reviewers found that the scatterfield metrology effort produced lots of results, they thought that it would be good to show how the results would lead to improved quality control and reduced component cost. The reviewers praised the the highly qualified and experienced team at National Institute of Standards and Technology for successfully collaborating with numerous organizations to prove the viability of the two approaches. |
| MN-007 | High Speed, Low Cost Fabrication of Gas Diffusion Electrodes for Membrane Electrode Assemblies *Emory De Castro; BASF* | 3.4 | X | | | According to reviewers, the overall approach of the project is solid and the progress and accomplishments are impressive. The reviewers noted that a four-fold increase in throughput rate, an order of magnitude reduction in defects, and a five-fold reduction in variation of Pt loading were very impressive accomplishments. They also noted that the 75% cost reduction attributed to reducing labor hours was a significant achievement for this project. The reviewers suggested that BASF carry out durability testing of the new catalyst layers. |
| MN-008 | Development of Advanced Manufacturing Technologies for Low Cost Hydrogen Storage Vessels *Mark Leavitt; Quantum Fuel Systems Technologies Worldwide, Inc.* | 3.0 | X | | | According to reviewers, the project directly addresses the U.S. Department of Energy's Hydrogen and Fuel Cells Program goals with an approach which will bring down the cost of Type IV storage tanks through improved utilization of carbon fiber. Reviewers noted that initial tests demonstrated good durability using less carbon fiber; however, some reviewers noted that, even if carbon fiber is reduced by as much as 20%–25%, carbon fiber reduction is not sufficient to substantially reduce the cost of hydrogen storage. Reviewers suggested that a thorough cost analysis needs to be performed and released. |

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## Technology Validation

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| TV-001 | Controlled Hydrogen Fleet and Infrastructure Analysis *Keith Wipke; National Renewable Energy Laboratory* | 3.8 | | | X | Reviewers concurred with the great importance of this project for providing a statistically valid understanding of the status of technology development relative to the U.S. Department of Energy's (DOE's) Hydrogen and Fuel Cells Program goals. They noted that the approach that has been developed is solid and is serving as a model for collecting data for additional fuel cell applications. Reviewers agreed that collaboration was essential and outstanding. The only weaknesses that reviewers mentioned were that the project was not broader in both participants and time. |
| TV-008 | Technology Validation: Fuel Cell Bus Evaluations *Leslie Eudy; National Renewable Energy Laboratory* | 3.3 | X | | | Reviewers recognize the importance of fuel cell buses in DOE's portfolio of fuel cell development and demonstration. It was suggested that the National Renewable Energy Laboratory's collection, analysis, and reporting of performance data have made a vital contribution to the status of the technology relative to newly developed targets for bus applications. Reviewers agreed that the project features good collaboration. Their recommendations included acquiring data on other types of hybrid buses for comparison; improving access to warranty repair costs; and increasing the ability to compare buses with similar age, size, and service conditions. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| TV-012 | Florida Hydrogen Initiative (FHI) *David Block; University of Central Florida* | 2.3 | | | X | Reviewers noted that many of the individual projects that make up the Florida Hydrogen Initiative were relevant to the Program's objectives, but were largely not relevant to the technology validation sub-program. One reviewer distinguished two projects as achieving good results. These included "Advanced HiFoil Bipolar Plates" (Enerfuel) and "Low Cost High Efficiency 500 W Portable PEM Fuel Cell " (Florida State University/Bing). The reviewer also noted "EV Charging Station Powered by a Fuel Cell" (Enerfuel and Florida Atlantic) achieved moderate success. Reviewers suggested that stronger coordination or a unified goal for the project may have had a greater benefit for the Florida Hydrogen Initiative. |
| TV-015 | Wind to Hydrogen *Kevin Harrison; National Renewable Energy Laboratory* | 3.0 | X | | | Reviewers believe that validation of electrolysis is essential and that supporting renewable energy with hydrogen storage and ancillary services is much needed. Reviewers commented that the broad scope of activities is a weakness and the project activities should be more focused on validation of DOE's hydrogen production targets. Focusing on a robust user facility was suggested by one reviewer while another suggested that precompetitive technology be the focus. |

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## Safety, Codes and Standards

| Project Number* | Project Title<br>*PI Name & Organization* | Final Score | Continue | Discontinue / Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| SCS-001 | National Codes and Standards Coordination<br>*Carl Rivkin;*<br>*National Renewable Energy Laboratory* | 3.4 | X | | | Reviewers recognized this project as essential to the development of Codes and Standards to enable the wide deployment of hydrogen and fuel cell technologies. Reviewers emphasized the strong leadership and appropriate coordination with critical code development organizations and standards development organizations (SDOs). However, they also thought that a focus, as it pertains to the deployment in California, could be added. In addition, simplified approaches providing key pieces of information, such as what gaps and risks exist, would be helpful. |
| SCS-002 | Component Standard Research & Development<br>*Robert Burgess;*<br>*National Renewable Energy Laboratory* | 3.0 | X | | | Reviewers acknowledged the technical focus and progress of this project. Specific strengths cited included the project's strong coordination with sensor manufacturers and domestic and international SDOs. However, the role of sensor testing at the National Renewable Energy Laboratory (NREL) in regards to industry and the impact on standards were not clear. Reviewers suggested several topics for future investigation, including wide area sensors, sensor testing with higher concentrations of hydrogen, and non-metallic materials used on the low pressure side of the fuel cell system. |
| SCS-004 | Hydrogen Safety, Codes and Standards: Sensors<br>*Eric Brosha;*<br>*Los Alamos National Laboratory* | 3.0 | X | | | Reviewers observed good progress toward developing a reliable, cost-effective hydrogen safety sensor for hydrogen infrastructure and stationary fuel cell applications. Reviewers commented on the project team's strong technical expertise and the involvement of industry. Reviewers indicated that the role of the participating commercial partner and the plan forward for commercialization was unclear. Reviewers recommended a stronger role from industry partner(s) and a cost analysis for the manufacturing of the sensor. |

| Project Number* | Project Title *PI Name & Organization* | Final Score | Continue | Discontinue / Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| SCS-005 | R&D for Safety, Codes and Standards: Materials and Components Compatibility *Daniel Dedrick; Sandia National Laboratories* | 3.3 | X | | | According to the reviewers, the project team has a significant impact on standards development and it is clearly filling a critical industry need. They cited excellent international collaborations with research institutions, original equipment manufacturers, and standards organizations. However, they recommended better defining the roles of participants and focusing on other materials used elsewhere in the world. In addition, they suggested that material testing should be conducted as applied to specific standards (e.g., SAE J2579) and include components in extreme cold conditions such as -40$^{o}$C. |
| SCS-006 | Hydrogen Safety Knowledge Tools *Linda Fassbender; Pacific Northwest National Laboratory* | 3.3 | X | | | Reviewers continued to praise the project's progress and ability to maintain critical hydrogen community resources. Reviewers acknowledged the use and increase of website visits, the reference by the Chemical Safety Board, and international collaboration with the International Association for Hydrogen Safety as important accomplishments. However, there was concern on the future direction of disseminating the information internationally and to key personnel such as Authorities Having Jurisdiction. The reviewers suggested examining advanced media options in order to obtain wider reach and exposure. |
| SCS-007 | Hydrogen Fuel Quality *Tommy Rockward; Los Alamos National Laboratory* | 3.8 | X | | | Reviewers commented that this project is making steady progress with an internationally accepted, science-based approach to testing and developing hydrogen fuel quality standards. Project strengths mentioned included a strong technical approach and good collaborations with industry and standard development organizations. Reviewers observed the important need to maintain a grasp on the advancement in fuel cell technologies. The reviewers suggested testing on short stacks and considering containments that can be found in a "system environment" from delivery to storage to automobile. |

| Project Number* | Project Title<br>*PI Name & Organization* | Final Score | Continue | Discontinue / Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| SCS-008 | Hydrogen Safety Panel<br>*Steven Weiner;*<br>*Pacific Northwest National Laboratory* | 3.2 | X | | | According to reviewers, this project continues to be a key resource—not just for the Safety, Codes and Standards (SCS) sub-program, but also for the overall DOE Hydrogen and Fuel Cells Program (the Program)—and is a “go-to” resource for the community. Examples of cited strengths of the panel include technical expertise, interest in promoting a safety culture, the continuous effort involved in performing site visits, and providing recommendations for the Program’s projects. Reviewers felt the work of the panel was straightforward and could benefit from evolution in its approach. Suggestions for consideration included involvement with third parties (e.g., insurers) and coordination with NREL’s data collection through the Technology Validation sub-program. |
| SCS-010 | R&D for Safety, Codes and Standards: Hydrogen Behavior<br>*Daniel Dedrick;*<br>*Sandia National Laboratories* | 3.1 | X | | | Reviewers commented favorably on this project’s ability to provide a science-based approach in hydrogen behavior as it relates to hydrogen release, ignition, and radiation. They recognized the strong collaborations from industry and international SDOs. Due to the large amount of information presented, the impact on the codes and standards efforts was not sufficiently well conveyed. The reviewers suggested continued coordination with appropriate SDOs as well as inclusion of relevant accident scenarios. |
| SCS-011 | R&D for Safety, Codes and Standards: Risk Assessments<br>*Daniel Dedrick;*<br>*Sandia National Laboratories* | 3.5 | X | | | The reviewers observed that the risk-informed approach built by the team and the incorporation of this approach into the codes and standards development process continue to be major accomplishments for SCS. It was observed that the approach applies relevant scientific input from targeted numerical and experimental efforts along with consultation and input from stakeholders. With this said, the reviewers commented that there should be a stronger push to obtain relevant industry data. In addition, reviewers suggested that a web-based quantitative risk assessment tool be developed and the incorporation of obstacles in indoor releases be added to the project scope. |

| Project Number* | Project Title<br>*PI Name & Organization* | Final Score | Continue | Discontinue / Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| SCS-015 | Hydrogen Emergency Response Training for First Responders<br>*Monte Elmore;*<br>*Pacific Northwest National Laboratory* | 3.6 | X | | | Reviewers commended this project for the progress it has made. They specifically recognized the hands-on training and knowledge of the instructors as invaluable. The reviewers expressed concern surrounding the potential loss of funding before the anticipated vehicle deployment in 2015. The reviewers suggested additional coordination with other key agencies such as emergency medical services, national fire academies, and other important regional/state organizations. |

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## Education

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| ED-010 | Development of Hydrogen Education Programs for Government Officials *Shannon Baxter-Clemmons; South Carolina Hydrogen and Fuel Cell Alliance* | 3.6 | | | X | Reviewers commended this project for taking a personal, well-focused approach to interfacing with government and business leaders about hydrogen and fuel cells. In addition, they complimented the project for its economic impact approach that highlights energy and environmental benefits. The reviewers noted the strong collaborations, including a combination of universities, industry, and government. The reviewers agreed with the approach of using the lessons already learned in South Carolina to help neighboring states develop their education efforts for hydrogen and fuel cells, and they would like to see a more aggressive effort in this area. This project is fully funded and will be completed in early 2013. |
| ED-013 | Raising H2 and Fuel Cell Awareness in Ohio *Pat Valente; Ohio Fuel Cell Coalition* | 3.5 | | | X | Reviewers noted that the Ohio Fuel Cell Coalition (OFCC) is very strategic and tactical in their approach. Several reviewers noted the OFCC's high level of collaboration with several university and industrial partners and noted that they do an excellent job of staying in touch with the activities in Ohio. Reviewers agreed with the project's approach of using forums and business-to-business networking and matchmaking, particularly the supply chain exchange. Due to OFCC's success in Ohio, reviewers would like to see this project expand their efforts to include neighboring states. This project is fully funded and will be completed in 2012. |

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## Market Transformation

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| MT-004 | Direct Methanol Fuel Cell Material Handling Equipment Deployment *Todd Ramsden; National Renewable Energy Laboratory* | 3.1 | X | | | Reviewers commented that this project was cleary relevant and good project planning execution has been made. Several reviewers commented that there should be more than one fuel cell developer. Although technical problems have been numerous, commenters stated that the problem resolutions along the way have been highly effective. |
| MT-006 | Fuel Cell Combined Heat and Power Industrial Demonstration *Dale King; Pacific Northwest National Laboratory* | 2.8 | X | | | Several reviewers commented that this project was highly relevant. More emphasis on collecting and analyzing real-world project data and less on modeling was recommended. One fuel cell supplier was considered inadequate by some reviewers who suggested adding more suppliers and fuel cells—e.g., solid oxide fuel cells. |
| MT-007 | Landfill Gas-to-Hydrogen *Shannon Baxter-Clemmons; South Carolina Hydrogen and Fuel Cell Alliance* | 3.4 | X | | | Excellent project relevance and planning were the comments from reviewers. Also, outstanding partnering and funds leveraging was observed. |
| MT-008 | Hydrogen Energy Systems as a Grid Management Tool *Mitch Ewan; Hawaii Natural Energy Institute* | 2.9 | X | | | A clearer relevance to the U.S. Department of Energy's (DOE's) Hydrogen and Fuel Cells Program goals is needed according to several reviewers. While high DOE funds leveraging was noted, schedule slippage related to DOE partners was considered a significant impediment toward a timely project completion. |

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## Systems Analysis

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| AN-001 | Infrastructure Analysis of Early Market Transition of Fuel Cell Vehicles *Brian Bush; National Renewable Energy Laboratory* | 2.7 | X | | | According to reviewers, the Scenario Evaluation and Regionalization Analysis model has been developed and successfully integrates other data analysis tools. Specific project strengths highlighted by reviewers include that the model maintains rigorous consistency scenario parameters with Hydrogen Analysis (H2A) assumptions. However, reviewers felt that more coordination with industry stakeholders and inclusion of a more robust scenario analysis was needed. It was suggested that the model be used to 1) examine a range of vehicle rollout scenarios from highly optimistic to pessimistic and 2) compare fuel cell electric vehicle (FCEV) and battery electric vehicle (BEV) penetration scenarios. |
| AN-012 | GREET Model Development and Life-Cycle Analysis Applications *Michael Wang; Argonne National Laboratory* | 3.8 | X | | | Reviewers continue to commend this project for the excellent ongoing progress and relevance it is demonstrating and for the inclusion of new analysis and additional case studies. Reviewers thought this project had significant strengths of providing life cycle analysis capabilities, adding shale and biogas analysis capabilities, and operating as the "gold standard" for greenhouse gas emissions calculations. Reviewers suggested that greater visibility to the underlying assumptions and data is needed. |
| AN-020 | Hydrogen Refueling Infrastructure Cost Analysis *Marc Melaina; National Renewable Energy Laboratory* | 3.0 | | | X | Reviewers commented that the project has made expected progress. Reviewers felt that the project's strength stems from the field of resources and collaboration, and from the concept of using four station types to assess how fueling stations might be implemented in the future. However, the project suffered from a small field of respondents and the results of hydrogen production/delivery approaches are aggregated together. Reviewers suggested similar data should be obtained for each type of hydrogen station to quantify and substantiate the results and findings. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
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| AN-021 | Comparing Infrastructure Costs for Hydrogen and Electricity<br>*Marc Melaina;*<br>*National Renewable Energy Laboratory* | 3.0 | | | X | The reviewers noted that a considerable amount of work was accomplished with a good team. Reviewers commented that the analysis project is very relevant to the U.S. Department of Energy's Hydrogen and Fuel Cells Program objectives of reducing petroleum use and that some of the project's assumptions need to be refined and validated. The collaboration used in the project was excellent but should be expanded to a wider array of organizations. However, the project should be expanded and calibrated with other major studies that have been completed. |
| AN-022 | Infrastructure Costs Associated with Central Hydrogen Production from Biomass and Coal<br>*Darlene Steward;*<br>*National Renewable Energy Laboratory* | 3.3 | | | X | Reviewers commented that this analysis project was very relevant to the Program's goals and objectives and made good progress. They commended the project for the use of geographic data in conjunction with cost analysis for hydrogen production from coal and biomass. The collaboration used in the project was excellent but should be expanded to a wider array of organizations. The project could be expanded to compare to present energy distribution systems, including the present gasoline delivery system. |
| AN-023 | H2-Vehicles Market Prospect, Cost, and Social Benefit<br>*David Greene;*<br>*Oak Ridge National Laboratory* | 3.2 | X | | | Reviewers observed that this project has showed the beneficial effect of subsidies and compared fuel cell vehicles to other technologies. They commended the project for the comprehensive sensitivity analysis toward implementation of hydrogen for vehicular applications. The project could be strengthened by including additional input from original equipment manufacturers (OEMs) and adding subsidies for electrical infrastructure. Reviewers suggested performing future studies recognize impact of compliance to U.S. regulatory standards such as CAFE. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| AN-024 | Issues Affecting Hydrogen Pathway Succession *Mark Ruth; National Renewable Energy Laboratory* | 2.8 | | | X | Reviewers observed that this project had an excellent choice of comparison technologies. They commended the project for the comprehensive sensitivity analysis toward implementation of hydrogen for vehicular applications. The project could be strengthened by adding the impact of technology developments and future technologies. The project score was low because the project is complete and no future work is planned. |
| AN-025 | Impact of Program Targets on Vehicle Penetration and Benefits *Zhenhong Lin; Oak Ridge National Laboratory* | 3.0 | | | X | Reviewers observed the project approach was sound but was limited by the available funding. They commended the project for the extensive collaboration. The project strength was the focus on the “what if” scenarios and impact on DOE goals. The project could be strengthened by including additional input from OEMs and a wider array of stakeholders. Reviewers suggested future studies recognize impact of compliance to U.S. regulatory standards such as CAFE. |
| AN-026 | Resource Analysis for Hydrogen Production *Marc Melaina; National Renewable Energy Laboratory* | 3.1 | | | X | Reviewers observed that this analysis project demonstrated good progress and approaches in addressing the resources required to produce hydrogen to support 100 million FCEVs. They commended the project for presenting an easy-to-understand analysis of the impact of renewable hydrogen production on resource supply. Reviewers suggested future analysis includes industry input and regional impact assessments. |
| AN-027 | Cost, Energy Use, and Emissions of Combined Hydrogen, Heat, and Power Tri-Generation Systems *Mark Ruth; National Renewable Energy Laboratory* | 2.9 | | | X | Reviewers believed that this project was a good application of the fuel cell power model. It was suggested that the project add more industrial input and the analysis assumptions be strengthened. Combined, heat, hydrogen, and power (CHHP) should remain in the portfolio as a possibility for a system approach, but other techniques for optimizing heat, hydrogen, and electricity balance should be addressed. |

| Project Number* | Project Title *Principal Investigator Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| AN-029 | Employment Impacts of Early Markets for Hydrogen and Fuel Cell Technologies *Marianne Mintz; Argonne National Laboratory* | 3.6 | X | | | Reviewers agreed with the purpose of the model, recognizing that understanding economic impact will be critical to advancing industry deployment. They identified the need to get this model to a broad audience. Reviewers acknowledged the impressive list of stakeholders and collaborators for the project. They recommended that future work should validate the economic impacts of individual installations of products for end-users and OEMs. They also noted that the model should be expanded to include FCEV fueling infrastructure. |

* Permanent consecutive project numbers were assigned for the 2010 AMR to make administration of the AMR more efficient. As projects are discontinued, the corresponding project number is retired. As new projects are added, new consecutive numbers are added. As a result, there are gaps in the consecutive project numbers. In addition, some projects were not selected for review at the 2012 AMR and are not shown in the Prologue Table (see Appendix D for the full list of projects not reviewed).

## American Recovery and Reinvestment Act

| Project Number* | Project Title *PI Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| H2RA-002 | Solid Oxide Fuel Cell Diesel Auxiliary Power Unit Demonstration *Dan Hennessy; Delphi Automotive* | 2.9 | X | | | According to reviewers, the project is on a clear path to commercialization and is addressing a key market for fuel cells—Class 8 sleeper trucks. Reviewers praised Delphi's commitment to the project and their involvement as an end user. However, the reviewers felt the desulfurization work and corresponding schedule delays were a major weakness. It was recommended that Delphi consider additional units for deployment beyond the single unit and look to other markets where auxiliary power units could be applicable. |
| H2RA-003 | Highly Efficient, 5-kW CHP Fuel Cells Demonstrating Durability and Economic Value in Residential and Light Commercial Applications *Donald Rohr; Plug Power Inc.* | 2.1 | X | | | Reviewers observed that this project has a huge potential for energy reduction at end-user sites (e.g., homes), but were concerned with the significant technical issues (e.g., membrane electrode assembly failures) and corresponding schedule delays. The reviewers felt there was valuable data from the modeling work and that the project team made a good decision to transition the remaining deployment duties to another fuel cell manufacturer. It was recommended that the project team show a cost/benefit analysis for this particular market. |
| H2RA-007 | Accelerating Acceptance of Fuel Cell Backup Power Systems *Donald Rohr; Plug Power Inc.* | 2.6 | X | | | Reviewers observed that transitioning some of the project responsibilities to another fuel cell manufacturer was a good decision, but are concerned with slow technical progress. The reviewers are uncertain about any commercialization prospects and recommended investigating system reliability. It was also recommended that the project team analyze the business case for this particular market. |

| Project Number* | Project Title *PI Name & Organization* | Final Score | Continue | Discontinue/ Further Review | Completed or Congressionally Directed Project | Summary Comments |
|---|---|---|---|---|---|---|
| H2RA-012 | Use of 72-Hour Hydrogen PEM Fuel Cell Systems to Support Emergency Communications *Kevin Kenny; Sprint* | 3.7 | X | | | Reviewers observed that this project has done a great job of methodically selecting sites and deploying large numbers of fuel cells and continues to make significant progress. The reviewers felt there was strong industry support, and the project has done well to establish a diverse set of collaborators. It was recommended that the project team look into reformer-based technologies to expand their potential deployment-site pool. |
| H2RA-013 | Analysis Results for ARRA Projects: Enabling Fuel Cell Market Transformation *Jennifer Kurtz; National Renewable Energy Laboratory* | 3.7 | X | | | Reviewers indicated that this project provides extremely valuable information from a variety of collaborators and fuel cell deployment sites. The reviewers felt this effort should continue, even as other Recovery Act projects come to completion. It was recommended that the project team collect data on internal combustion engine and battery applications in addition to the fuel cell data collection. The reviewers also recommended more effort in sharing the results with industry. |

* Permanent consecutive project numbers were assigned for the 2010 AMR to make administration of the AMR more efficient. As projects are discontinued, the corresponding project number is retired. As new projects are added, new consecutive numbers are added. As a result, there are gaps in the consecutive project numbers. In addition, some projects were not selected for review at the 2012 AMR and are not shown in the Prologue Table (see Appendix D for the full list of projects not reviewed).

# Table of Contents

Prologue Letter ........ iii

Prologue Table ........ iv

Introduction ........ 1

Hydrogen Production and Delivery ........ 9

PD-002: Biomass-Derived Liquids Distributed (Aqueous Phase) Reforming ........ 12
PD-004: Distributed Bio-Oil Reforming ........ 16
PD-013: Electrolyzer Development for the Cu-Cl Thermochemical Cycle ........ 20
PD-014: Hydrogen Delivery Infrastructure Analysis ........ 23
PD-016: Oil-Free Centrifugal Hydrogen Compression Technology Demonstration ........ 26
PD-017: Development of a Centrifugal Hydrogen Pipeline Gas Compressor ........ 31
PD-021: Development of High Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery ........ 36
PD-022: Fiber Reinforced Composite Pipelines ........ 40
PD-024: Composite Pipeline Technology for Hydrogen Delivery ........ 43
PD-025: Hydrogen Embrittlement of Structural Steels ........ 47
PD-027: Solar High-Temperature Water Splitting Cycle with Quantum Boost ........ 50
PD-028: Solar-Thermal ALD Ferrite-Based Water Splitting Cycles ........ 53
PD-029: High-Capacity, High Pressure Electrolysis System with Renewable Power Sources ........ 56
PD-030: PEM Electrolyzer Incorporating an Advanced Low Cost Membrane ........ 59
PD-031: Renewable Electrolysis Integrated System Development and Testing ........ 62
PD-033: Directed Nano-scale and Macro-scale Architectures for Semiconductor Absorbers and Transparent Conducting Substrates for Photoelectrochemical Water Splitting ........ 65
PD-035: Semiconductor Materials for Photoelectrolysis ........ 69
PD-036: Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures ........ 73
PD-037: Biological Systems for Hydrogen Photoproduction ........ 77
PD-038: Fermentation and Electrohydrogenic Approaches to Hydrogen Production ........ 80
PD-039: Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacterial System ........ 83
PD-048: Electrochemical Hydrogen Compressor ........ 87
PD-053: Photoelectrochemical Hydrogen Production ........ 90
PD-065: Unitized Design for Home Refueling Appliance for Hydrogen Generation to 5,000 psi ........ 93
PD-067: Hydrogen by Wire - Home Fueling System ........ 96
PD-070: One Step Biomass Gas Reforming-Shift Separation Membrane Reactor ........ 99
PD-071: High Performance, Low Cost Hydrogen Generation from Renewable Energy ........ 103

PD-072: Development of Hydrogen Selective Membranes/Modules as Reactors/Separators for Distributed Hydrogen Production ........ 106
PD-079: Novel Photocatalytic Metal Oxides ........ 109
PD-081: Solar Hydrogen Production with a Metal Oxide Based Thermochemical Cycle ........ 112
PD-088: Vessel Design and Fabrication Technology for Stationary High-Pressure Hydrogen Storage ........ 116
PD-091: Bio-Fueled Solid Oxide Fuel Cells ........ 120

**Hydrogen Storage ........ 123**

ST-001: System Level Analysis of Hydrogen Storage Options ........ 125
ST-004: Hydrogen Storage Engineering Center of Excellence ........ 129
ST-005: Systems Engineering of Chemical Hydride, Pressure Vessel, and Balance of Plant for On-Board Hydrogen Storage ........ 134
ST-006: Advancement of Systems Designs and Key Engineering Technologies for Materials Based Hydrogen Storage ........ 138
ST-007: Chemical Hydride Rate Modeling, Validation, and System Demonstration ........ 142
ST-008: System Design, Analysis, Modeling, and Media Engineering Properties for Hydrogen Energy Storage ........ 146
ST-009: Thermal Management of On-Board Cryogenic Hydrogen Storage Systems ........ 149
ST-010: Ford/BASF-SE/UM Activities in Support of the Hydrogen Storage Engineering Center of Excellence ........ 152
ST-014: Hydrogen Sorbent Measurement Qualification and Characterization ........ 156
ST-019: Multiply Surface-Functionalized Nanoporous Carbon for Vehicular Hydrogen Storage ........ 159
ST-021: Weak Chemisorption Validation ........ 163
ST-022: A Joint Theory and Experimental Project in the Synthesis and Testing of Porous COFs for On-Board Vehicular Hydrogen Storage ........ 167
ST-023: New Carbon-Based Porous Materials with Increased Heats of Adsorption for Hydrogen Storage ........ 170
ST-024: Hydrogen Trapping through Designer Hydrogen Spillover Molecules with Reversible Temperature and Pressure-Induced Switching ........ 174
ST-028: Design of Novel Multi-Component Metal Hydride-Based Mixtures for Hydrogen Storage ........ 178
ST-040: Liquid Hydrogen Storage Materials ........ 183
ST-044: SRNL Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Metal Hydride and Adsorbent Systems ........ 187
ST-045: Key Technologies, Thermal Management, and Prototype Testing for Advanced Solid-State Hydrogen Storage Systems ........ 190
ST-046: Microscale Enhancement of Heat and Mass Transfer for Hydrogen Energy Storage ........ 194
ST-047: Development of Improved Composite Pressure Vessels for Hydrogen Storage ........ 197
ST-048: Hydrogen Storage Materials for Fuel Cell Powered Vehicles ........ 201
ST-053: Lifecycle Verification of Polymeric Storage Liners ........ 205
ST-093: Melt Processable PAN Precursor for High Strength, Low-Cost Carbon Fibers ........ 208

ST-098: Development of a Practical Hydrogen Storage System based on Liquid Organic Hydrogen Carriers and a Homogeneous Catalyst ... 211

ST-099: Development of Low-Cost, High Strength Commercial Textile Precursor (PAN-MA) ... 216

**Fuel Cells ... 219**

FC-006: Durable Catalysts for Fuel Cell Protection During Transient Conditions ... 222

FC-007: Extended, Continuous Pt Nanostructures in Thick, Dispersed Electrodes ... 225

FC-008: Nanosegregated Cathode Catalysts with Ultra-Low Platinum Loading ... 230

FC-009: Contiguous Platinum Monolayer Oxygen Reduction Electrocatalysts on High-Stability-Low-Cost Supports ... 234

FC-010: The Science and Engineering of Durable Ultralow PGM Catalysts ... 239

FC-012: Polymer Electrolyte Fuel Cell Lifetime Limitations: The Role of Electrocatalyst Degradation ... 243

FC-013: Durability Improvements Through Degradation Mechanism Studies ... 248

FC-014: Durability of Low Pt Fuel Cells Operating at High Power Density ... 252

FC-016: Accelerated Testing Validation ... 256

FC-017: Fuel Cells Systems Analysis ... 260

FC-018: Manufacturing Cost Analysis of Fuel Cell Systems and Transportation Fuel Cell System Cost Assessment ... 265

FC-020: Characterization of Fuel Cell Materials ... 268

FC-021: Neutron Imaging Study of the Water Transport in Operating Fuel Cells ... 271

FC-026: Fuel-Cell Fundamentals at Low and Subzero Temperatures ... 274

FC-028: Transport Studies Enabling Efficiency Optimization of Cost-Competitive Fuel Cell Stacks ... 278

FC-032: Development of a Low Cost 3-10kW Tubular SOFC Power System ... 281

FC-036: Dimensionally Stable High Performance Membranes ... 284

FC-044: Engineered Nano-scale Ceramic Supports for PEM Fuel Cells ... 288

FC-048: Effect of System Contaminants on PEM Fuel Cell Performance and Durability ... 293

FC-049: Development of Micro-Structural Mitigation Strategies for PEM Fuel Cells: Morphological Simulations and Experimental Approaches ... 296

FC-052: Technical Assistance to Developers ... 299

FC-054: Transport in PEMFC Stacks ... 303

FC-063: Novel Materials for High Efficiency Direct Methanol Fuel Cells ... 307

FC-064: New MEA Materials for Improved DMFC Performance, Durability, and Cost ... 312

FC-065: The Effect of Airborne Contaminants on Fuel Cell Performance and Durability ... 316

FC-067: Materials and Modules for Low-Cost, High Performance Fuel Cell Humidifiers ... 320

FC-070: Development of Kilowatt-Scale Coal Fuel Cell Technology ... 323

FC-072: Extended Durability Testing of an External Fuel Processor for SOFC ... 326

FC-076: Biomass Fuel Cell Systems ... 329

FC-077: Fuel Cell Coolant Optimization and Scale-up (plus work under SBIR III project)................... 333

FC-078: 21st Century Renewable Fuels, Energy, and Materials Initiative ......................................... 336

FC-079: Improving Fuel Cell Durability and Reliability ........................................................................ 340

FC-081: Fuel Cell Technology Status—Voltage Degradation ............................................................ 344

FC-083: Enlarging Potential National Penetration for Stationary Fuel Cells Through System Design Optimization........................................................................................................ 348

FC-084: WO3 and HPA Based Systems for Durable Platinum Catalysts in PEMFC Cathodes .......... 352

FC-085: Synthesis and Characterization of Mixed-Conducting Corrosion Resistant Oxide Supports ............................................................................................................................ 357

FC-086: Development of Novel Non Pt Group Metal Electrocatalysts for Proton Exchange Membrane Fuel Cell Applications…. ...................................................................... 362

FC-087: High-Activity Dealloyed Catalysts ......................................................................................... 366

FC-088: Development of Ultra-Low Platinum Alloy Cathode Catalyst for PEM Fuel Cells .................. 370

FC-089: Analysis of Durability of MEAs in Automotive PEMFC Applications....................................... 374

FC-090: Corrugated Membrane Fuel Cell Structures.......................................................................... 377

FC-091: Advanced Materials and Concepts for Portable Power Fuel Cells........................................ 381

FC-092: Investigation of Micro- and Macro-Scale Transport Processes for Improved Fuel Cell Performance ...................................................................................................................... 385

FC-096: Power Generation from an Integrated Biomass Reformer and Solid Oxide Fuel Cell (SBIR Phase III)................................................................................................................... 388

FC-101: PEM Stationary Power Plant ................................................................................................. 392

FC-102: New High Performance Water Vapor Membranes To Improve Fuel Cell Balance of Plant Efficiency and Lower Costs…...................................................................................... 395

**Manufacturing R&D....................................................................................................................... 399**

MN-001: Fuel Cell MEA Manufacturing R&D ....................................................................................... 401

MN-004: Manufacturing of Low-Cost, Durable Membrane Electrode Assemblies Engineered for Rapid Conditioning…........................................................................................................ 405

MN-005: Adaptive Process Controls and Ultrasonics for High Temperature PEM MEA Manufacture ................................................................................................................... 409

MN-006: Metrology for Fuel Cell Manufacturing.................................................................................. 413

MN-007: High Speed, Low Cost Fabrication of Gas Diffusion Electrodes for Membrane Electrode Assemblies ......................................................................................................... 416

MN-008: Development of Advanced Manufacturing Technologies for Low Cost Hydrogen Storage Vessels.................................................................................................................. 419

**Technology Validation.................................................................................................................. 423**

TV-001: Controlled Hydrogen Fleet and Infrastructure Analysis ....................................................... 425

TV-008: Technology Validation: Fuel Cell Bus Evaluations ............................................................... 428

TV-012: Florida Hydrogen Initiative (FHI)........................................................................................... 432

TV-015: Wind to Hydrogen ........ 436

**Safety, Codes and Standards ........ 441**

SCS-001: National Codes and Standards Coordination ........ 443

SCS-002: Component Standard Research & Development ........ 447

SCS-004: Hydrogen Safety, Codes and Standards: Sensors ........ 451

SCS-005: R&D for Safety, Codes and Standards: Materials and Components Compatibility ........ 456

SCS-006: Hydrogen Safety Knowledge Tools ........ 460

SCS-007: Hydrogen Fuel Quality ........ 465

SCS-008: Hydrogen Safety Panel ........ 469

SCS-010: R&D for Safety, Codes and Standards: Hydrogen Behavior ........ 473

SCS-011: R&D for Safety, Codes and Standards: Risk Assessments ........ 477

SCS-015: Hydrogen Emergency Response Training for First Responders ........ 481

**Education ........ 487**

ED-010: Development of Hydrogen Education Programs for Government Officials ........ 489

ED-013: Raising H2 and Fuel Cell Awareness in Ohio ........ 493

**Market Transformation ........ 497**

MT-004: Direct Methanol Fuel Cell Material Handling Equipment Deployment ........ 499

MT-006: Fuel Cell Combined Heat and Power Industrial Demonstration ........ 502

MT-007: Landfill Gas–to–Hydrogen ........ 505

MT-008: Hydrogen Energy Systems as a Grid Management Tool ........ 508

**Systems Analysis ........ 513**

AN-001: Infrastructure Analysis of Early Market Transition of Fuel Cell Vehicles ........ 515

AN-012: GREET Model Development and Life-Cycle Analysis Applications ........ 519

AN-020: Hydrogen Refueling Infrastructure Cost Analysis ........ 522

AN-021: Comparing Infrastructure Costs for Hydrogen and Electricity ........ 525

AN-022: Infrastructure Costs Associated with Central Hydrogen Production from Biomass and Coal ........ 528

AN-023: H2-Vehicles Market Prospect, Cost, and Social Benefit ........ 531

AN-024: Issues Affecting Hydrogen Pathway Succession ........ 534

AN-025: Impact of Program Targets on Vehicle Penetration and Benefits ........ 536

AN-026: Resource Analysis for Hydrogen Production ........ 539

AN-027: Cost, Energy Use, and Emissions of Combined Hydrogen, Heat, and Power Tri-Generation Systems ........ 542

AN-029: Employment Impacts of Early Markets for Hydrogen and Fuel Cell Technologies ........ 545

**American Recovery and Reinvestment Act ..... 549**

H2RA-002: Solid Oxide Fuel Cell Diesel Auxiliary Power Unit Demonstration ..... 551

H2RA-003: Highly Efficient, 5-kW CHP Fuel Cells Demonstrating Durability and Economic Value in Residential and Light Commercial Applications ..... 556

H2RA-007: Accelerating Acceptance of Fuel Cell Backup Power Systems ..... 559

H2RA-012: Use of 72-Hour Hydrogen PEM Fuel Cell Systems to Support Emergency Communications ..... 562

H2RA-013: Analysis Results for ARRA Projects: Enabling Fuel Cell Market Transformation ..... 565

**Appendix A: Attendee List ..... 569**

**Appendix B: Sub-Program Comments ..... 603**

**Appendix C: Evaluation Forms ..... 631**

**Appendix D: Projects Not Reviewed ..... 637**

**Appendix E: Survey Results ..... 643**

# Introduction

The fiscal year (FY) 2012 U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program and Vehicle Technologies Program Annual Merit Review and Peer Evaluation Meeting (AMR) was held from May 14–18, 2012, at the Crystal City Marriott and Crystal Gateway Marriott in Arlington, Virginia. This report is a summary of comments by AMR peer reviewers on the hydrogen and fuel cell projects funded by DOE's Office of Energy Efficiency and Renewable Energy (EERE) and the hydrogen production projects funded by the Office of Fossil Energy. DOE uses the results of this merit review and peer evaluation, along with additional review processes, to make funding decisions for upcoming FYs.

The objectives of this meeting include the following:

- Review and evaluate FY 2012 accomplishments and FY 2013 plans for DOE laboratory programs; industry/university cooperative agreements; and related research, development, and demonstration (RD&D) efforts
- Provide an opportunity for program stakeholders and participants (e.g., fuel cell manufacturers, component developers, and others) to provide input to help shape the DOE-sponsored RD&D program in order to address the highest priority technical barriers and facilitate technology transfer
- Foster interactions among the national laboratories, industry, and universities conducting RD&D

The peer review process followed the guidelines of the *Peer Review Guide* developed by EERE. The peer review panel members, listed in Table 1, provided comments on 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 | Ayers, Katherine | Proton OnSite |
| 2 | Barbier, Francoise | Air Liquide |
| 3 | Baturina, Olga | U.S. Naval Research Laboratory (former) |
| 4 | Beattie, Paul | Ballard Power Systems, Inc. |
| 5 | Benard, Pierre | Universite du Quebec a Trois-Rivieres |
| 6 | Bender, Guido | National Renewable Energy Laboratory |
| 7 | Benjamin, Thomas | Argonne National Laboratory |
| 8 | Bennett, Kristin | KB Science LLC |
| 9 | Birdsall, Jackie | California Fuel Cell Partnership |
| 10 | Blair, Larry | Consultant, U.S. Department of Energy |
| 11 | Blanchet, Scott | Nuvera Fuel Cells, Inc. |
| 12 | Borup, Rod | Los Alamos National Laboratory |
| 13 | Bouwkamp, Nico | California Fuel Cell Partnership |
| 14 | Bowden, Mark | Pacific Northwest National Laboratory |
| 15 | Bowman, Robert | Oak Ridge National Laboratory |
| 16 | Boyd, Robert | Boyd Hydrogen, LLC |
| 17 | Brosha, Eric | Los Alamos National Laboratory |
| 18 | Brown, Craig | National Institute of Standards and Technology |
| 19 | Buchner, John | University of Maryland, College Park |
| 20 | Burgunder, Albert | Praxair, Inc. |
| 21 | Cai, Mei | General Motors, Research and Development Center |
| 22 | Cairns, Julie | CSA Group |
| 23 | Campbell, Stephen | Automotive Fuel Cell Cooperation |
| 24 | Carlstrom, Chuck | H2 Pump LLC |
| 25 | Chahine, Richard | Hydrogen Research Institute |
| 26 | Choudhury, Biswajit | DuPont Fuel Cells |
| 27 | Christensen, John | Consultant, U.S. Department of Energy/National Renewable Energy Laboratory |

| No. | Name | Organization |
|---|---|---|
| 28 | Cole, Brian | U.S. Army Night Vision Laboratory |
| 29 | Cole, James Vernon | CFD Research Corporation |
| 30 | Collins, William | UTC Power |
| 31 | Conti, Amedeo | Nuvera Fuel Cells, Inc. |
| 32 | Cox, Phillip | University of North Florida |
| 33 | Curry-Nkansah, Maria | Imago Energy LLC |
| 34 | Davis, Benjamin | Los Alamos National Laboratory |
| 35 | De Castro, Emory | BASF Fuel Cell, Inc. |
| 36 | Debe, Mark | 3M |
| 37 | Dedrick, Daniel | Sandia National Laboratories |
| 38 | DelPlancke, Jean-Luc | European Commission, Fuel Cells and Hydrogen Joint Undertaking |
| 39 | Dinh, Huyen | National Renewable Energy Laboratory |
| 40 | Dixon, David | The University of Alabama |
| 41 | Dross, Robert | Nuvera Fuel Cells, Inc. |
| 42 | Ehlers, Peter | CSA Group |
| 43 | Eisman, Glenn | H2Pump LLC |
| 44 | Elrick, William | California Fuel Cell Partnership |
| 45 | Erdle, Erich | Erdle Fuel Cell & Energy Consulting |
| 46 | Ernst, William | EnerSys Innovation |
| 47 | Ewan, Mitch | Hawaii Natural Energy Institute (HNEI) |
| 48 | Fan, Chinbay | Gas Technology Institute |
| 49 | Felter, Tom | Sandia National Laboratories |
| 50 | Fenske, George | Argonne National Laboratory |
| 51 | Fisher, Allison | Energizer Battery-Specialty Power |
| 52 | Fletcher, James | University of North Florida |
| 53 | Funk, Stuart | LMI |
| 54 | Gangi, Jennifer | Breakthrough Technologies Institute |
| 55 | Garland, Roxanne | DOE (retired) |
| 56 | Garzon, Fernando | Los Alamos National Laboratory |
| 57 | Gennett, Thomas | National Renewable Energy Laboratory |
| 58 | Gervasio, Don | University of Arizona |
| 59 | Gittleman, Craig | General Motors Corporation |
| 60 | Glass, Robert | Lawrence Livermore National Laboratory |
| 61 | Graetz, Jason | Brookhaven National Laboratory |
| 62 | Grassilli, Leo | Consultant, Office of Naval Research |
| 63 | Gross, Karl | H2 Technology Consulting, LLC |
| 64 | Gross, Tom | Electricore |
| 65 | Gupta, Ram | National Science Foundation |
| 66 | Hamilton, Jennifer | California Fuel Cell Partnership |
| 67 | Hardis, Jonathan | National Institute of Standards and Technology |
| 68 | Harris, Aaron | Sandia National Laboratories |
| 69 | Haugen, Greg | 3M |
| 70 | Hays, Charles | California Institute of Technology |
| 71 | Hennessey, Barbara | U.S. Department of Transportation |
| 72 | Herbert, Thorsten | NOW GmbH |
| 73 | Herring, Andy | Colorado School of Mines |
| 74 | Hershkowitz, Frank | ExxonMobil, Research and Engineering Company |
| 75 | Hirano, Shinichi | Ford Motor Company |
| 76 | Holladay, Jamie | Pacific Northwest National Laboratory |
| 77 | Imam, Ashraf | U.S. Navy, Naval Research Laboratory |
| 78 | James, Brian | Strategic Analysis Inc. |
| 79 | James, Charles (Will) | Savannah River National Laboratory |
| 80 | Jarvi, Tom | Sun Catalytix Corp |
| 81 | Jensen, Craig | University of Hawaii at Manoa |
| 82 | Jorgensen, Scott | General Motors, Research and Development Center |
| 83 | Josefik, Nicholas | U.S. Army Corps of Engineers |
| 84 | Kasab, John | Ricardo |

| No. | Name | Organization |
|---|---|---|
| 85 | Keller, Jay | Sandia National Laboratories, retired/SRA International |
| 86 | Kerr, John | Lawrence Berkeley National Laboratory |
| 87 | Kienitz, Brian | W.L. Gore & Associates, Inc. |
| 88 | King, David | Pacific Northwest National Laboratory |
| 89 | Knights, Shanna | Ballard Power Systems |
| 90 | Kocha, Shyam | National Renewable Energy Laboratory |
| 91 | Kopasz, John | Argonne National Laboratory |
| 92 | Krause, Theodore | Argonne National Laboratory |
| 93 | Kumar, Romesh | Argonne National Laboratory |
| 94 | Kunze, Klaas | BMW AG |
| 95 | Kurtz, Jennifer | National Renewable Energy Laboratory |
| 96 | Lakshmanan, Balsu | General Motors Corporation |
| 97 | Lear, William | University of Florida |
| 98 | Lewis, Michele | Consultant |
| 99 | Lieberman, Robert | Intelligent Optical Systems |
| 100 | Linkous, Clovis | Youngstown State University |
| 101 | Lipp, Ludwig | FuelCell Energy, Inc. |
| 102 | Madden, Tom | Sun Catalytix |
| 103 | Maes, Miguel | National Aeronautics and Space Administration |
| 104 | Markovic, Nenad | Argonne National Laboratory |
| 105 | Maroni, Victor | Argonne National Laboratory |
| 106 | McLean, Gail | U.S. Department of Energy, Office of Science |
| 107 | McWhorter, Scott | U.S. Department of Energy |
| 108 | Medeiros, Maria | U.S. Navy, Office of Naval Research |
| 109 | Melis, Tasios | University of California, Berkeley |
| 110 | Mergel, Jürgen | Forschungszentrum Jülich GmbH |
| 111 | Merritt, James | U.S. Department of Transportation |
| 112 | Miller, James | Argonne National Laboratory |
| 113 | Minh, Nguyen | Center for Energy Research, University of California, San Diego |
| 114 | Mittelsteadt, Cortney | Giner Electrochemical Systems, LLC |
| 115 | Mohtadi, Rana | Toyota Research Institute of North America |
| 116 | More, Karren | Oak Ridge National Laboratory |
| 117 | Moreland, Gregory | SRA International, Inc. |
| 118 | Morgan, Jason | Ballard Material Products |
| 119 | Mountz, David | Arkema, Inc. |
| 120 | Mukerjee, Sanjeev | Northeastern University |
| 121 | Mukundan, Rangachary | Los Alamos National Laboratory |
| 122 | Myers, Deborah | Argonne National Laboratory |
| 123 | Ohi, Jim | H2O-E |
| 124 | Ohma, Atsushi | Nissan Motor Company |
| 125 | Olson, Gregory | SRA International |
| 126 | Ott, Kevin | Los Alamos National Laboratory |
| 127 | Owejan, Jon | GM Electrochemical Energy Research Laboratory |
| 128 | Padro, Catherine | Los Alamos National Laboratory |
| 129 | Parks, George | FuelScience LLC |
| 130 | Paster, Mark | Consultant |
| 131 | Penev, Michael | National Renewable Energy Laboratory |
| 132 | Perret, Robert | Nevada Technical Services LLC |
| 133 | Perry, Mike | United Technologies Research Center |
| 134 | Petrovic, John | Los Alamos National Laboratory, retired/Petrovic and Associates |
| 135 | Pietrasz, Patrick | Ford Motor Company |
| 136 | Pintauro, Peter | Vanderbilt University |
| 137 | Pivovar, Bryan | National Renewable Energy Laboratory |
| 138 | Podolski, Walt | Argonne National Laboratory |
| 139 | Ramani, Vijay | Illinois Institute of Technology |
| 140 | Rambach, Glenn | Trulite, Inc. |

| No. | Name | Organization |
|---|---|---|
| 141 | Richards, Mark | Versa Power Systems |
| 142 | Ricker, Rick | National Institute of Standards and Technology |
| 143 | Rinebold, Joel | Connecticut Center for Advanced Technology, Inc. |
| 144 | Roan, Vernon | University of Florida |
| 145 | Roger, Chris | Arkema Inc. |
| 146 | Rossmeissl, Neil | U.S. Department of Energy, Biomass Program |
| 147 | Rufael, Tecle | Chevron Energy Technology Company |
| 148 | Sandrock, Gary | Sandia National Laboratories |
| 149 | Schlasner, Steven | University of North Dakota, Energy & Environmental Research Center |
| 150 | Schneider, Jesse | Consultant |
| 151 | Serfass, Patrick | Technology Transition Corporation |
| 152 | Siegel, Don | University of Michigan, Ann Arbor |
| 153 | Sievers, Robert | Teledyne Energy Systems |
| 154 | Silverman, Linda | U.S. Department of Energy, Education and Workforce Development |
| 155 | Simnick, James | BP America |
| 156 | Simpson, Lin | National Renewable Energy Laboratory |
| 157 | Sofronis, Petros | Consultant |
| 158 | Soto, Herie | Shell |
| 159 | Spendelow, Jacob | Los Alamos National Laboratory |
| 160 | Stanfield, Eric | National Institute of Standards and Technology |
| 161 | Stanic, Vesna | EnerFuel, Inc. |
| 162 | Steele, Eugene | Steele Consulting |
| 163 | Steen, Marc | European Commission, Joint Research Centre |
| 164 | Steenberg, Thomas | Danish Power Systems |
| 165 | Stolten, Detlef | Forschungszentrum Jülich GmbH |
| 166 | Sutherland, Ian | General Motors Corporation |
| 167 | Swider-Lyons, Karen | U.S. Navy, Naval Research Laboratory |
| 168 | Thomas, C.E. (Sandy) | Consultant |
| 169 | Tran, Thanh | U.S. Navy, Naval Surface Warfare Center, Carderock Division |
| 170 | Trocciola, John | FuelCell Perspectives |
| 171 | Ulsh, Michael | National Renewable Energy Laboratory |
| 172 | Vanderborgh, Nicholas | Consultant |
| 173 | Veenstra, Mike | Ford Motor Company |
| 174 | Vernstrom, George | 3M |
| 175 | Wachsman, Eric | University of Maryland |
| 176 | Wagner, Frederick | General Motors Corporation |
| 177 | Wainright, Jesse | Case Western Reserve University |
| 178 | Waldecker, James | Ford Motor Company |
| 179 | Walk, Alex | Consultant |
| 180 | Warner, James | Fuel Cell and Hydrogen Energy Association |
| 181 | Weber, Adam | Lawrence Berkeley National Laboratory |
| 182 | Weil, K. Scott | Pacific Northwest National Laboratory |
| 183 | Wheeler, Douglas | DJW Technology LLC |
| 184 | White, Chris | California Fuel Cell Partnership |
| 185 | Williams, Mark | URS Corporation |
| 186 | Wipke, Keith | National Renewable Energy Laboratory |
| 187 | Wolak, Frank | FuelCell Energy, Inc. |
| 188 | Wolverton, Christopher | Northwestern University |
| 189 | Woods, Stephen | National Aeronautics and Space Administration |
| 190 | Yuzugullu, Elvin | SRA International, Inc. |
| 191 | Zelenay, Piotr | Los Alamos National Laboratory |
| 192 | Zheng, Jinyang | Zhejiang University |
| 193 | Zhu, Yimin | Nanosys, Inc. |

## Summary of Peer Review Panel's Crosscutting Comments and Recommendations

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

## Analysis Methodology

A total of **145** projects were reviewed at the meeting. As shown in Table 1, **193** review panel members participated in the AMR process, providing a total of **853** project evaluations. These reviewers were asked to provide numeric scores (on a scale of 1–4, with 4 being the highest) for five aspects of the work presented. Sample evaluation forms are provided in Appendix C. Scores and comments were submitted using laptops (provided onsite) 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.

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

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

For each project, 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 0.20] + [Score 3 x 0.40] + [Score 4 x 0.10] + [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.

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.

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

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

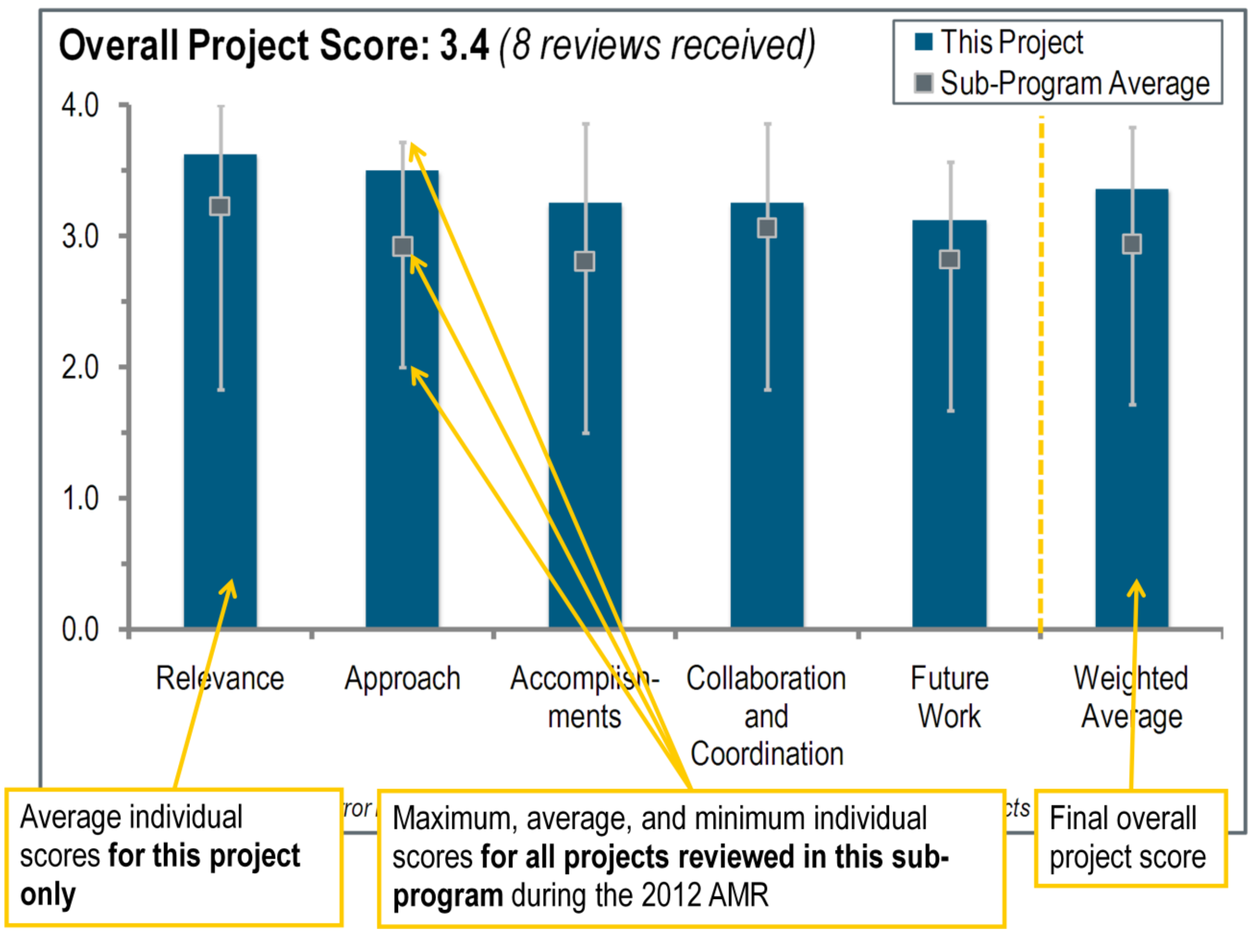
Reviewers were also asked to provide summary comments regarding Recovery Act project strengths and weaknesses and specific recommendations.

## Organization of the Report

The project comments and scores are grouped by sub-program (Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing Research and Development [R&D]; Technology Validation; Safety, Codes and Standards; Education; Systems Analysis; and Recovery Act activities) in order to align with the Program planning scheme. Each of these sections begins with a brief description of the general type of R&D or other activity being conducted. Next are the results of the reviews of each project presented at the 2012 AMR. The report also includes a summary of the qualitative comments for each project, as well as a graph showing the overall project score and a comparison of how each project aligns with all of the other projects in its sub-program area. A sample graph is provided in Figure 1.

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

**Figure 1: Project Score Graph with Explanation**



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

**Table 2: Sample Project Scores**

| | Relevance (20%) | Approach (20%) | Accomplish-ments (40%) | Collaboration and Coordination (10%) | Future Work (10%) |
|---|---|---|---|---|---|
| Project A | 3.4 | 3.3 | 3.3 | 3.2 | 3.1 |
| Project B | 3.1 | 2.8 | 2.7 | 2.7 | 2.9 |
| Project C | 3.0 | 2.6 | 2.7 | 2.8 | 2.9 |
| Project D | 3.4 | 3.5 | 3.4 | 3.2 | 3.3 |
| Project E | 3.6 | 3.7 | 3.5 | 3.4 | 3.4 |
| 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 line bar indicating the related maximum, minimum, and average values for all of the projects in Project A's sub-program area (the last three lines in the table above) would overlay each corresponding bar to facilitate comparison. In addition, each project's criteria scores would be weighted and combined to produce a final, overall project score that would permit meaningful comparisons to other projects. Below is a sample calculation for the Project A weighted score.

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

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# 2012 — Hydrogen Production and Delivery
# Summary of Annual Merit Review of the Hydrogen Production and Delivery Sub-Program

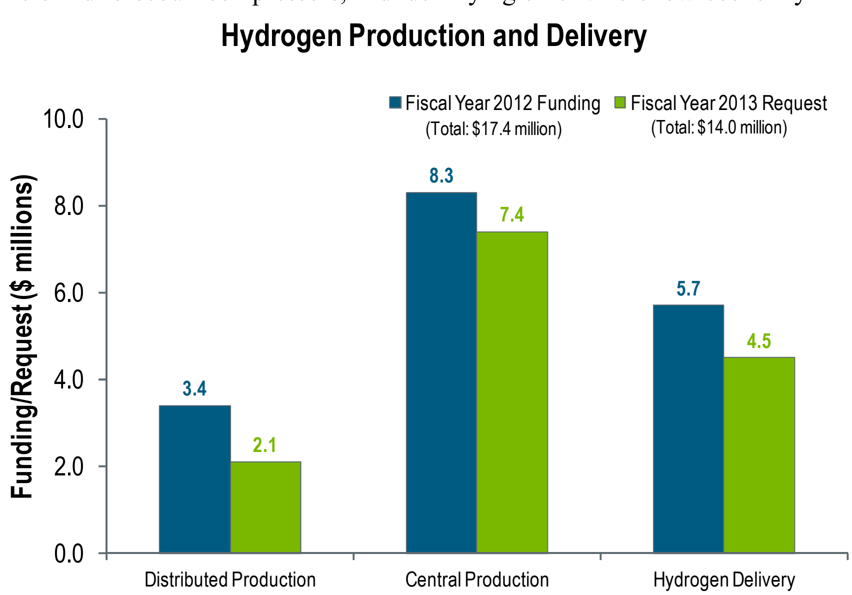
## Summary of Reviewer Comments on the Hydrogen Production and Delivery Sub-Program:

This review session evaluated hydrogen production and delivery research and development (R&D) activities in the U.S. Department of Energy (DOE) Fuel Cell Technologies (FCT) Program in the Office of Energy Efficiency and Renewable Energy. The hydrogen production projects reviewed represented a diverse portfolio of technologies to produce hydrogen from renewable energy sources. Production project sub-categories included bio-derived renewable liquids reforming, water electrolysis, biomass gasification, solar-driven thermochemical cycles, photoelectrochemical (PEC) direct water splitting, biological hydrogen production, and separations technologies. The hydrogen delivery projects reviewed included R&D in advanced composite tube trailer vessels, low-cost pipeline materials, pipeline and forecourt compression, electrochemical compression technology, and delivery cost analyses.

The reviewers recognized the production and delivery projects as well aligned with DOE goals and objectives, and found that these projects, in general, had made substantial progress during the past year. Specific progress was noted in optimized component and reactor designs and in materials fabrication. Reviewers stressed the importance of continued improvement of performance and durability in materials, devices, and systems for renewable hydrogen production pathways, and for pipelines and compressors for hydrogen delivery. They also emphasized the need for continued cost modeling of production and delivery technologies to identify and address cost barriers, for further development of materials characterization protocols and performance metrics for early development technologies, and for expanded data collection to inform codes and standards development.

## Hydrogen Production and Delivery Funding by Technology:

The fiscal year (FY) 2012 appropriation for the Hydrogen Production and Delivery sub-program of the FCT Program was $17.4 million. Funding was distributed approximately 67% to 33% between Hydrogen Production and Hydrogen Delivery, respectively, the same distribution used in FY 2011. Production funding is increasingly focused on early development, long-term, renewable pathways such as PEC, biological, and solar-thermochemical hydrogen production. This trend is expected to continue in FY 2013 with a $14 million budget request. The Delivery portfolio emphasis in FY 2012 and FY 2013 is on reducing near-term technology costs such as those associated with tube trailers and forecourt compressors, and identifying other viable low-cost early market delivery pathways.



## Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the production and delivery projects were above-average to high, scoring in the range of 2.3–3.7, with an average score of 3.1. The scores are indicative of the technical progress that has been made over the past year.

**Bio-Derived Liquids Reforming:** Two projects in bio-derived liquids reforming were reviewed, with an average score of 2.8. Projects in this area addressed hydrogen production through catalytic steam reforming of pyrolysis oil, and aqueous phase reforming of pyrolysis oil at moderate temperatures. In general, the projects reviewed consisted of a technically sound approach, focusing on identification of effective catalysts for use in the reforming process. Reviewers noted that the projects appeared to be well aligned with DOE objectives and demonstrated improved catalyst performance, but lacked a clear path forward to meet cost targets based on feedstock costs and catalyst cost and durability. Reviewers stressed the importance of documenting the results of these investigations and suggested that these approaches be considered for non-transportation energy applications for which higher hydrogen costs may be acceptable.

**Biological Hydrogen Production:** Four projects in biological hydrogen production were reviewed, with an average score of 3.4. Projects in this area encompassed a portfolio of photobiological and fermentative production methods that use various algal, cyanobacterial, and bacterial microorganisms that produce hydrogen through splitting water or fermentation of biomass. Reviewers cited a number of achievements, including the improvement in light collection efficiency of organisms, improved feedstock utilization as an intermediate step toward scaling-up fermentation, and light-induced hydrogen production by a bacterial hydrogenase expressed in algal cells. However, they also expressed concern that there could be difficulty with scaling up the projects to industrial scale. A key recommendation was that collaborations should be sought with experts in related fields and with industry, and that more specific and quantitative targets be identified for intermediate steps in the different projects in this longer term pathway.

**Biomass Gasification:** One project on the development of a one-step biomass gas reforming-shift separation membrane reactor was reviewed and received a final score of 2.3. Reviewers remarked on the slow progress for this project, noted that recent work has focused on modeling, and suggested that experimentation be given more emphasis next year. Reviewers recommended that integrated gasifier tests be started to validate performance and cost estimates, and stressed the need to characterize the status of hydrogen purity, selectivity, and membrane durability.

**Electrolysis:** Four electrolysis projects were reviewed, with an average score of 3.0. The major emphases of these projects were on cost reduction and efficiency improvement through cell and stack optimization, higher pressure operation, and validation of integration with renewable resources. Reviewers noted that the projects reported excellent progress, even those faced with early difficulties, demonstrating a good blend of analysis, design, and experimentation. They also commended the effective collaborations and quality of designs. It was recommended that the projects continue to emphasize meeting future cost and efficiency challenges, and focus on scaling up as well as qualification and manufacturing issues.

**Home Refuelers (Small Business Innovation Research [SBIR]):** Two SBIR projects addressing home refuelers were reviewed, with an average score of 3.0. Projects reviewed in this subsection focused on home refueling applications. Reviewers noted the emphasis placed on codes and standards as well as economic analysis. One project also made significant prototype and testing progress. Reviewers pointed out that there may be some difficulty associated with these projects due to the concerns of the public in having new technology within households. Reviewers suggested that an emphasis be placed on safety, cost, and outside collaboration to address issues related to the implementation of these systems.

**Hydrogen Delivery:** Nine projects in delivery were reviewed, with an average score of 3.3. Projects reviewed in the Delivery sub-program portfolio continued to receive high marks from reviewers for the sound progress made toward the sub-program's cost goals. High-capacity tube trailer vessels, pipeline materials, and pipeline compressors all showed progress toward the targets. Reviewers highlighted the level of expertise in this broad topic area and were impressed with the degree of collaboration within many of the projects. While recommendations for improvements tended to be project-specific, there was a general consensus that future work should focus on cost analysis with industry participation and include scaled testing and validation of the components in hydrogen environments. Reviewers encouraged testing activities to collect necessary data for the creation of codes and standards relevant to the use of the components in hydrogen applications.

**Photoelectrochemical Hydrogen Production:** Four projects in PEC hydrogen production were reviewed, with an average score of 3.2. Reviewers felt that projects in this area were well aligned with DOE objectives, with a broad focus on developing viable PEC material systems and prototypes. They acknowledged notable accomplishments in improved current-voltage characteristics and enhanced durability in the promising materials under investigation. Projects were rated highly for material improvements in catalytic activity, for successful accomplishments in meeting durability milestones, and for effective collaborations with the PEC Working Group. Recommendations for future work included stronger emphasis on identifying the sources of losses in the PEC material systems, continued work on demonstrating extended durability, and continued techno-economic analysis to determine long-term viability of this pathway.

**Separations:** Two projects in separations were reviewed, with an average score of 3.2. The first focused on the development of hydrogen separation membranes for use in a water-gas-shift (WGS) membrane reactor. The second, an SBIR Phase III project, addressed development and demonstration of a biogas clean-up system. For the first project, reviewers commended the progress made in membrane development, but stressed the importance of integrating the WGS unit into the membrane reactor and testing the system in actual synthesis gas streams. For the second project, the reviewers commented on the importance of biogas purification for fuel cell applications. Reviewers commended the good design and promising sorbent material for this technology, but remarked that it was not clear that it represents a major advance over current commercial technology. They suggested that the project team consider additional applications for this technology.

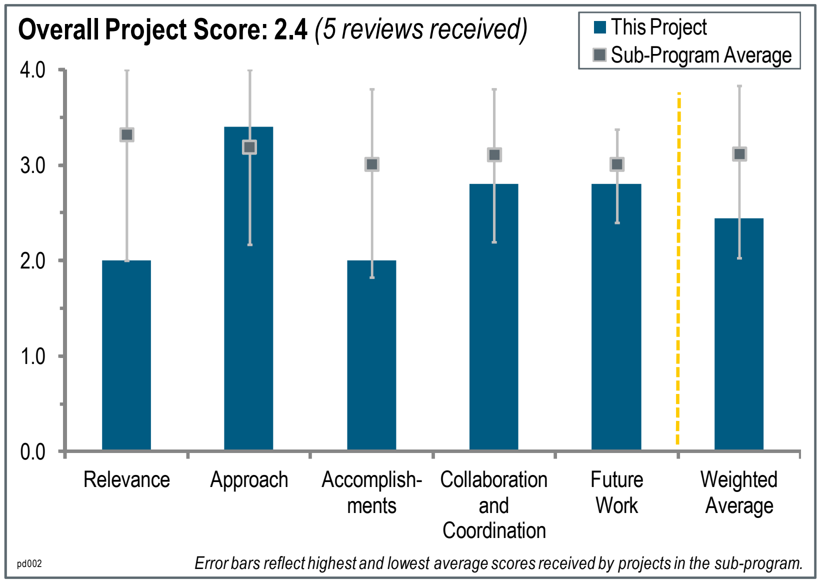
**Solar-Driven High-Temperature Thermochemical Production:** Four projects in solar-driven high-temperature thermochemical hydrogen production were reviewed, with an average score of 2.8. Efforts in these projects were directed toward improving reactor designs, improving voltage and overall efficiency, and addressing membrane crossover issues. There was also an investigation into isothermal reactor operation for a two-step metal oxide cycle. The domestic and international collaborations in these projects were favorably noted. Reviewers recommended longer durability tests and degradation characterization for membranes and reactor materials. They felt that development of specific performance metrics and continued economic analysis are needed in order to frame the value proposition of these hydrogen production cycles. It was suggested that, since water-splitting reaction materials are still being screened, future work should focus on advanced materials research to improve redox capacity and cycle life of reactor materials.

# Project # PD-002: Biomass-Derived Liquids Distributed (Aqueous Phase) Reforming

David King; Pacific Northwest National Laboratory

## Brief Summary of Project:

The primary objective of this project is to develop aqueous-phase reforming (APR) catalysts and technology to convert bio-derived liquids to hydrogen ($H_2$) that meets the 2012 U.S. Department of Energy (DOE) cost target of $3.80/gge, as verified by the Hydrogen Analysis (H2A) model. Specific objectives related to feedstock issues are to: (1) identify primary compounds in bio-oil that are extractable into an aqueous phase, (2) determine the effectiveness of aqueous-phase reforming in producing $H_2$ from these water-soluble compounds, and (3) estimate the cost of $H_2$ production using the best catalytic results as a function of feedstock cost.




## Question 1: Relevance to overall DOE objectives

This project was rated **2.0** for its relevance to DOE objectives.

- The project goal is very reasonable: explore aqueous-phase reforming using renewable bio-oil feedstock as a way to achieve the DOE target of $3/kg of $H_2$.
- This project is involved in downstream processing of the bio-oil (generated from biomass) to produce $H_2$. The concept was that some $H_2$ could be generated from what is essentially a waste product. However, the current cost of bio-oil renders this project a no-go regardless of the technical proficiency of the experimental team.
- The project has little likelihood of being able to support the goals unless the aqueous-phase components of bio-oil can become significantly cheaper and the acetic acid content is dramatically reduced from the stated 7%–10% or mitigated in some way. There are costs shown that are competitive, but they require significant and unrealistic improvements in reaction extent.
- Eight years into the project, there still does not appear to be a lot of advancement in aqueous-phase bio-derived liquid reforming, especially in meeting the DOE production cost targets. The project is relevant in that it is developing a technology for producing $H_2$ using renewable biomass as a feedstock, particularly a feed stream that is considered to be of little value. However, the technology does not fit well into DOE's definition of centralized or distributed production technologies. Given that it targets the aqueous-phase by-products from a biomass pyrolysis project, it is not suitable for distributed forecourt application, and the heating value of the aqueous-phase by-products is much too low for a centralized process. All results presented indicate that it will not hit DOE cost targets even if full conversion of all components could be achieved. There appears to be no pathway for reducing the cost of $H_2$ below that of the cost achieved if full conversion is realized, which will be nearly impossible to achieve.

## Question 2: Approach to performing the work

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

- Attempting to identify catalyst candidates with a high-throughput combinatorial reactor is a very reasonable approach.

- This project is sharply focused on critical barriers. It was determined that the critical barrier of the cost of bio-oil could not be overcome.
- The approach was well tuned to identifying the compounds from a surrogate bio-oil source (pine sawdust), screening for appropriate catalysts, evaluating those catalysts, and using H2A to derive $H_2$ costs. Unfortunately, the data and results showed a very high cost of $H_2$, with no realistic options for cost reduction.
- The approach is to focus on attributes of the aqueous-phase bio-oil and screen reforming catalysts. This reviewer has concerns about the basic focus on the aqueous pyrolysis products. Apparently, 45% of the carbon in the feedstock is a solid and therefore not used in the $H_2$ generation process. This is a huge hit to overall efficiency. This reviewer wants to know what the energy content is of this solid material, and if there is a plan to use it. If not, this reviewer wonders if the economics can survive based only on the liquid portion. Perhaps a system that processes the whole spectrum of pyrolysis products would be better.
- The principal investigator (PI) and team members are technically very strong, and the overall technical approach is strong. The technical approach of using combinatorial approaches for accelerating the optimization of catalyst performance is a significant advantage of this project and appears to have been somewhat successful in identifying "optimal" catalyst compositions. From a catalyst consideration, the PI has identified three major technical barriers for improving catalyst performance: (1) overcoming the catalyst "poisoning" effect of acetic acid, which is present in significant concentrations in the aqueous-phase extract from pyrolysis oil; (2) controlling selectivity between carbon-carbon and carbon-oxygen bond cleavage; and (3) dealing with the inability to reform the light hydrocarbon gases that are produced. Unfortunately, there appears to be no clear path forward at this time for dealing with any of these three major issues. Stability is another major issue that needs to be addressed. Results presented indicate that higher reaction temperatures may be detrimental to catalyst durability, which may limit the ability to deal with the acid poisoning issue.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The technical work is sound. The PI and his team are very knowledgeable. The technique of the aqueous-phase reforming has merit, but the cost of the bio-oil feedstock negated this approach. The presenter rightly assessed the project's viability as poor due to results from his economic analysis using new cost estimates from DOE. The presenter also calculated what the cost of bio-oil should be in order for aqueous-phase reforming to be economically viable.
- There was progress in each task and the accomplishments achieved were well done, but, again, they provide little to no progress toward a goal for the production of $H_2$ from aqueous-phase bio-oil at anything close to the goal.
- Progress toward optimizing catalyst performance has been markedly improved over the past year through the use of combinatorial techniques. There appears to be little progress toward dealing with the major technical barriers affecting catalyst performance—acid poisoning and carbon-oxygen versus carbon-carbon bond cleavage selectivity makes it nearly impossible to see how this technology could meet DOE cost targets.
- The project team has achieved a solid accomplishment in identifying the major components in the aqueous fraction. Determining the approximate fraction of each component would be even more important if, as is suggested, some components are hard or impossible to reform. Combinatorial progress identified platinum (Pt)-containing catalysts as having substantial activity, which is no big surprise. However, work does suggest that Pt-Co and Pt-Zn are promising catalysts. The project demonstrates a well-thought-out approach to using chemical reaction theory to select a promising catalyst. The H2A task was well done and clearly shows that major improvements in $H_2$ conversion and bio-oil cost need to occur to get the $H_2$ cost down to even $5/kg. Bio-oil cost is a problem not because bio-oil is dramatically expensive, but because they need to consume a lot of it due to losing a large fraction of heating value in the pyrolysis step and in the low conversion of $H_2$.

### Question 4: Collaboration and coordination with other institutions

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

- There appears to be good, long-term collaboration with Washington State University (WSU) and Virent.
- The partners are fairly well coordinated.

- There is collaboration with WSU and the batch APR studies at high temperatures to mitigate problems with species such as acetic acid.
- There are very strong collaboration partners, including Virent, which is the leading industrial organization developing aqueous-phase reforming of biomass, and Professor Jingguang Chen and Dion Vlachos at the University of Delaware, which has historically had very strong programs in catalysis. It is difficult to judge the contributions of Brookhaven National Laboratory and the University of Delaware, given that their role in this project focuses on x-ray spectroscopy and there was no discussion of these studies in the presentation. While the role and value of x-ray spectroscopy in these types of studies is well-known to this reviewer, the overall benefit to the project is not obvious. The role of WSU is critical, given that it is tasked with how to deal with acetic acid, which is very problematic for the catalysts evaluated in this system.

### Question 5: Proposed future work

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

- This reviewer would strongly suggest that a go/no-go decision review be made soon on this project.
- The project is wrapping up. Documenting results should be the highest priority. The fiscal year 2013 plan for the examination of a single-step route to $H_2$ from cellulose seems like an outlier research effort begun in the waning days of the project that will receive too little effort, time, and money to achieve any useful results or conclusions.
- The project team is commended for showing, through H2A analysis, that this project is not economically viable, given the cost of bio-oil.
- The future plan is focused on addressing some of the major technical issues such as the acetic acid issue; however, the specific path forward to overcoming these issues is not defined. As such, it is not clear whether significant progress can be made. While extended catalyst testing is a desirable activity, the benefit of these tests is unclear, even if the selected catalyst proves durable over the proposed 50-hour test period, given that the major technical issues previously identified will probably not be resolved before these tests begin. The plan appears to be to terminate the project and summarize the results, which seems to be the most appropriate choice at this time.
- This reviewer thinks the future work plan is reasonable in content, but in reality, it will not bring about an $H_2$ source for transportation. Completing the future work beyond report writing will mostly just increase the shut-down cost of the project. However, as presented in the August 2011 Manufacturing Workshop in Hydrogen Storage Technologies, there are numerous energy applications where it is acceptable for $H_2$ to cost more than $100/kg. It may be worthwhile to consider “diverting” this effort toward “other markets,” similar to the way metal hydrides were diverted.

### Project strengths:

- Documentation of what has not worked to date may help other research projects.
- The methodical investigation of aqueous-phase reforming is an area of strength.
- The team has good analytical and experimental capabilities in component analysis and catalytic studies.
- This project features a strong scientific team. Significant progress has been made toward improving catalyst performance through the implementation of combinatorial techniques. The team is focused on attempting to find value by processing a by-product waste or low-value biomass feedstock.

### Project weaknesses:

- The cost of bio-oil is an area of weakness.
- The tough feedstock choice is an area of weakness.
- There is no clear catalyst that will work effectively to produce a high yield of $H_2$.
- Linkage of experimentally demonstrated bio-oil conversions does not lead to a projected cost of $H_2$ consistent with DOE targets.
- The technology does not seem to fit well into DOE vision of centralized and distributed production facilities. There is no clear pathway to achieving DOE cost targets for $H_2$. There are a number of technical issues related to catalyst performance—acid poisoning, reaction pathway selectivity, dealing with small molecules—that seem extremely difficult to overcome through improved catalyst performance. There might be some pretreatment technologies that could deal with the acid components in the feedstock, but this would introduce

additional costs to a project that already cannot meet cost targets. Given that full conversion seems nearly impossible, this reviewer would raise the issue of the cost of treating the process wastewater stream, and whether this has this been considered in the cost analysis.

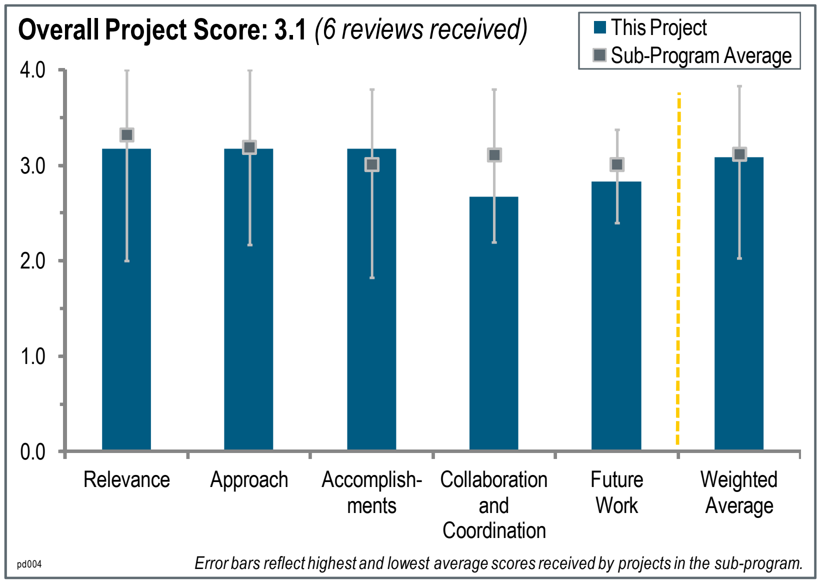
### Recommendations for additions/deletions to project scope:

- This reviewer would recommend a no-go for this project. The funds can be better utilized elsewhere.
- The project team should allow the project to wrap up according to the current plans.
- The project team should look for higher-value $H_2$ applications or close the project.

## Project # PD-004: Distributed Bio-Oil Reforming

Stefan Czernik; National Renewable Energy Laboratory

**Brief Summary of Project:**
The overall objectives of this project are to develop the necessary understanding of the process chemistry, compositional effects, catalyst chemistry, deactivation, and regeneration strategy as a basis for process definition for automated distributed reforming; and to demonstrate the technical feasibility of the process. For fiscal year (FY) 2012, the project will: (1) demonstrate 100 hours of commercial catalyst performance in an integrated bench-scale system, (2) achieve 100 L/h of hydrogen ($H_2$) production at a yield of up to10 g of $H_2$ per 100 g of bio-oil, and (3) assess the process energy efficiency and the cost of $H_2$.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- The project has very good relevance to DOE goals.
- The project is very relevant to DOE's long-term goals.
- Generating $H_2$ from renewable sources helps make $H_2$ fuel cells even more sustainable.
- The project has the potential for developing an effective domestic source of $H_2$ at cost and quality that meets DOE's targets, including greenhouse gas emissions. Uncertainty in feedstock cost is the major concern.
- The project is clearly relevant to the Hydrogen Production sub-program in terms of using renewable domestic feedstocks to produce \$2/gge $H_2$. Depending on the assumed price of the feedstock, the cost target can be met with this technology.
- Autothermal reforming (ATR) of natural gas has been demonstrated for distributed $H_2$ production and has been shown to be able to meet the DOE cost target for $H_2$ production, suggesting that this technology could potentially be a viable approach for utilizing bio-oil—a renewable feedstock—for producing $H_2$. It is not clear how this technology fits into the DOE vision of distributed and centralized $H_2$ production. It is also not clear whether this technology could be utilized in distributed (forecourt) production, or what advantage it would have over direct gasification of solid biomass for centralized production.

### Question 2: Approach to performing the work

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

- The approach resulted in continued progress over the last four years.
- The idea is basically adapting conventional steam reforming systems to the use of biofuels. The project wisely focuses on the two unique aspects of the project: feed preparation and the actual reformations. All other aspects (water-gas-shift [WGS], compression, separation, boiler) are standard chemical engineering equipment.
- First of all, the collaboration team, including Chevron, supports the initial worthiness of using fast pyrolysis followed by autothermal reforming to produce $H_2$. Also, in addition to $H_2$ production, the bio-oil can be used to produce other biofuels, which helps to spread the project risks to other potential markets. Furthermore, the switch

from attempting to develop a catalyst to the use of a commercially available catalyst appears to have significantly helped move the project forward. The overall approach appears to be very methodical and well planned.

- The technical approach is straightforward, with nearly all of the effort apparently being focused on the reformer. The choice of a platinum (Pt)-based commercial catalyst with a relatively low Pt loading is questionable based on the considerable amount of research and development effort to develop gasoline reformers. Rhodium (Rh)-based catalysts, although more expensive, are typically more active than the Pt-catalyst. A life-cycle cost analysis to compare the impact of the choice of catalyst (Pt or Rh) on the overall lifetime operating costs should be performed. It is not clear how much effort is being focused on developing and optimizing the other process unit operations. It is not clear that the technology can produce $H_2$ at DOE target cost, and there is no clear path forward toward meeting the cost target. The project seems more focused on demonstrating a complete system than actually designing, developing, and optimizing the complete system. It was unclear what advantage the electrochemical separator has over more proven, commercial $H_2$ separation/recovery technologies.
- The project team should consider simplifying the biomass-to- $H_2$ process by eliminating the centralized production approach for now and focus on reforming the bio-oil on-site. It is a big leap going from 100 L/hour bench scale to 1,500 kg/day (approximately 7,500 times). Perhaps an intermediate pilot is needed. It seems that the majority of the cost would be from front-end transportation and processing. Even if the bio-oil reforming is technically feasible, the costs associated with going from raw feedstock to reformer feed may turn out to be cost prohibitive. The project team should look more into alternatives to remove impurities prior to reforming. The team needs to get away from methanol additive and look into more practical alternative(s).

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- More work is needed to reach the DOE cost targets.
- Advancing from the building of the bench-scale reactor in 2009 to the demonstration of 100 hours of commercial catalyst performance with 10% yield of $H_2$ demonstrates good progress.
- The team has made significant progress toward improving $H_2$ conversion, but the exact approach/methods used to improve performance are not clearly identified. The team appears to have improved performance from 7.5 g of $H_2$ per 100 g of fuel to 9.1 g of $H_2$ per 100 g of fuel. The team appears to have solved the problem of incomplete gasification by adding a high-temperature filter prior to the reactor. While this is not an ideal solution, it is practical and effective.
- Completion of the integrated bench-scale system is a major achievement to demonstrate proof-of-concept for this technology to produce fuel-cell-quality $H_2$. An estimate of overall process efficiency would have been appreciated, given that it appears a considerable amount of effort was focused on process analysis during the past year.
- The integrated system is performing very well, with 9.1 g of $H_2$ per 100 g of oil, and at 8.2 g/h $H_2$ production on a commercial catalyst. The main issue on progress toward DOE targets will be from the uncertainty in the cost of the bio-oil feedstock. If not solved, it has the potential of tying transportation fuel to an uncertain supply market, which is a problem even if it is domestic and clean.
- Typical $H_2$ content of feed pyrolysis-oil is 6%–8%. Consequently, 10.1 g of $H_2$ per 100 g of pyrolysis oil, reported previously (no WGS reactor) seems high. It would be helpful for the project to quantify the breakdown of $H_2$ contributions—feed, ATR, WGS, methanol, etc. That way, the researchers can target suitable cost reduction opportunities.

### Question 4: Collaboration and coordination with other institutions

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

- This project needs industrial partners.
- Collaboration either was not done or not reported.
- This project features very good coordination and collaboration with the Colorado School of Mines and the University of Minnesota.

- The collaboration team includes a national laboratory, academia, and industry. Each participant adds a strong element to the project, including understanding the process chemistry, developing the catalyst, and identifying the feedstock.
- Historically, this project had two strong collaborations, one with the University of Minnesota, which has been involved in autothermal reforming for nearly 20 years, and Chevron, which has been actively involved in the biofuels arena. However, both of these collaborations ended prior to the current review period, and it is difficult to determine the contribution from these two collaborations to the overall development of the technology. There is an ongoing collaboration with the Colorado School of Mines to develop a kinetic model of the reforming reactions and to define the operating conditions for the autothermal reformer, which would be beneficial to the reactor design. However, there was no discussion of any of the results from this collaboration in this presentation, so it is very difficult to judge the value of the collaboration.

### Question 5: Proposed future work

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

- Longer-term testing and higher yield are very important.
- The future plans build positively on the progress to date. For FY 13, this reviewer thinks that a modeling effort should precede the plans to construct the 200-psi system.
- The project appears to be ready for the logical next step, which involves assembling the fully integrated bio-oil system and complete optimization and performance testing.
- There are very few specifics as to how the team anticipates further improving $H_2$ yield. The team merely states its goals. The plan to test for 100 hours is fine, but the work plan should contain specific metrics for performance at the 100 hour mark (e.g., >9.1 g of $H_2$ per 100 g of fuel after 100 hours of testing at 850°C, 1 atm, etc.)
- Demonstration of 100 hours of performance testing of the integrated system is critical for demonstrating the proof-of-concept of this technology and for providing a more realistic determination of process efficiency and the cost of $H_2$.
- The team needs to put many more hours on the bench-scale unit. Some catalyst accelerated stress testing may be worthwhile. The team should focus mostly on overcoming the technical barriers and should not worry too much about economic numbers at this point. Any cost estimates at this stage are very suspect anyhow. If this works reasonably well on a larger (pilot) scale, the reforming piece will not be a big portion of the overall cost (going from raw feedstock to high-purity $H_2$).

### Project strengths:

- The economic analysis reveals that if the feedstock is reasonably priced, the cost and efficiency targets for $H_2$ production can be met using fast pyrolysis and autothermal reforming.
- The overall integrated process concept is good, and the work to date has shown significant progress toward validating the process design.
- Strengths of the project include the team's ability to manage progressive accomplishments and design and build a $H_2$ production system.
- The project is a clearly structured attempt to achieve high-efficiency $H_2$ production from bio-oil. The project correctly concentrates on the fuel vaporization and reactor. Significant improvement in $H_2$ conversion has been achieved. The proposed solution is straightforward and practical. The statement of reactor flow conditions (space velocity, O/C ratio, temperature) is concise and informative. The use of the Hydrogen Analysis (H2A) model cost estimator on this project is a prime example of how the tool was meant to be used. Key assumptions are appropriately listed.

### Project weaknesses:

- This reviewer has concerns about the high-temperature filter required to remove soot/solid-particles after vaporization of the bio-oil. This reviewer also has concerns about the sulfur tolerance of the catalysts.
- The three critical weakness are: (1) the potential fluctuation of feedstock prices to the upside (however, this may be mitigated with natural gas use for power), (2) the atomization of the bio-oil (there are likely industrial-scale

solutions for this), and (3) bio-oil stability (a solution can be found by working with companies that stabilize other oil-based products).

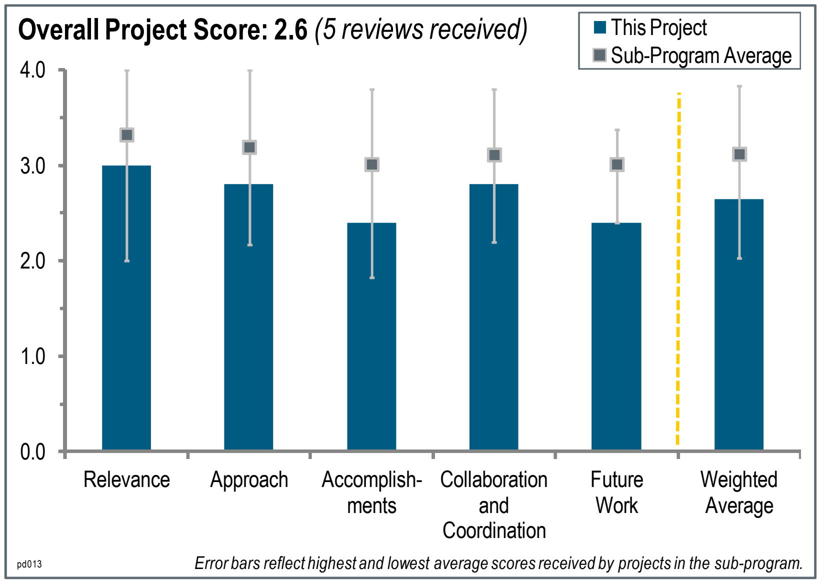
- Stationary reforming systems such as this technology are anticipated to have operating lifetimes well in excess of 40,000 hours. It is not clear that 100 hours of testing is sufficient to project the long-term durability of the catalyst. Degradation of the catalyst will result in increasing quantities of longer-chained hydrocarbon compounds exiting the reformer, which will not only impact the $H_2$ yield but could also have a detrimental impact on the performance of the downstream process units to purify the $H_2$ and may be difficult to reverse. The cost of $H_2$ estimates for the past few years of this project have remained relatively constant and above the DOE cost target. There is no clear path forward identified for reducing the cost of $H_2$ to meet the DOE target.

### Recommendations for additions/deletions to project scope:

- This reviewer would add to the focus the development of a best additive package to stabilize the bio-oil.
- The long-term impact of sulfur in the bio-oil needs to be addressed at some point.
- The team should consider running a model before the 200-psi system final design and build.
- The project team need to focus on what it controls, not feedstock market prices.
- The sulfur tolerance of the catalyst and/or the ability to easily remove sulfur from the fuel should be investigated. The team should explore the cost impact of Pt at the proposed loading levels. It was unclear how much the catalyst cost contributes to the system cost. More explanation is needed regarding the basis of the capital cost estimation, as rising capital cost is part of the reason for the year-to-year $H_2$ cost increase. More work needs to be done to understand the key differences in bio-oil composition and how that affects $H_2$ production. Ideally, the system is robust and can handle a wide range of input bio-oils. The team did not discuss this aspect, but it should do so in the future.
- Given that the $H_2$ yield and cost is dependent on the source of the bio-oil as noted in the presentation, effort should be devoted to evaluating and estimating the $H_2$ production costs and yields from a wide range of bio-oil sources derived from different feedstocks.

## Project # PD-013: Electrolyzer Development for the Cu-Cl Thermochemical Cycle

Michele Lewis; Argonne National Laboratory




### Brief Summary of Project:

The objective of the project is to develop a commercially viable process for producing hydrogen ($H_2$) that meets U.S. Department of Energy (DOE) cost and efficiency targets using the copper-chlorine (Cu-Cl) thermochemical cycle. To increase efficiency in the Cu-Cl electrolyzer process, the project will investigate improvements to membrane and other hardware properties for higher cycle efficiency, reduced costs, and long-term durability of the electrolyzer.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.0** for its relevance to DOE objectives.

- This project is part of the DOE's long-term development portfolio.
- This project is part of DOE's portfolio of projects for long-term central production of $H_2$ from solar energy and is based on thermochemical cycles.
- The project represents a potential long-term option for the production of $H_2$ in a closed chemical cycle. It is not clear how the overall system cost compares to other electrolysis technologies—lower voltage is good, but balance of plant will be considerably more complex.
- The work is reasonably aligned, but it is hard to tell if it is relevant because there is no indication of how these specific technical goals and progress elements impact the commercial viability. In other words, there is a whole cycle here, and this reviewer does not see any explanation of the specific barriers to economic implementation of that cycle, and why these specific research items advance that goal. This reviewer wants to know what the critical targets are that must be reached to achieve the target cost of $H_2$. It is possible that the researchers understand this, but in a review of this magnitude, with outside reviewers, this background is essential, and missing.
- $H_2$ production technology remains challenging. Today, most $H_2$ is made using fossil fuels as starting materials. The proposed research utilizes thermal energy (i.e., heat) as the necessary reactant to synthesize $H_2$. The transformational concept is to generate fuel from waste heat. Low-cost $H_2$ could be the result. Because there are large amounts of waste heat available, this technology, if successfully developed, could generate huge quantities of $H_2$ with no concurrent $CO_2$ emissions. Moreover, significant amounts of heat would disappear, being converted into fuel.

### Question 2: Approach to performing the work

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

- The approach is only fair because it is unclear how this project impacts the feasibility or attractiveness of the $H_2$ production goal.
- The project has an interesting membrane approach with two completely different options. It is unclear how the different approaches impact the system. The team needs to look at cell electrochemical efficiency (it is unclear where polarization is). It would also be interesting to do an overall efficiency analysis, including heating requirements.

- The project is focused on the electrolysis reaction because of the Cu crossover. The project's approach is well in line to address critical issues related to identifying membranes and optimizing the performance of the electrolyzer. However, the technical targets of the project were missing in the presentation.
- The approach is focused primarily on the development of the electrolyzer, with minimal amount of effort related to the rest of the cycle. Because the membrane needed the most improvement, that seems reasonable. The challenge with a hybrid system such as this is that electricity is very expensive, and unless the electricity is produced with renewable power, this process will have greenhouse gas emissions associated with it.
- This project is just one task in the engineering of the Cu+/Cu++ thermochemical cycle. It was decided that the electrolysis step, where the $H_2$ is produced, was the limiting technology, and that funds should be focused on that one process step. Therefore, the approach is to develop a useful unit operation device, which would be needed in a larger, complete system. The project targets center on technical specifications for that device. The technical approach was well thought out, and the approach for evaluation is appropriate.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The target performance necessary for demonstration of promise was successfully achieved. This advance was the result of building a new electrolysis device that allows $H_2$ production using only a small energy input.
- The researchers have made good progress in membrane Cu crossover issues, with multiple pathways now feasible. The team is meeting defined technical milestones, but the technology is still showing significant durability and stability issues. The team needs to increase focus on mechanisms of degradation and place a high priority on understanding and solving this issue. It is not really worth scaling until the device can last more than a couple days.
- The project made progress finding membranes that show no visible Cu deposition and reducing platinum loading. The results are promising, but tests at longer periods of time are needed because current density decreased with time due to degradation processes. The detection of Cu with appropriate methods of characterization should be better investigated to prove that the problem is solved.
- Progress is clearly being made, but again, it is unclear how this research and development quantitatively relates to the overall goal of economic and feasible $H_2$ production. Also, the results seemed scattered—they addressed certain items, but there was no strong theme explaining why these were the right items to address.
- The results for fiscal year (FY) 2012 seem very similar to FY 2011, so there does not appear to be a significant amount of progress. While the presenter showed improved degradation rates, the rates are still too high for a full-size electrolyzer. The Cu crossover issue has not been resolved. A visual examination after 24-hour operation is not sufficient for a system that is expected to operate for thousands of hours.

### Question 4: Collaboration and coordination with other institutions

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

- The collaborators seem well integrated into the project.
- Interactions were not very well defined—several partners were listed, but it is not clear what their roles are or how they are impacting Pennsylvania State University's direction.
- The project has many collaborations, but this reviewer would like to better see the expertise of each partner and its contribution to the project.
- The project has a lot of partners. It is clear what Argonne National Laboratory, the Gas Technology Institute (GTI), and Pennsylvania State University have done. The Canadian group seems to be doing its own work, independent of this project; it is not clear how they are interacting with other members of the project.
- Although this project is a joint U.S.-Canadian activity, technical success depended upon collaboration between GTI and Pennsylvania State University. Those organizations were recruited because of unique technical skills, which were necessary for the demonstrated success.

### Question 5: Proposed future work

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

- The project will remain focused on the electrolysis, with longer-term durability tests.
- Most of the focus should go toward degradation understanding, and the importance of scale-up should be lessened.
- The proposed future work seems like more of the same, which might be fine, but this reviewer cannot tell because it is not described how these specific activities relate to the goal of feasible and economic $H_2$ production.
- There are still many problems with the electrolyzer that need to be overcome prior to the scale-up. Scale-up seems premature.
- The future work was to optimize the electrolysis hardware. In any useful piece of hardware, such optimization needs to be done in concert with the rest of the system. Indeed, it probably makes little sense to move the electrolysis hardware forward until a detailed system design of the complete system is complete. Certainly, heat and mass transfer modeling is required. The concept presented called for additional membrane work. It seems that the demonstrated membrane (the one that showed adequate performance) is good enough for now. Work on the overall system is probably the next step.

### Project strengths:

- The progress versus last year is good; the project is meeting its milestones.
- In the field of solar thermochemical cycles, the major advantage of the project is that the maximum temperature of the process is less than 550°C.
- This is a three-step thermochemical process that is less complex than some of the other processes. There is a large number of partners for the project.
- The concept is compelling. Making useful chemicals from normally wasted heat fluxes with no concurrent $CO_2$ seems too good to be true. The concept is the strongest part of this activity.

### Project weaknesses:

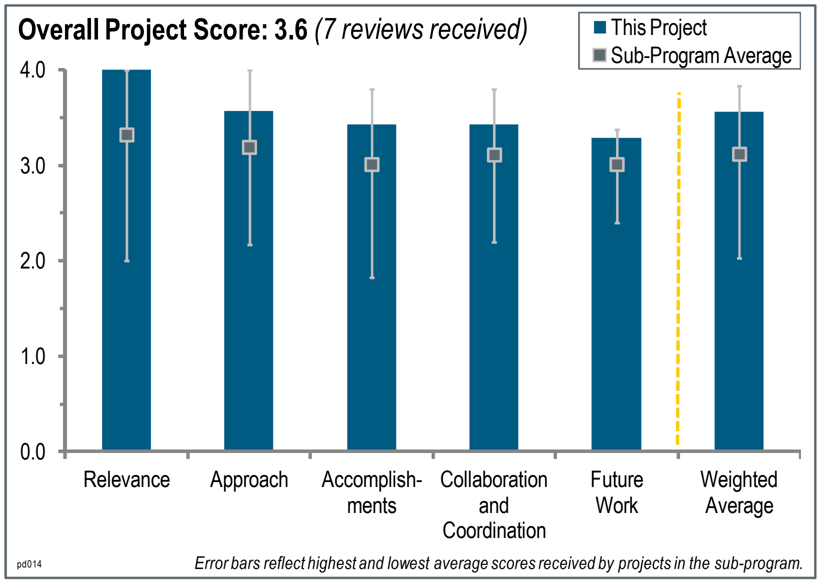
- This project addresses a very big topic and has a very small budget.
- Cell efficiency (power) should be explicitly stated in the presentation, along with any progress from the last year. For a long-term technology, the focus should be on resolving the critical materials issues such as durability; this should be the focus for next year.
- The detection of Cu by relevant and convincing methods of characterization is missing. It cannot be concluded that the critical problem of Cu crossover is solved only because there is no visible detection of Cu. Then, the results are obtained on short, 36-hour tests.
- Clearly, the weakness was framing. The value proposition, what the critical needs are to achieve that value proposition, and how each of the technical elements contributes to solving those problems were not clear.
- All hybrid cycles for thermochemical $H_2$ production require an electric power source. This is a significant weakness for these processes.

### Recommendations for additions/deletions to project scope:

- The project team should focus on understanding degradation issues.
- This reviewer would not support any project that cannot clearly state its value proposition.
- First of all, researchers should confirm by adequate characterization that there is no Cu deposition, because this milestone is key to going further. Then, before investigating too much on membrane degradation mechanisms, the team should brainstorm with the membrane community to take advantage of the knowledge accumulated these past years on this topic in various fields (fuel cell, polymer electrolyte membrane electrolysis, etc.).
- The project team should return the emphasis to a model of the entire process, using the performance numbers demonstrated with the current, improved electrolysis. If possible, the team should do some work on (short-term) durability experiments to show that the membrane has at least the ability to run for a few days. The team should make plans (cost, time) for a project that designs, fabricates, and evaluates a prototype at a significant production rate.

# Project # PD-014: Hydrogen Delivery Infrastructure Analysis

Marianne Mintz; Argonne National Laboratory




## Brief Summary of Project:

This project will provide a platform to: (1) evaluate hydrogen ($H_2$) delivery cost, energy usage, and greenhouse gas emissions; (2) estimate the impact of alternative conditioning, distribution, storage, and refueling options; (3) incorporate advanced options as data become available; and (4) assist the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program in target setting. It will compare alternative component and delivery system options; assist in technology program planning; and support existing DOE-sponsored modeling efforts including Hydrogen Analysis (H2A) model components; H2A production; the Macro-System Model; Jobs and Output Benefits of Stationary Fuel Cells; and the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation Model (GREET).

## Question 1: Relevance to overall DOE objectives

This project was rated **4.0** for its relevance to DOE objectives.

- This work has identified and is addressing the key near-term challenges to make this market work.
- This is a very helpful tool to track technology paths and adjust program area priorities.
- This work has been vital to guiding the Hydrogen Production and Delivery sub-program. It has enabled the sub-program to understand where costs are in the delivery pathways and to focus research where it will make a difference.
- Given the range of delivery options and challenges involved with each of these options, analysis is necessary to provide insight into the cost and cost sensitivities of different delivery scenarios on an “apples-to-apples” basis. The project contributes significantly to that effort.
- The H2A Delivery Scenario Analysis Model (HDSAM) is critical to understanding the cost limits of $H_2$ delivery. Argonne National Laboratory (ANL), particularly the principal investigator, has been a key analyst for this development and implementation.
- The project is extremely relevant. It can be used to provide a roadmap for the rest of the Hydrogen Production and Delivery sub-program projects. In particular, it demonstrates that compressor cost is a cost factor that needs serious attention. Furthermore, the project found that station capital cost varies not only as a function of throughput and power, but of vendor as well.

## Question 2: Approach to performing the work

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

- The project features very convincing work. This reviewer likes the modeling work and the clear identification of operational barriers.
- It is a great approach to integrate the technology options for shipping, compression, liquefaction, storage, and dispensing for $H_2$ delivery in a usable model.

- Overall, this project has a good approach. It was not obvious who is setting the priorities in the sub-program's projects and who decides which topics to analyze. For example, this reviewer wants to know how industry is involved, besides providing data.
- ANL has been proactive in developing models and seeking input from stakeholders to ensure that the models are accurate. Excel-based models have been developed that are simple to use and widely available to anyone interested in $H_2$.
- Compression, tube trailer, trucking, and forklift cases were analyzed during the current period, representing high-priority scenarios that address important delivery barriers. The presentation did not make apparent the basis of the forklift refueling cost estimate or, thus, the confidence in and error bars around the estimated costs.
- The project evaluated current compressor technologies, high-pressure tube trailers, and $H_2$ tracking options, and assessed differences between fuel cell electric vehicles (FCEVs) and forklifts. The reported approach on slide 4 is quite comprehensive, as it addresses the spectrum of the solution pathways by carefully relying on data from research project results, industry, and DOE.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Overall, the project features good results. The analysis of "hydrogen trucking options" seems quite superficial and is not very surprising.
- The HDSAM update and completing three of the five evaluation milestones for several different technologies appears to be good progress.
- This reviewer appreciates the fact that the study remains focused on how to make $H_2$ an economical solution within the next five to eight years. Acknowledging the current technological challenges and looking for ways to resolve those challenges is terrific.
- This year's exploration of early market conditions has been important for understanding transition issues. The work has been important in understanding the trade-offs between liquid delivery with pumping versus gas delivery with compression.
- Delivery costs are a substantial barrier to $H_2$ FCEV and fuel development. This model has given the U.S. DRIVE Partnership team a means to evaluate the key cost barriers and address technology research and development related to its solutions.
- The project essentially assessed the delivery issues and the capital utilization for early market penetration, suggesting that delivery station cost should be pushed upstream and compressor technology cost should be addressed in earnest, as it presents several challenges (see slide 13). The results reported on slide 10 are very important because they stress a hard-to-explain variability of compressor cost among various vendors.

### Question 4: Collaboration and coordination with other institutions

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

- The project has a broad range of collaborators.
- There is good collaboration among the national laboratories that are working together to make HDSAM better.
- The project team performed good work with stakeholders to get model input.
- This reviewer did not see explicit industry collaboration and wants to know how industry is involved.
- This reviewer did not pick up on clear collaboration with other institutions; however, this reviewer is aware of the project's effort to consult with industry. This is important because it can save time and capture the techno-economic challenges that come with certain solutions.
- The project's results demonstrate an extensive network of collaborations with national laboratories and industry. This reviewer assumes progress would not be as reported if it were not for these collaborations. The participation of M. Paster in the project is important.

### Question 5: Proposed future work

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

- The directions look good.
- The goals are on-point to help $H_2$ fuel become a fully integrated solution in the U.S. economy.
- The future work focuses on high-priority delivery issues.
- The future work was rushed due to the expiration of the presentation time.
- It was not obvious who is setting the priorities in the projects and who decides which topics to analyze. For example, one reviewer wants to know how industry is involved, besides providing data. This reviewer is very interested in the ionic compressor analysis and the comparison to other compression technologies.
- The proposed work reported on slide 26 is the appropriate next step for the project. With regard to bullet item 3 on slide 21 (evaluate storage technology options and new concepts), attention should be drawn to how the various storage concepts will be assessed. For instance, storage concepts such as the steel/concrete vessel presented in this DOE Hydrogen and Fuel Cells Program Annual Merit Review have many deficiencies in their design and, as such, incorporation in the project's analysis may lead to erroneous results.

### Project strengths:

- Of all of the projects, this may deliver the most value to the development of $H_2$ energy commercialization.
- The model allows flexibility regarding different options for $H_2$ delivery and predicts costs.
- The systematic inclusion of parameters affecting cost is a strength of this project.
- The model is a very valuable and flexible tool to help DOE sharpen its program. A strength of this project is the active national laboratory partnership.
- This project features excellent, unbiased analysis by the principal investigator and his colleagues. It offers a high return on DOE investment.

### Project weaknesses:

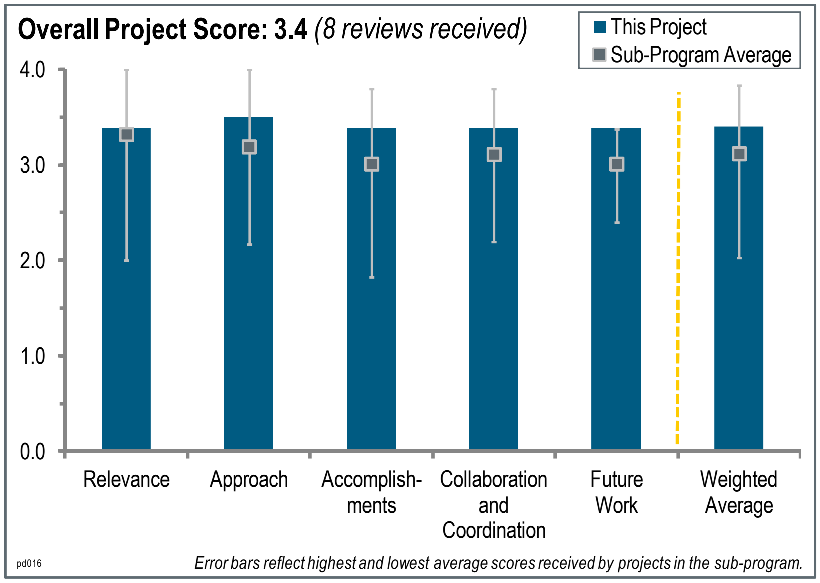
- This project really deserves more resources to fully vet the identified goals.
- The model needs validation of predicted costs.
- It was not obvious who is setting the priorities in the projects and who decides which topics to analyze. For example, this reviewer wants to know how industry is involved, besides providing data. In some cases, the analysis appears too superficial.
- It is not clear why the compressor cost is vendor dependent. Reasons need to be researched and documented. The project seems to focus on early FCEV markets; more explanation is needed on long-term markets. Also, there are no details on the technical approach taken to cost various solution pathways. For instance, this reviewer would like to know how the soundness of specific technologies is weighed in. By way of example, the steel/concrete vessel proposed to be examined in the future is a technology that is based on design estimates, and, as such, the related costing may lead to an unrealistic and unreliable assessment of the relevant solution pathway.

### Recommendations for additions/deletions to project scope:

- The project team should provide transparency on how topics are selected and how industry is involved.
- This reviewer felt that there were no additional recommendations to make.
- The comparison between volume utilization results for steel and composite tubes reported on slide 16 perhaps is unfair to the steel option. It is not clear what steel design criteria have been used to estimate the tube thickness. To the knowledge of this reviewer, the criterion used by the industry—namely that the hoop stress must be 30% of the elastic limit—is over-conservative. If this criterion—which is based on a maximum allowable stress—was used, the analysis needs to be revisited. A fracture mechanics analysis of the type leak before break is the proper one.

## Project # PD-016: Oil-Free Centrifugal Hydrogen Compression Technology Demonstration

Hooshang Heshmat; Mohawk Innovative Technology, Inc.




### Brief Summary of Project:

The objective of this project is to design a reliable and cost-effective centrifugal compressor for hydrogen ($H_2$) pipeline transport. The goal specifications for the compressor are that it should be able to handle a flow of 240,000 to 500,000 kg/day, handle a pressure rise from 300–500 psig up to 1,200–1,500 psig, and be able to operate with contaminant-free/oil-free $H_2$.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- Relevance to the Hydrogen Production and Delivery sub-program is high because compression is a key cost item barrier for $H_2$ delivery.
- This project addresses the cost of large-scale gas compression as a significant economic barrier.
- Although this reviewer admires the work being performed, this reviewer does not think this project has any relevance for the next 10 years regarding aiding the commercialization of the fuel cell markets. When it does begin to have relevance, advancements in materials will probably relegate this technology to the shelf.
- Based on Mohawk's estimates, the technology can meet the DOE compression performance, capital investment, and maintenance cost targets.
- Work is 80% complete. The barriers are well defined. Project targets have been discussed and met in work completed as of today. The design concept of bearings, thrust bearings, foil seals, and compressors are very advanced and computational fluid dynamics (CFD) analyzed. The design meets DOE targets in cost and weight. Size was not addressed. Structural materials issues are resolved.
- The cost-effective and safe compression of $H_2$ is critical to the DOE Hydrogen and Fuel Cells Program. This project promises to deliver a compression technology that utilizes state-of-the-art technologies and new technologies emerging from current research.
- The project is extremely relevant. Compressor technology is an essential component of $H_2$ transport. Although the design meets the DOE targets (see slide 9), issues related to system aerodynamics, materials selection, and performance in the presence of $H_2$ need to be addressed.
- Cost-effective and reliable $H_2$ compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest pressure bulk storage facilities will ultimately be vital for viable use of $H_2$ as a major energy carrier. Currently, only reciprocating compressors work for these applications; they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with $H_2$, such as that being developed in this project, could result in more cost-effective and reliable $H_2$ compression. The widespread use of pipelines for $H_2$ transport is not likely to occur until $H_2$ has made considerable penetration into the transportation market. Recent analyses of $H_2$ delivery indicate that pipeline compression is only a relatively small part of the cost of delivering $H_2$. There are other more pressing $H_2$ delivery issues that need to be resolved, such as refueling station compression costs and $H_2$ storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. Although the range of costs approaches the 2012 and 2017 cost targets for this application in the currently posted Hydrogen Delivery Multi-Year Research and Development Plan, the costs are significantly

higher than the current (2010) costs in the recently updated and posted Hydrogen Analysis (H2A) Delivery Scenario Analysis Model (HDSAM) Delivery Model.

### Question 2: Approach to performing the work

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

- It was a good approach to partner with Mitsubishi to allow developments to be used in the natural gas compression industry.
- The project approach is a modular single- or double-entry centrifugal compressor. System CFD analysis and component finite element analysis (FEA) have been performed.
- This reviewer hopes that the work performed can be applied, but this reviewer suspects that a very large testing program will be required to validate the design work and stage the technology for commercial consideration. This reviewer would not advocate DOE funding for that test activity.
- The hermetically sealed, oil-free centrifugal compressor design, assembly, and testing for single-stage compression is a reasonable approach for bench-scale analysis of pressure and flow characteristics. Running the $H_2$ through the single stage six times should give a good indication of the six-stage compressor performance. Also, doing validation studies at both Mohawk Innovative Technology, Inc. (MiTi) and Mitsubishi Heavy Industry (MHI) provides additional confidence in the performance results from the oil-free coupling technology.
- The project appears to have a pragmatic and systematic approach. The presentation could be improved. The approach was not very obvious.
- The components are in assembly stage and appear to be on track for full-scale testing in the near future. Tests were successfully completed on the motors.
- The approach features an excellent mix of research, emerging technologies, state-of-the-art technologies, and proven technologies. That is, if a significant improvement in compression technologies is to be made that reduces energy consumption and costs, it will not be made using only existing technologies (or it would already exist today). This project appears to be consulting and utilizing resources from other DOE projects, DOE laboratories, universities, and other entities better than any other project seen by this reviewer. In particular, this project presented its design and a list of issues, plans, and tests all designed to address the identified issues. This made the approach appear more carefully thought out and planned than other projects.
- The project is taking an excellent approach in most ways to achieve its objectives. It is using state-of-the-art centrifugal design methods, including mean line CFD and finite element analysis. It is incorporating the state-of-the-art foil bearings and seals needed to achieve breakthrough rpms while totally eliminating all oil lubricants, thus eliminating any possibility of oil contamination of the $H_2$. Building a full-scale single-stage unit for rigorous testing and evaluation will yield extremely valuable results without the excessive cost of building a more complete test compressor. There are two potential designs being evaluated within the project. This increases the probability of success. The project includes both MiTi, which had the original concept, and MHI, which brings tremendous experience and expertise in current large centrifugal compressor technology. The project is now getting world-class advice from Sandia National Laboratories (SNL) and Dr. Sofronis at the University of Illinois on $H_2$ embrittlement relative to material selection, and the plan includes testing of materials at SNL and the National Institute of Standards and Technology (NIST). More attention is still needed on material issues in this $H_2$ environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit be run with $H_2$, but there are no plans to do this within this project. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. Although the range of estimated costs approaches the 2012 and 2017 cost targets for this application in the currently posted Hydrogen Delivery Multi-Year Research and Development Plan, the costs are significantly higher than the current (2010) costs in the recently updated and posted HDSAM Delivery Model.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Mohawk has built the first oil-free $H_2$ compressor using foil bearings.
- The accomplishments are good, but in terms of a commercial design, probably 5–8 years will be required to demonstrate a truly functional, oil-free compressor.

- The assembly of the single-stage compressor is a major accomplishment that clears the path for reliability performance testing and improved cost estimates.
- The results are promising compared to DOE targets, but they are only based on “MiTi estimates” (slide 9). Material selection is based on an extensive literature search and consultation with $H_2$ embrittlement experts. This is not sufficient.
- With the current status of 80% complete, the compressor system appears to meet all DOE goals and requirements.
- Very good progress has been made and the results to date are very encouraging. The compressor, subcomponents, and single-stage test unit have been designed. The single-stage test unit has been fabricated and assembled, and some testing and results have been completed. Material issues and selection for the $H_2$ environment have been discussed with experts in the field, and initial material selection has been completed. However, testing these materials appropriately in $H_2$ and under operating conditions has not yet been done.
- This project appears to excel in anticipating all of the issues and making plans accordingly. The investigators appear to be on schedule to complete initial verification testing in fiscal year 2012. The future work appears to involve some decision making between two different systems being developed by completely different parts of this team, and a redesign incorporating the best features of both designs is planned in the future. A pro-con table for these two systems was presented. However, this decision will almost certainly be made on the basis of cost. As a result, this decision will require accurate and equivalent cost estimating for both systems, which the team appears unprepared to do. Leaving cost estimating primarily to one part of the team or the other will make the decision, instead of the attributes of the different designs. That is, progress toward this decision point did not seem to be as outstanding as it was in the other areas.
- Material issues and needs have been thoughtfully identified as they pertain to the individual components. Components have been manufactured and assembled. Initial motor spin testing has been performed (slide 16), bearing temperature has been estimated as a function of time (slide 17), and compressor dynamic verification analysis was done up to 30,000 rpm. All of these accomplishments are significant.

### Question 4: Collaboration and coordination with other institutions

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

- There are not many collaborators, but good progress was made with those used.
- The collaborations with SNL and NIST are important and beneficial to the project.
- Industry is involved. Commercial potential is identified (slide 4).
- It is clear that there is collaboration with MHI on this project, but this reviewer did not have the sense that the level of collaboration was high. Beyond MHI, this reviewer did not pick up on meaningful collaboration work.
- The collaboration with MHI brings to bear some important resources, not to mention an important industry player that could be a potential end user of the centrifugal compressor technology. Also, the compatibility studies of the foil bearing and foil seal materials with $H_2$ from NIST add to the design safety.
- Industry partners are already building and using 2-stage $H_2$ compressors. This work is seen as a natural step. National laboratories are also participating as consultants.
- The project includes both MiTi, which had the original concept, and MHI, which brings tremendous experience and expertise in current large centrifugal compressor technology. The project is now getting world-class advice from SNL and Dr. Sofronis at the University of Illinois on $H_2$ embrittlement relative to material selection, and the plan includes some testing of materials at SNL and NIST.
- This project appeared to be the best in this category of any this reviewer saw at this meeting. That is, the researchers appeared to not only be talking and consulting with other laboratories, but actually listening. Actual design changes could be tracked to consultations with other laboratories and projects.

### Question 5: Proposed future work

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

- All testing in line with ASME requirements will be completed. The $H_2$ comparison will be completed. The project team needs to refine its estimates of capital cost to scale-up production.
- There is little value for the DOE Hydrogen and Fuel Cells Program in pursuing future work on this design.
- No tests with $H_2$ are planned or included in the existing budget.

- There are good plans to demonstrate compressor use at 60,000 rpm. It is good to see the concern for safety at 60,000 rpm.
- The future work is well thought through and designed. It includes completing full testing and evaluation of both single-stage test units in air and helium (He), selecting the best approach, and completing the full compressor design based on the results of the single-stage testing. $H_2$ material compatibility of the foil seals and foil bearings will be done at SNL and NIST. Finally, the cost estimate will be updated. More attention is still needed on material issues in this high-pressure $H_2$ environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit is run with $H_2$; this is not currently in the project plan.
- There is an excellent plan for validation testing and the evaluation of performance and costs within the scope of the budget. The final design needs to be subjected to $H_2$ testing and durability testing. However, the decision between these two competing systems needs to be made before this can be included as part of the project.
- The goal is to select between double- and single-entry designs and assess capital cost in relation to the DOE 2017 targets. A detailed testing plan is outlined on slide 20 with a proposal for material testing for foil bearings and foil seals to be carried out at SNL and NIST. However, testing of the entire system in the presence of $H_2$ is missing from the proposed work.

### Project strengths:

- A strength of this project is how it considers the challenges and opportunities to develop an oil-free compressor for $H_2$ service.
- The two industrial partners, MiTi and Mitsubishi, are quite capable of proving or disproving the feasibility of the technology.
- Strengths of this project include industry collaboration, promising estimates, and commercial potential.
- The project looks to be on target to meet or exceed all DOE requirements.
- This project features a novel use of foil bearings for the first time in a compressor.
- The system design is in place, meeting DOE targets (slide 9); components have been fabricated; material issues have been carefully outlined on slide 11; and relevant laboratory testing at SNL and NIST has been proposed.
- This project features excellent coordination and consultation with other DOE projects, laboratories, universities, and other entities, including consultations early enough to actually affect the design process and the final design. There is a nice combination of big and small company collaborations, giving the impression of a team with combined excellence in innovation and manufacturing.
- Cost-effective and reliable $H_2$ compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest-pressure bulk storage facilities will ultimately be vital for the viable use of $H_2$ as a major energy carrier. Currently, only reciprocating compressors work for these applications, and they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with $H_2$, such as that being developed in this project, could result in more cost-effective and reliable $H_2$ compression. The project is taking an excellent approach in most ways to achieve its objectives. It is using state-of-the-art centrifugal design methods, including mean line CFD and FEA. It is incorporating the state-of-the-art bearings and seals needed to achieve the breakthrough rpms into the design. Building a full-scale, single-stage unit for rigorous testing and evaluation will yield extremely valuable results without the excessive cost of building a more complete test compressor. There are two potential designs being evaluated within the project. This increases the probability of success. The project includes both MiTi, which had the original concept, and MHI, which brings tremendous experience and expertise in current large centrifugal compressor technology.

### Project weaknesses:

- The market need is decades away.
- This reviewer does not think that this will eliminate the need for compressor redundancy. It would be useful to converse with natural gas experts to get their thoughts.
- The material selection based on literature research is a weakness. No $H_2$ tests are included in the existing budget. Another weakness is the cost of approximately $7 million, compared to approximately $4 million for a compressor from PD-017.
- This reviewer felt that there were no weaknesses.
- There has not yet been a test of the compression system with $H_2$.

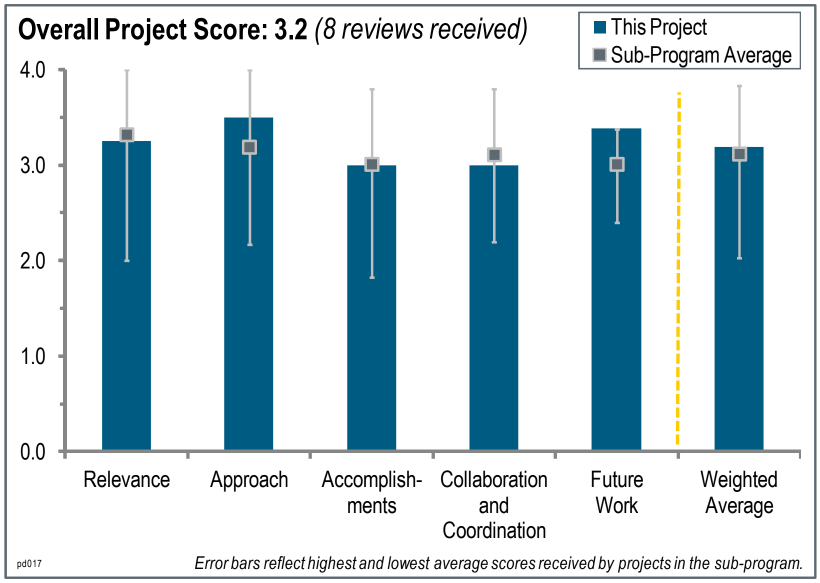
- The use of Beta Titanium as rotor material needs to be revisited. This high-solubility system is well known to be $H_2$-embrittlement susceptible. Testing of the entire system in the presence of $H_2$ is missing from the proposed work.
- The widespread use of pipelines for $H_2$ transport is not likely to occur until $H_2$ has made considerable penetration into the transportation market. Recent analyses of $H_2$ delivery indicate that pipeline compression is only a relatively small part of the cost of delivering $H_2$. There are other, more pressing $H_2$ delivery issues that need to be resolved, such as refueling station compression costs and $H_2$ storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. Although the range of costs approaches the 2012 and 2017 cost targets for this application in the currently posted Program Multi-Year Research and Development Plan, the costs are significantly higher than the current (2010) costs in the recently updated and posted HDSAM Delivery Model. More attention is still needed on material issues in this $H_2$ environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit is run with $H_2$. This is not part of the current project plan.
- Big partners can push small partners around without even knowing that they are doing it. This project is a year or two away from a critical decision between two competing designs being developed by big and small partners. It is unclear who will make this decision and on what criteria. It also appeared that the cost estimating input to the decision for both designs would be done by the large partner. One would like to think that performance and cost will be the only criteria in the decision, but corporate decisions are rarely this simple. What happens after this decision was also unclear. The presentation said that a redesign incorporating the best of both would be developed. Validation, qualification, and durability testing will eventually be required, and it appeared that all of this would be done by the large partner, as the current budget ends with He testing.

### Recommendations for additions/deletions to project scope:

- This reviewer recommends performing cost analysis compared to other technologies, as well as material compatibility tests.
- Additional testing at national laboratories (possibly at NIST in Boulder, Colorado) need to be completed to study mechanical loading.
- More attention is still needed on material issues in this $H_2$ environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit is run with $H_2$.
- Testing in $H_2$ should be added.
- The investigators should test with $H_2$.
- The process for material selection and testing in the presence of $H_2$ should be accelerated and finalized. In particular, the selection of titanium for the rotor should be justified and validated for the given environmental operating conditions.

## Project # PD-017: Development of a Centrifugal Hydrogen Pipeline Gas Compressor

Frank Di Bella; Concepts NREC




### Brief Summary of Project:

The objective of the project was to design and model an advanced centrifugal compressor system for high-pressure hydrogen ($H_2$) pipeline transport to support: (1) delivery of up to 1 million kg/day of pure $H_2$ to forecourt stations at less than $1/gge with less than 0.5% leakage and pipeline pressures of >1,200 psig, (2) a reduction of initial system equipment costs to less than $6.3 million (U.S. Department of Energy [DOE] model), (3) a reduction in operating and maintenance costs through improved reliability, and (4) a reduction in system footprint.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The work performed to date is valuable, however, as the requirements for the $H_2$ and fuel cell markets develop, the relevance of this development work has receded. If and when high-pressure $H_2$ pipelines are needed, the work presented will no longer be relevant to the commercial markets.
- Pipeline delivery of 99.99% $H_2$ at <$1/gge with 98% efficiency is aligned with the goals of the Hydrogen Production and Delivery sub-program.
- This project addresses the cost of large-scale gas compression as a significant economic barrier.
- The third phase is construction and validation while staying focused on state-of-the-art structural analysis. The problem is an aerodynamic issue, given the speed of the compressor.
- It is critically important that the energy spent compressing $H_2$ is minimized through the development of new, scientifically sound compression technologies and robust designs. The safe and reliable operation of these is a critical component of meeting this cost objective and ensuring public safety.
- This project is relevant to the success of the U.S. DRIVE Partnership program.
- The project is extremely relevant. Compressor technology is an essential component of $H_2$ transport. Issues related to system aerodynamics, materials selection, and performance in the presence of $H_2$ need to be addressed.
- Cost-effective and reliable $H_2$ compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest-pressure bulk storage facilities will ultimately be vital for the viable use of $H_2$ as a major energy carrier. Currently, only reciprocating compressors work for these applications, and they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with $H_2$, such as that being developed in this project, could result in more cost-effective and reliable $H_2$ compression. The widespread use of pipelines for $H_2$ transport is not likely to occur until $H_2$ has made considerable penetration into the transportation market. Recent analyses of $H_2$ delivery indicate that pipeline compression is only a relatively small part of the cost of delivering $H_2$. There are other, more pressing $H_2$ delivery issues that need to be resolved, such as refueling station compression costs and $H_2$ storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. The projected cost of the centrifugal compressor under development in this project is $4 million (for 240,000 kg/day). This is about equal to the current (2010) costs in the recently updated and posted Hydrogen Analysis Delivery Scenario Analysis Model (HDSAM) Delivery Model. The Hydrogen and Fuel Cells Program (the Program) is targeting to reduce this cost.

### Question 2: Approach to performing the work

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

- This project features a very systematic and professional technical approach.
- The goals are well defined. The work will satisfy the original feasibility design.
- The conceptual development of this system is admirable; however, the real challenge in developing a technology of this nature rests with testing and manufacturing programs, which this organization is not really prepared to do in a rigorous manner.
- The compressor, if successful, may have applications to improving natural gas compression reliability so the risk can be shared by multiple industries. Also, the next phase of advancement includes building the centrifugal compressor with commercially available proven parts, such as bearings and seal technology. This, hopefully, will reduce manufacturing performance risks and costs. Once assembled, performance testing will commence and better manufacturing and installed cost estimates should be forthcoming.
- The presenter clearly defined his approach and the goal of meeting the requirements using a maximum amount of commercially available supplies and equipment in order to keep the per-unit cost down. The cost of any compromises this approach induces was unclear from the presentation.
- Concepts NREC planned on using existing equipment to design the needed compressor.
- The approach involves aerodynamic analysis of the entire system and its individual components. In this regard the project looks promising, if cost could be further brought down.
- The project is taking an excellent approach to achieving its objectives. It is using state-of-the-art centrifugal design methods, including finite element analysis (FEA). It is incorporating the currently available state-of-the-art tilt-pad bearings, gas face seals, and other components to improve the probability of success while focusing on the design and materials for the rotors needed to incorporate the breakthrough revolutions per minute (rpm) into the design. The project includes close attention to the material selection relative to required strengths and $H_2$ embrittlement. Testing of components in $H_2$ is an integral part of the project. Building a single-stage prototype for rigorous testing and evaluation, including operating it with $H_2$, will yield critically valuable results. The project includes collaboration with Air Products, which has extensive $H_2$ compressor experience, as well as centrifugal compressor experience all under commercial use conditions. It is also doing work at Texas A&M University and Savannah River National Laboratory (SRNL) on testing materials in $H_2$ environments. Finally, the researchers are getting advice from Sandia National Laboratories (SNL) and Dr. Sofronis at the University of Illinois, who are arguably the best in the world relative to material issues in $H_2$ environments. The testing and choice of aluminum for the rotors appears to be resulting in a much lower costs compared to the use of stainless steel to withstand the $H_2$ environment. Parts of the total compressor are lubricated with oil. Good attention has been paid in the design to try to eliminate the possibility of oil contamination, but only testing will confirm if there is no oil contamination of the $H_2$. It is not clear whether the work being done on coatings is of significant value, because suitable and reasonably priced materials of construction have been identified.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The organization has performed good work in the design phase of this effort.
- Until the compressor is built and fully functional, it is hard to say whether the design exercise to date has been worth it.
- There have been very promising results for Phases I and II. DOE proposal requirements were satisfied with the feasibility design. This reviewer gives the project only three points due to the lack of material analysis and tests.
- The presentation made it clear that progress is being made at an acceptable rate and that system validation testing should be completed early in the next fiscal year. Continued testing in $H_2$ will be important if the unit performs acceptably in other testing.
- The team has made fairly good progress. It needed to down-size from a two-stage prototype to a one-stage prototype due to cost.
- The project team has made excellent progress with developing a 6-stage compressor to meet the delivery requirements. This reviewer is not sure about the materials selection to avoid $H_2$ embrittlement concerns. Mass

production with aluminum casting will increase the potential of casting voids. Low-strength steels may work as well; however, they may be more comparable for high-volume production.

- Very good progress has been made and the results to date are very encouraging. Critical components have been developed or specified for near-term availability. Detailed design and cost analysis of the full-size, six-stage compressor has been completed. A one-stage laboratory unit has been designed—parts are being procured and it is being assembled. Critical materials of construction questions relative to the strength and a hydrogen environment are being addressed through both testing and expert advice. It appears the project has fallen a bit behind schedule, with testing of the prototype compressor to be completed in March 2013 versus November 2012. The compressor, subcomponents, and single-stage test unit have been designed. The single-stage test unit has been fabricated and assembled with some testing and results completed. Material issues and selection for the $H_2$ environment have been discussed with experts in the field, and initial material selection has been completed. However, testing these materials appropriately in $H_2$ and under operating conditions has not yet been done.
- The overall system analysis is appropriate and the proposed design meets the DOE target requirements. Several milestones have been reached: a full load laboratory prototype has been designed, an algorithm for emergency shutdown is in place, and FEA stress and aerodynamic analyses of all of the components have been performed. On slide 14, it is stated that the compressor has been successfully spun to 10% over speed for 15 minutes. Item 3 of slide 14 reports low blade frequency. It is not clear why the blade frequency is low.

## Question 4: Collaboration and coordination with other institutions

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

- Collaborations cover the required expertise. Industry is involved.
- There is excellent coordination with industry subject matter experts to oversee the progress. This reviewer suggests adding vibration measurements in the single-stage system being built for future testing.
- This project features an excellent group of partners and planned collaborations.
- There is great cooperation with other vendors for compressor module as well as national laboratories and Air Products.
- This reviewer noted collaborations with Texas A&M and Air Products. Given the respected interests (coatings and design), these are good connection points and should be developed further. Given the early design and development status, it seems vital to have collaborations that target the eventual licensing and development testing of the compressor design.
- Praxair has been replaced by Air Products. This makes one wonder if this was due to a significant issue. Otherwise, if this is a reliable approach, it is unclear why a large producer of $H_2$ would lack interest. This reviewer is also curious about the reliability of Air Products as a collaborator and a potential tester of the field compressor. Other than these questions, the project appears to be well balanced in terms of collaborators.
- The project includes collaboration with Air Products, which has extensive $H_2$ compressor experience as well as centrifugal compressor experience all under commercial use conditions. It is also doing work at Texas A&M University and SRNL on testing materials in $H_2$ environments. Finally, it is getting advice from SNL and Dr. Sofronis at the University of Illinois, who are arguably the best in the world relative to material issues in $H_2$ environments. Collaboration with a commercial producer/supplier of centrifugal compressors and possibly a commercial producer/supplier of very large $H_2$ reciprocating compressors would add strength to this project team.
- The reported partnership with Air Products is good and collaborations with SRNL and SNL are important if they are substantial—for example, as stated on slide 20 for work with Dr. San Marchi. The collaboration with Texas A&M is not contributing to the project's health and robustness. In fact, this collaboration is a serious project weakness. The technical approach of Texas A&M on assessing $H_2$ embrittlement through the punch test is an approach the ASME codes and standards committee does not even consider for discussion. Force-displacement data (e.g., uniaxial tension test) are used for material screening purposes only; in fact, there is no underlying physics that justifies transferability of these data to fracture toughness estimation.

### Question 5: Proposed future work

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

- The project is nearing completion in meeting the target goals.
- The project is a bit rushed on the future plans for the Phase III building compressor.
- The project team should make sure the materials are compatible.
- The control system has yet to be built. It was unclear if it has been modeled under operating conditions to validate it. Future plans are to have the unit tested at a future site still under consideration.
- The proposed future work appears to be on a slow path through system development. At this point, for the technology to be relevant for $H_2$ service or maybe other applications, it needs to be put into the hands of a competent test and commercial design organization that knows how to commercialize technology of this nature. One reviewer is doubtful that Concepts NREC can achieve the ultimate objectives.
- Another reviewer wonders if there will be an impact on future maintenance costs if the unit is hermetically sealed. Also, in consulting with Air Products and Praxair, this reviewer wants to know if the researchers really think that this compressor will be so reliable that they will not need a redundant compressor.
- The future work is well thought through and designed. It includes completing and fully testing and evaluating the laboratory prototype test unit with $H_2$. Further testing of materials is also planned. It would be better if the design and cost estimate of the complete commercial size compressor were to be reviewed and updated based on the results of the single-stage prototype testing. It is not clear that the work planned on coatings is of significant value because suitable and reasonably priced materials of construction have been identified.
- The proposed future work is reported on slide 21. The proposed assembly as a completely functioning compressor system and the installment of a laboratory prototype system are appropriate tasks to be pursued. However, testing of the entire system in the presence of $H_2$ is missing in the proposed work. It is proposed that coatings will be investigated in the presence of $H_2$. It is not clear why coatings are needed and what components of the compressor are required to operate with coatings. In fact, nowhere in the presentation was there any mention of a deficiency of the structural materials, as they would operate in the presence of $H_2$ and the absence of coatings mentioned.

### Project strengths:

- The concept design is an area of strength.
- The design and engineering expertise appears to be sound.
- One strength is the industry collaboration. The project has produced very promising results compared to DOE targets.
- The project features strong industry partnership with excellent oversight of work.
- The team has produced a real, working one-stage demonstration for system validation testing in fiscal year 2013.
- Good progress with building a new compressor using existing components.
- A strength of this project is the complete aerodynamic and system analysis and the choice of Aluminum 7075-T6 as a rotor material. However, given the low strength of the aluminum alloy and that the rotor will operate at 66,000 rpm, the project should report the safety factor used in the stress analysis (see slide 12). It was unclear if the safety factor is appropriate according to standard practices in compressor technology.
- Cost-effective and reliable $H_2$ compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest-pressure bulk storage facilities will ultimately be vital for the viable use of $H_2$ as a major energy carrier. Currently, only reciprocating compressors work for these applications, and they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with $H_2$, such as that being developed in this project, could result in more cost-effective and reliable $H_2$ compression. The project is taking an excellent approach to achieving its objectives. It is using state-of-the-art centrifugal design methods, including FEA. It is incorporating the currently available state-of-the-oil art bearings, seals, and other components to improve the probability of success while focusing on the design and materials for the rotors needed to incorporate the breakthrough rpm into the design. The project includes close attention to the material selection relative to required strengths and $H_2$ embattlement. Testing of components in $H_2$ is an integral part of the project. Building a single-stage prototype for rigorous testing and evaluation, including operating it with $H_2$, will yield critically valuable results. The project includes collaboration with Air Products, which has extensive $H_2$ compressor experience as well as centrifugal compressor experience

all under commercial use conditions. It is also doing work at Texas A&M University and SRNL on testing materials in $H_2$ environments. Finally, it is getting advice from SNL and Dr. Sofronis at the University of Illinois, who are arguably the best in the world relative to material issues in $H_2$ environments. The testing and choice of aluminum for the rotors appears to be resulting in a much lower costs compared to the use of stainless steel to withstand the $H_2$ environment. Very good progress has been made and the results to date are very encouraging.

### Project weaknesses:

- There is no clear pathway to commercialization.
- It is not certain that the project will meet the reliability assumptions and hence eliminate the need for compressor redundancy.
- This reviewer is not sure that exploring titanium components with Texas A&M is useful work for the project because aluminum rotors seem to work acceptably.
- Durability testing should be included if the design passes the functional validation testing. While the project has an excellent group of collaborators and researchers, they almost appear to be included as an afterthought. That is, this work appears to be lagging behind the design and construction of the one-stage demonstration, indicating that they started work after the critical decisions were made and had little input to this part of the project even though that is the role of this work. Of course, in this case, late may be better than never, depending on how the system validation and durability test turn out.
- No testing of the compressor in the presence of $H_2$ has been reported. Assessment of $H_2$ embrittlement of materials is done through an inadequate test. In fact, no assessment of the behavior of chromoly shaft steel and the Nitronic 50 material to be used for the enclosure has been reported. Given the significance, magnitude, and variability of the compressor cost, the two compressor projects involved in the sub-program need to be given priority and carefully administered and reviewed.
- The widespread use of pipelines for $H_2$ transport is not likely to occur until $H_2$ has made considerable penetration into the transportation market. Recent analyses of $H_2$ delivery indicate that pipeline compression is only a relatively small part of the cost of delivering $H_2$. There are other more pressing $H_2$ delivery issues that need to be resolved, such as refueling station compression costs and $H_2$ storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. The projected cost of the centrifugal compressor under development in this project is $4 million (for 240,000 kg/day). This is about equal to the current (2010) costs in the recently updated and posted HDSAM Delivery Model. The Program is targeting to reduce this cost. It would be better if the design and cost estimate of the complete commercial size compressor were to be reviewed and updated based on the results of the single-stage prototype testing. Collaboration with a commercial producer/supplier of centrifugal compressors and possibly a commercial producer/supplier of very large $H_2$ reciprocating compressors would add strength to this project team and improve on the final design of the compressor.
- There are no major weaknesses.
- This reviewer felt that there are no weaknesses.

### Recommendations for additions/deletions to project scope:

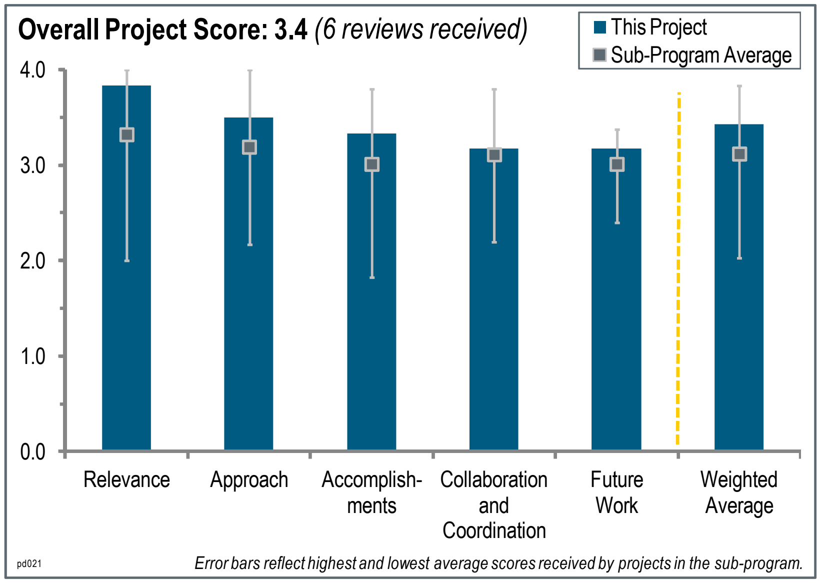
- This project should be brought to a conclusion and Concepts NREC should be encouraged to license the concept to organizations that are interested in funding its commercial development.
- The project team should extend industry collaboration, if possible, to ensure market relevance.
- Funding should be included for validation and durability testing if the system passes the design validation tests.
- This reviewer has no recommendations at this time, other than to review the titanium work with Texas A&M.
- The project should assess the overall system and component behavior through operation in the presence of $H_2$. The team should seek out expertise at SRNL, SNL, or the National Institute of Standards and Technology.
- It would be better if the design and cost estimate of the complete commercial-size compressor were reviewed and updated based on the results of the single-stage prototype testing. Collaboration with a commercial producer/supplier of centrifugal compressors and possibly a commercial producer/supplier of very large $H_2$ reciprocating compressors would add strength to this project team and improve on the final design of the compressor. It is not clear that the work being done on coatings is of significant value, because suitable and reasonably priced materials of construction have been identified.

## Project # PD-021: Development of High Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery

Don Baldwin; Lincoln Composites

### Brief Summary of Project:

To reduce the cost of near-term transportation of gaseous hydrogen ($H_2$) from the production or city gate site to the fueling station, this project aims to design and develop the most effective bulk hauling and storage solution for $H_2$ in terms of cost, safety, weight, and volumetric efficiency. A tank and corresponding International Organization for Standardization (ISO) frame will be developed that meet or exceed U.S. Department of Energy (DOE) low-cost, high-volume goals and can be used for the storage of $H_2$ in a stationary or hauling application.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.8** for its relevance to DOE objectives.

- High-pressure trucks are a critical component of reducing $H_2$ delivery costs.
- This project has good relevance for the early stage of the transition to $H_2$. It would be necessary to make this point by contrasting this option with liquid, pipelines, and cold-compressed delivery.
- Lincoln Composites has tackled most of the barriers and has received special permits for the new, larger systems. The targets focused on near-term cost and weight look to be completed. Technical targets were met, but they are dependent on the cost of the carbon fiber, which is outside the project's scope.
- Unless this reviewer is mistaken, tube trailer delivery is still expected to be the most cost-effective means of delivering $H_2$ until significant market penetration occurs. Therefore, lowering the cost of this means of delivery is critical to enabling the initial stage of market penetration beyond limited fleet vehicles such as forklifts. Therefore, this is a very relevant project with a clear focus and goal.
- The relevance of this project has increased in recent years because reducing the high cost of compression at the station could be achieved by utilizing pressurized truck tube delivery. It is essential for the DOE Hydrogen and Fuel Cells Program to have a viable infrastructure.
- Carbon fiber composite storage manufacturing technology is essential technology for 21st century mobility for onboard vehicle storage, and hopefully it will soon be more widely seen as proven technology for a wide range of $H_2$ storage applications: bulk $H_2$ transport, zero-emission consumer products, and cascade fueling of high pressure vessels (a necessary component of the local $H_2$ fueling station). Low-cost carbon fiber composite pressure vessel technology for mass production is employed today for worldwide use of 5,000 psig paintball cartridges, and translucent liquefied petroleum gas LPG containers developed mass-market end-user small appliances in Europe. Carbon fiber technology began with military and aerospace applications, but the manufacturing technology will serve high-volume consumer applications, $H_2$ fueling stations, and transport applications. The use of composite $H_2$ vehicle storage tank technology in stationary or transportable pressure vessel applications makes a lot of sense from an economic and physical property of material perspective. For many applications, compared with incumbent steel pressure vessel technology, composite storage systems can contain $H_2$ at higher pressure with a lower container mass, lower material cost, and lower manufactured cost. Steel pressure vessel technology has a long commercial history; the introduction of composite pressure vessel technology is an upset to the steel market that is being stressed by the material properties of supercritical $H_2$ and steels. Over the past decade, carbon fiber composting pressure vessels have been used for numerous fleets as

overhead compressed natural gas (CNG) bus fuel tanks with an excellent service record, and smaller, more limited scope composite carbon fiber storage tank transport projects have received U.S. Department of Transportation (DOT) special permits. After extensive testing and work by the principle investigator (PI) on this project, DOT approvals will result in new "American Built" commercial tube trailers and ISO shipping package design. These new products can be used in the United States to carry renewable $H_2$ to fueling stations and throughout the world to support U.S. Department of Defense requirements, such as the transport of compressed helium and $H_2$ to forward bases.

### Question 2: Approach to performing the work

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

- The approach includes good work related to developing and testing large vessels.
- The approach is good, and it is clearly explained on slide 6.
- The approach is well defined, and has some adjustments in the construction and environmental testing of loads. Trade studies on 350 bar is the next logical step. The team could stay within the limits of the existing frames while dropping the cost per kilogram of $H_2$ delivered.
- Lincoln Composites has carried out a systematic project to increase capacity and control costs to produce an optimal design for $H_2$ transport. The ISO-compatible design should allow the wide adoption of tanks.
- With the goal and focus clearly defined, it would be difficult for a team with this experience to not come forward with an excellent approach, and the presentation supported this view. Every issue, future or past, seemed to be in the plan, and the team seemed to be making excellent adjustments as it progressed.
- This project is a large step forward and will allow for the use of these carbon composite commercial products in $H_2$ supply and distribution projects that will support the deployment of fueling stations to support the early pre-commercial fleets of fuel cell electric vehicles (FCEVs) that will be deployed beginning in 2015. The PI and DOE project management have done an excellent job of identifying barriers, including commercial barriers and fear-, uncertainty-, and doubt-related code barriers that limit the current pressure rating at 3,600 psig. There are a number of physical property aspects of $H_2$ that are not well understood and are still subject to some mystery and commercial intelligence. From a commercial perspective, fear, uncertainty, and doubt will manifest as a perceived threat to future business and efforts will be made to lobby against the progress of an $H_2$ economy. From a code perspective, fear, uncertainty, and doubt sometimes manifest as a need to keep safety levels at the highest threat levels.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project meets many of the targets.
- While the lack of commercial pull to take this technology to the next pressure level of 350 bar (5,000 psig) is limiting the potential for higher payloads today, the out-of-the box thinking has resulting in a >5 tube trailer design that contains 26% more $H_2$ than the four-cylinder ISO frame model that was initially the focus of this project.
- Excellent progress has been shown toward meeting cost targets; however, the stated impossibility of measuring market size is a function of lack of market demand. The challenge is determining how to meet the market demand while justifying the business models.
- Production of tanks and DOT approval are significant milestones for the Hydrogen Production and Delivery sub-program.
- It is unfortunate that this project will not include the development of a 350-bar trailer.
- The presentation made a strong case for clear, consistent progress in the evaluation of tanks and qualification testing. A good case was also made for the decision to shelve the 350-bar system until a sounder business case could be made.

### Question 4: Collaboration and coordination with other institutions

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

- It seems like collaborations are sparse. One reviewer wonders if the researchers could benefit from working with other institutions for improved vessel design, construction, etc.
- The PI and project team have worked well with DOT and ASME to help establish precedence for commercial tube trailers using carbon composite pressure vessels.
- There has been close collaboration with the American Bureau of Shipping to meet the regulations and standards. The team is also working with DOT on special permits.
- Lincoln Composites has worked well with regulatory agencies to obtain a permit for the Titan trailer.
- The collaboration with DOT was good. Further collaboration is recommended with the potential customer base.
- The presentation made it clear that collaborations are under way in terms of vessel qualification testing (American Bureau of Shipping, DOT), and that collaborations will be required for system components such as fire systems, valves, and manifolds. Planned collaborations and coordination in the future work were unclear. However, this group has considerable experience, and it is unclear how much further collaboration is necessary for the team to meet the program objectives and goals. Everything seems to be well on track and on a good schedule.

### Question 5: Proposed future work

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

- The >5 tube trailer concept should be submitted to DOT for approval. Continuing development of the improved fire protection systems is a good focus for future work.
- The team should pursue future work with Canada and work to increase compatibility to make the project more cost effective. It should also add laboratory space to study pressurized effects with polymers and supporting equipment.
- Titan 5 and Titan 5+ designs are a good extension of current technology. Achieving 350 bar would be nice, but Lincoln Composite's market-based decision not to seek approval was a good one.
- Everything necessary for qualification and licensing seems to be taken care of or in the plan. The issuing of a special permit by DOT supports this conclusion. If the investigators are considering adding smaller tanks on either side of the larger bottom tank, another question might be how large of a tank could be put up the middle of the four-tank upper array.
- The future work is fine, but it would be useful to also include specific actions and benefits to the project. Also, the cost studies are in the approach section, but they were not highlighted in the future work. The cost study would be an important task for this project.
- It is not clear if cancelling the 5,000 psi effort negates the potential advantages of this concept. The whole point, as this reviewer understood it, was to minimize truck and driver cost by increasing the amount of $H_2$ delivery per trip. This reviewer wonders if the PI can claim good progress toward achieving DOE goals at 250 bar. Abandoning the 350-bar effort due to uncertainty in future market demand seems unreasonable. If everything Lincoln Composites does is ruled by future market viability, it is unclear why the company accepts DOE money. This reviewer wonders if it should pursue the work on its own.

### Project strengths:

- This project features an experienced team and good infrastructure.
- The project is building on an already available commercial product for CNG.
- These new "American Built" commercial compressed gas transportation products can create jobs and be used to bring small, centralized $H_2$ production to local distribution points. Another strength is the distribution (delivery) technology that enables the deployment of fueling stations to support the early pre-commercial fleets of FCEVs.
- The project benefits from market pull from natural gas markets, resulting in a very high degree of leverage of DOE dollars. Lincoln Composite's expertise and experience have contributed to the success of this project.

- This project features an experienced company; a clear focus on the most cost-effective design; evidence of good, clear decision making; an excellent approach and plan; good accomplishments; experience with DOT licensing requirements; and a special license.

### Project weaknesses:

- As this reviewer understands it, the project will now focus on qualifying components for 250-bar compressed $H_2$ storage. It would seem that this would be well-known technology.
- Commercial deployment and investment in new $H_2$ distribution systems, including high-pressure tube trailers, is quite limited due to the lack of short-term vehicle volume at the dispenser to cover station development costs. This lack of current commercial market for a 350-bar (5,000 psig) tube trailer has limited the ability of the project sponsor and the PI to justify the investment in certification of 350-bar tubes for this transport application.
- Cost-savings opportunities may be limited. This reviewer wonders if more materials testing and fatigue testing should be included. While monitored service is a proven technique for testing on the fly, it is unclear whether $H_2$ could afford a slip; that is, one catastrophic failure could prevent this technology from gaining acceptance and penetrating the market. There appears to be very little concern with the supporting materials of the manifolds, gauges, valves, etc.
- The project needs to align the future work with the approach outline and provide further details regarding the approach used to estimate the results that are compared to the DOE targets. In particular, the cost estimate needs further explanation.
- This project has no weaknesses.

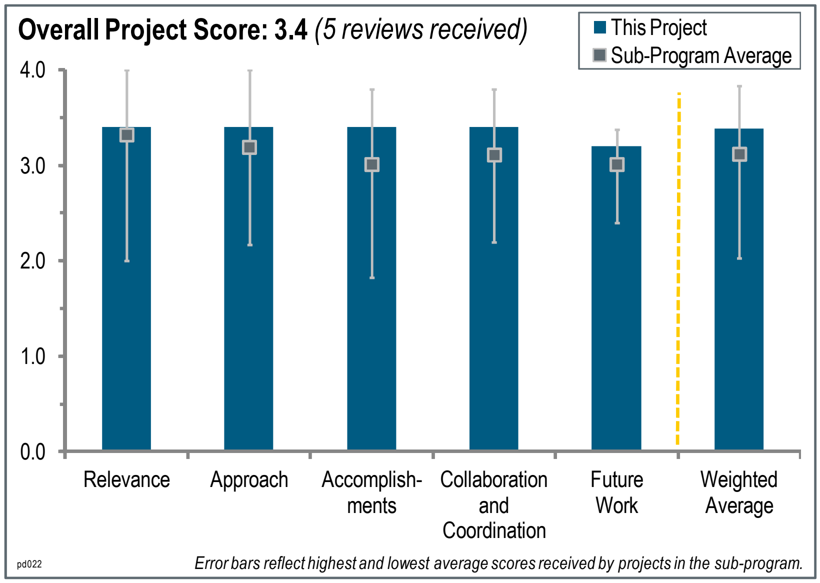
### Recommendations for additions/deletions to project scope:

- Leaving the 350-bar option in the project shows the potential for future developments, even though there is no current market pull to higher density for distribution.
- The team should expand qualification testing of components and assembled systems, including safety systems.
- Investigators should add further interface with potential customers to define the market. They should also add the details of the cost study, and add the lessons learned from the existing CNG commercial trailer experience.

## Project # PD-022: Fiber Reinforced Composite Pipelines

Thad Adams; Savannah River National Laboratory

**Brief Summary of Project:**
The objectives of this project are to provide test data to support a technical basis for fiber-reinforced polymer (FRP) pipelines in hydrogen ($H_2$) service and to integrate FRP pipelines into ASME B31.12 Hydrogen Piping and Pipeline Code by 2015. A proposed demonstration will facilitate codification and public acceptance and provide a test case for permitting. The fiberglass pipeline will serve as a test and surveillance facility and as a final proof-of-concept for FRP pipeline in $H_2$ service. The facility will have an integrated educational component for the public.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- FRP has the potential to significantly reduce pipeline costs.
- The project is extremely relevant. It assesses the structural integrity, safety, and reliability of FRP pipelines.
- This project is bringing together ASME and industry pipeline companies to demonstrate the viable use of FRP pipelines to transfer $H_2$. The goal is to get FRP into ASME codes by 2015.
- Steel pipelines for $H_2$ transport have high costs due to embrittlement concerns and the cost of installation due to welding joints. This potentially offers a lower-cost solution.
- Because FRP pipelines could dramatically lower the cost of installing pipelines and delivering $H_2$, developing test methods and data for the qualification of FRP pipes for $H_2$ service is critical to the longer-term goals of the DOE Hydrogen and Fuel Cells Program. This project has identified the critical issues, experiments, and priorities, and is conducting the required tests.

### Question 2: Approach to performing the work

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

- The project includes excellent fatigue testing results for FRP.
- The project has a well-defined goal of incorporation into codes by 2014. Tests appear to have been well planned and executed.
- This is a decent effort at getting ASME codes to be revised for FRP pipe.
- The project relies on burst testing, as well as fatigue testing of flaws and unflawed pipelines (see slides 6–9). Also, the project involves a large coordination/collaboration effort (see slides 10–13).
- The team has identified potential failure modes and materials issues and has conducted a series of tests to verify the ability of these materials to meet the conditions of service. The team is collaborating with ASME and others to better identify the test methods and data that would be required to qualify a material and design for service. This includes a review of existing FRP pressure pipe codes and standards and workshops on $H_2$ service.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- It appears that the project barriers and objectives are being met.
- The workshop with ASME was a good step toward FRP approval.
- The team has made good progress concerning getting codes in line, fatigue testing, flaw tolerance, etc.
- The project team has identified issues, experiments, and priorities. The team is conducting tests in accordance with its established priorities and is clearly making significant progress. Experiments current, past, and future all line up in a nice, logical sequence.
- The comparison in the behavior of the flawed and unflawed pipelines is a significant result. At this point, the third bullet item on slide 8 is hard to follow: it is unclear whether it implies that the unflawed sample is less durable than the sample with fatigue damage. In addition, the identification of how the failure progresses through fiber delamination from an inserted flaw is a significant milestone of this project.

### Question 4: Collaboration and coordination with other institutions

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

- The collaboration with ASME on B31.12 helps speed the process for codes and standards. Looking at other standards is an excellent approach to making sure that FRP will be accepted.
- This project features good work with ASME.
- Good cooperation with partners on the South Carolina demonstration project.
- The collaboration with ASME is important and should continue.
- This is one of the best projects for interactions and collaborations. In the case of this project, it is essential that the investigators interact with designers, code setting organizations (e.g., ASME), and regulators to determine the data they would require to design, qualify, and permit this type of material for $H_2$ service, and they have done an outstanding job of working with collaborators to identify issues and prioritizing tests.

### Question 5: Proposed future work

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

- A new demonstration project will provide some real-time data on $H_2$ pipelines.
- Full-scale service emulation-type long-term testing is an excellent approach for a material that has not yet seen real $H_2$ service, and there is little if any data on the kinetics of any potential failure mode.
- The team needs real test data on FRP performance. One reviewer suggests that the team try both and compare data for trench less and trenched installations. The team may want to incorporate active monitoring to prevent third-party damage. B31.12 ASME code needs to include FRP.
- The sub-program needs to carefully define what it can learn from a large-scale pipeline demonstration. This would be a high-cost project. A cost-benefit analysis needs to be carried out to ensure a good return for DOE's investment. Work that contributes to ASME approval should be the top priority. This reviewer is not sure that installing a pipeline is good for public opinion of $H_2$.
- The proposed future work was described on slides 14–19, but the focus was mainly on interactions for codes and standards and demonstration. A description of what questions need to be answered was missing. Slide 21 mentions "perform long term stress rupture tests for flawed FRP samples," but this statement is rather vague. No specifics were mentioned as to what new information is sought relative to what has already been reported.

### Project strengths:

- The departure from conventional steel pipe offers a chance for a lower-cost solution.
- The spooling of pipeline reduces insulation cost. Stress cycle testing looks to be a strength of this project; however, the data may need to be run beyond the 8,000 cycles. Partners for demonstration are identified.
- Strengths of this project include collaborations with other laboratories, codes setting organizations, and regulators to identify test needs and priorities; consistent progress in testing in accordance with priorities and

goals; and coordination with local government for the full-scale $H_2$ pipeline testing facility. It is nice to see this material being subjected to the same kind of testing scrutiny that other materials have to go through to be accepted and qualified for use in this or similar service conditions.
- The project is moving in the right direction. The focus on how a flawed pipeline responds to fatigue loading is clear, and the related results so far on the failure mechanism are significant. The leadership of G. Rawls to steer the project in this direction is notable because this is the first time that a serious structural integrity assessment of FRP pipelines has been reported in a DOE Hydrogen and Fuel Cells Program Annual Merit Review meeting since 2006.

### Project weaknesses:

- The team should provide more definition of additional fatigue testing.
- The team needs to perform a long-term stress rupture test on FRP that has been installed. The team should also monitor soil moisture and pH.
- The main weakness in the past was the poorly defined requirements for getting materials of this type accepted for $H_2$ service and used by designers. This has largely been solved through the development of collaborations. Now that the criteria are being set and the critical data is being obtained, this reviewer would like to see more tests, including the combined effects of absorbed $H_2$ and water because the pipes will be buried. Essentially, this is included in full-scale testing, but this reviewer would find quantified answers more scientifically appealing.
- Fatigue tests have shown that flawed pipeline life is affected relative to unflawed pipeline life. However, there were no conclusions drawn as to how these results can be used to predict, for example, the remnant life of a flawed pipeline. If FRP pipelines are used for $H_2$ transport, rigorous requirements for life assessment must be used as plans are made to use da/dN versus DK curves to design steel pipelines. The project's approach needs to be expanded by identifying the parameter space of the potential failure modes that need to be systematically investigated. Certainly, steps toward this direction have already been taken (e.g., how failure progresses from the presence of a flaw), but potential tests to understand and evaluate the structural integrity, for example, of the liner/reinforcement interface, have not been identified.

### Recommendations for additions/deletions to project scope:

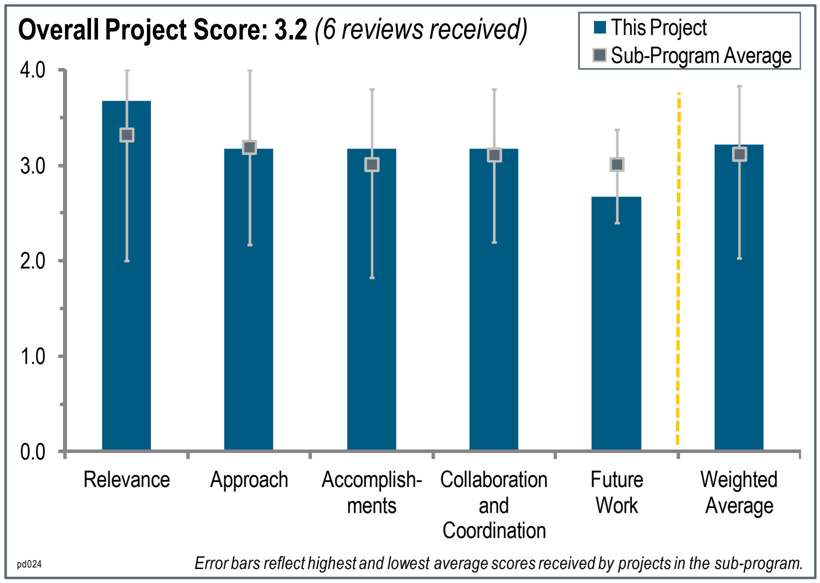
- The team needs to study the trench-less process that most gas industry companies are now using.
- The team should continue with the demonstration project in South Carolina.
- More funds should be made available so that the testing matrix can be expanded. Because the pipelines will be buried, this reviewer would like to see more on the combined effects of absorbed water and $H_2$, including on the following topics: (1) whether water absorption will change the permeation rate of $H_2$, (2) whether the permeation of water through the pipe will contaminate the $H_2$, (3) whether the two combined will influence fatigue performance or burst strength. Essentially all of this is included in full-scale testing, but this reviewer would prefer more scientifically quantified answers.
- The project should proceed beyond comparing and quantifying individual test cases (flawed versus unflawed FRPs). The project should start developing conclusive knowledge that can assist with predicting pipeline remnant life and safety. This can be accomplished by thoroughly identifying and quantifying potential failure mechanisms.

## Project # PD-024: Composite Pipeline Technology for Hydrogen Delivery

Barton Smith; Oak Ridge National Laboratory

### Brief Summary of Project:

The objective of this project is to address the barriers of: (1) pipeline capital cost, reliability, and leakage; (2) the hydrogen ($H_2$) compatibility of pipeline materials; and (3) technology acceptance. The objectives specifically for the 2012 fiscal year are to: (1) complete high-pressure cyclic fatigue and stress-rupture tests of fiber-reinforced polymer (FRP) pipelines, (2) reassess FRP pipeline capital cost, and (3) collaborate on ASME codes and standards acceptance.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.7** for its relevance to DOE objectives.

- The project is extremely relevant. It addresses composite pipeline technology for $H_2$ transport.
- FRP has the potential to significantly reduce pipeline costs compared to steel. ASME certification is a necessary step toward the incorporation of FRP in $H_2$ use.
- Because FRP pipes could dramatically lower the cost of installing pipelines and delivering $H_2$, developing test methods and data for the qualification of FRP pipes for $H_2$ service is critical to the longer-term goals of the Program. This project has identified the critical issues and is conducting the required tests.
- Pipelines are an important part of the chicken-and egg-problem with infrastructure and autos. Without the pipelines, cars might not be part of the picture. Showing a long-term promise for reduced cost as the infrastructure grows is an important motivator. However, localized production may alleviate some of the long-distance transport issues.
- The barriers and technical targets are well designed. Capital cost and technical targets are defined by regulatory agencies. The codes and standards are on pace to be met. New cyclic fatigue testing is proceeding. The ASTM D2143 test is under consideration. It appears that it will meet the DOE 2020 goals.
- Pipeline transport and distribution of $H_2$ is a potentially viable and cost-effective delivery pathway. Current analyses done for the DOE Hydrogen and Fuel Cells Program (the Program) show that pipelines are the low-cost pathway to transport large amounts of $H_2$ long distances (greater than about 50 miles). Such transport is likely from a central $H_2$ production plant to terminals at or near city gates. This project is focused on utilizing fiber composite pipelines to reduce these transport costs compared to steel pipelines, which is necessary to achieve the Program's delivery cost targets. These composite pipelines might have additional advantages over steel if they can be engineered to include sensors for pipeline integrity. However, it is important to remember that the cost to transport $H_2$ from central production plants to terminals at city gates if the distance is less than 50 miles is only a small fraction of the total cost of $H_2$ delivery. The widespread use of pipelines for $H_2$ transport and distribution is not likely to occur until $H_2$ has made at least considerable penetration into the transportation market. Recent analyses show that distribution and service pipelines are more costly for $H_2$ distribution than recently developed higher-pressure tube trailers. In addition, urban areas may be reluctant to embrace a $H_2$ pipeline infrastructure due to safety concerns and/or the disruption its construction might cause. However, pipelines for $H_2$ transport from central production facilities to terminals might be far easier to employ and potentially the lowest-cost option for this part of a $H_2$ delivery infrastructure. As a result, pipeline transport might be very useful at even relatively low market penetrations. Although recent analyses show that distribution and service pipelines are more costly for $H_2$ distribution than recently developed higher-pressure tube trailers, if the cost of these small

diameter pipelines could be dramatically reduced, they might become the preferred option for $H_2$ distribution in urban areas once $H_2$ has become a major energy carrier.

### Question 2: Approach to performing the work

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

- The project features good experiments, and it is well designed and focused on critical issues.
- The project has an excellent work scope and a very in-depth review of the progress.
- Cyclic testing under $H_2$ pressure is vital to ensuring pipe operability in an $H_2$ environment.
- This is a very small project, so reviewers need to be realistic about what can be accomplished. The project helps keep an eye on the future (i.e., polymer pipelines), without using a lot of resources needed for breakthroughs in limiting technologies, such as clean distributed sources of $H_2$ and improved fuel cells for consumer vehicles.
- The project aims to understand the durability, integrity, and safety of FRP pipelines through high-pressure fatigue testing. The project also investigates the $H_2$ permeability of the FRP pipelines. Certainly both durability and permeability are important and need to be understood and quantified. The issue is whether investigation of fatigue alone is enough to ascertain the reliability of the FRP pipelines. One reviewer wonders if there are any other failure modes.
- This year's work is focused on testing available composite pipe for pressure and temperature cycling, plus the impact of blow down. It also includes direct measurements of $H_2$ leakage after the cycling and blow down experiments. These are likely the most important and practical tests that can help start to qualify composite pipeline for $H_2$ service. The project now appears truly engaged with ASME to start to develop codes and standards for composite pipelines for $H_2$ transport use, which is very important. It is vital to work with ASME and stakeholders to tabulate a full list of testing that needs to be done to fully qualify composite pipeline for $H_2$ service and to run those tests. This is part of the future work. The team is performing a continued evaluation of composite pipelines and materials using the recently improved test equipment. This is important to verify acceptable leakage/permeation rates for $H_2$ pipeline service.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Testing has shown no real problems.
- The project is making acceptable progress, considering the low (but appropriate low) funding. The results are not awe inspiring, but they seem to be solid, and if there was a huge problem with the fiber-reinforced pipe, the authors would have found it.
- The team has achieved good accomplishments over the past year. The team may need to converse with industry vendors that are doing trench-less installation to determine current cost.
- Considering the very small amount of funding provided for this project in 2012, very good progress has been made. Testing on available composite pipe for pressure and temperature cycling plus the impact of blow down has been done with very promising results. Direct measurements of $H_2$ leakage after the cycling and blow down experiments were also done. These tests showed no leakage from the pipe itself, very low permeability through the pipe, and some measurable but low leakage through the pipe joining fittings. These measurements are very encouraging and point to the joining fittings as the area for more careful and accurate measurements of leakage issues in the future. The team is also continuing to evaluate the $H_2$ permeability of composite pipelines and materials using the recently improved test equipment. This is important and showing promising results. The level of dependence on temperature and $H_2$ pressure is also being determined.
- Slide 17 states that no blistering or delamination of liner was evident during visual inspection of the liner following pressure blow down. Slide 18 reports that quality assurance testing of the pipeline was examined by Fiberspar, and no loss of performance capabilities of the tested pipeline was identified. On slide 16, a large leak is reported at a pressure transducer port due to a bad seal. On slide 20, an updated capital cost for the FRP pipeline is reported, and slide 21 states the FRP pipelines provide a 15% overall cost reduction in comparison to steel. A summary of the estimated costs is provided on slides 23 and 24. Lastly, slides 26 through 29 provide information from solubility and permeability tests.

### Question 4: Collaboration and coordination with other institutions

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

- The project features good coordination with ASME and others. The team should be sure to include polymer producers.
- This project has widespread collaboration that includes the principle composite pipeline manufacturers, polymer manufacturers, Savannah River National Laboratory (SRNL), the DOE Pipeline Working Group, and ASME.
- Based strictly on the presentation, this reviewer has rated this as fair. A talk by one of the collaborators later in the day did a better job of showing symbiosis and saying how those two projects fold together. Slide 4 lists many collaborators, and that is fine. However, one reviewer was left wondering what the key and critical collaborations are, and whether the interaction is superficial or represents real engagement with industry.
- Coordination with industry standards development organizations is part of the oversight in the project. Industry input on the performance of existing pipelines provides good information with which to compare. Another reviewer suggests reviewing the use of off-shore connector technology for new construction systems.
- The collaboration with SRNL is extremely important in view of the work reported in the project number PD-022. Collaboration with ASME is also required to make sure that the underlying fundamentals of codes and standards specifically for FRP-composite pipelines are properly addressed.

### Question 5: Proposed future work

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

- The future work should be tailored to provide the data needed by ASME.
- The future work plan appears to be good, but the focus did not appear to be as sharp on clearly defined barriers as an "excellent" rating would imply.
- The proposed work has nice general words concerning communication and sharing with stakeholders. However, it would have been more impressive to say the collaborators voted on or committed to the future direction to address a particular topic, rather than to mention an example of what might be looked at during the next year.
- Cycle testing of pressure cycling needs to be considered to reflect the actual pipe cycling. A possible test plan needs to be developed to study both pressure and temperature cycling. The cycling test of a limited number of cycles needs to be expanded to truly represent existing $H_2$ pipeline cycles and temperatures. The investigators need to understand why the properties of the polymers improve as a system.
- The future plan is well thought through. It includes working with ASME and stakeholders to review all testing that has been done, establishing the full list of testing that needs to be done to qualify composite pipelines for $H_2$ service, and defining the research needed to close the gaps between the work done and what is left to do.
- Slide 30, which is the only slide that reports future work, is very general and did not reference any specifics. Stated collaborations for acceptance of FRP pipelines for $H_2$ delivery do not count for future technical work. Slide 30 proposes to "identify research that needs to be completed to close knowledge gaps and establish plans to conduct research." This is extremely important. In general, slide 30 does not outline a concrete research plan appropriate for a project that has been in progress for a number of years.

### Project strengths:

- The revised cost estimate of the design appears to be realistic.
- The speaker had a clear and sincere manner in describing the work, especially during the question and answer session.
- The project features well-defined materials and issues with targets coming into focus due to DOE lead collaborations with industry, designers, code-setting bodies, and regulators. There is a good plan for the transition to market and cost estimates.
- A strength of the project is its focus on FRP pipelines, which can be used as an alternative to steel pipelines for $H_2$ transport. The project could serve to springboard the investigation of $H_2$ interactions with polymers and composites in general, but there is no such direction besides the permeability studies.
- Pipeline transport and distribution of $H_2$ is a potentially viable and cost-effective delivery pathway. Current analyses done for the Program show that pipelines are the low-cost pathway to transport large amounts of $H_2$ long distances

(greater than about 50 miles). This project is focused on utilizing fiber composite pipelines to reduce costs compared to steel pipelines, which is necessary to achieve the Program's delivery cost targets for this application. Pipelines for $H_2$ transport from central production facilities to terminals might be reasonable to employ and could potentially be the lowest-cost option for this part of an $H_2$ delivery infrastructure. As a result, pipeline transport might be very useful at even relatively low market penetrations. This year's work on this project is focused on testing available composite pipe for pressure and temperature cycling, plus the impact of blow down. It also includes direct measurements of $H_2$ leakage after the cycling and blow down experiments. These are likely the most important and practical tests that can help start to qualify composite pipeline for $H_2$ service. The results to date are very promising. The project now appears to be truly engaged with ASME to start to develop codes and standards for composite pipelines for $H_2$ transport use, which is very important. This project has widespread collaboration that includes the principle composite pipeline manufacturers, polymer manufacturers, SRNL, the DOE Pipeline Working Group, and ASME. The future plan includes working with ASME and stakeholders to review all of the testing that has been done, establishing the full list of testing that needs to be done to qualify composite pipelines for $H_2$ service, and defining the research needed to close the gaps between the work done and what is left to do.

### Project weaknesses:

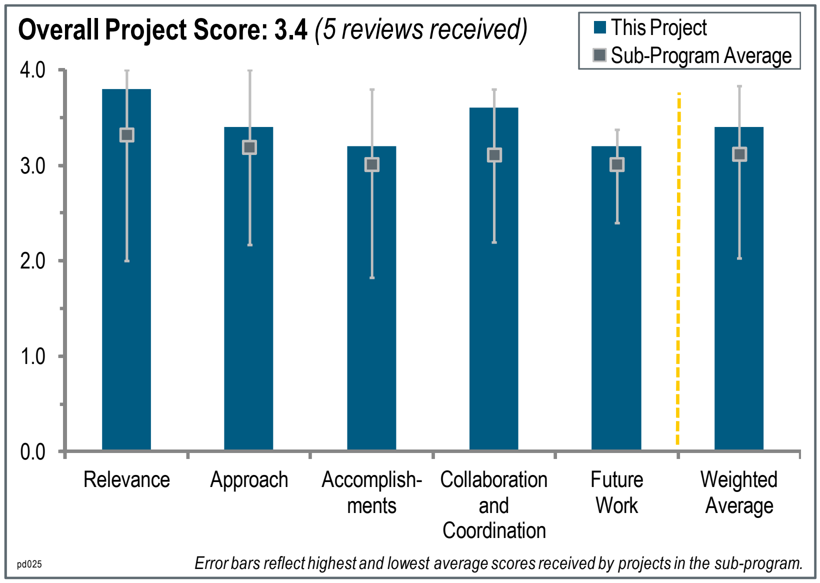
- The amount of leakage due to permeation seems high (per mile per year). This reviewer understands that it is small as a percent of the delivered amount. Perhaps a direct comparison to steel pipeline losses (industry norm) would make this reviewer feel more comfortable.
- It is important to remember that the cost to transport $H_2$ from central production plants to terminals at city gates if the distance is less than 50 miles is only a small fraction of the total cost of $H_2$ delivery. Thus, the priority of this project needs to be weighed against the priority of other projects in a limited funding environment. The project now appears to be truly engaged with ASME in starting to develop codes and standards for composite pipelines for $H_2$ transport use. It is vital that the investigators work with ASME and stakeholders to tabulate a full list of testing that needs to be done to fully qualify composite pipeline for $H_2$ service, and to run those tests. This is part of the future work.
- There are no clear weaknesses, but this reviewer would like to see the testing matrix expanded to address the combined effects of absorbed water and $H_2$, because the pipes will be buried. Issues this reviewer would like to see answered include the following: (1) whether water absorption will change the permeation rate of $H_2$, (2) whether the permeation of water through the pipe will contaminate the $H_2$, and (3) whether the two combined will influence fatigue performance or burst strength. Essentially, this would be covered by a thoroughly instrumented demonstration project, but this reviewer would like to see the effects quantified.
- A central weakness of this project is that it has not identified the parameter space of the potential failure modes that need to be systematically investigated. Cycling pressurization and depressurization may be recommended by ASTM, but they may not be sufficient to capture the operation of potential failure mechanisms of FRPs in the presence of $H_2$. By way of example, if the current project's approach and strategy to assessing the integrity of the FRP composite pipelines is applied to the case of a steel pipeline with no internal flaws through pressurization and depressurization, the steel pipeline shall never fail and hence it shall be deemed safe. On the other hand, it is known that the issue of hydrogen-induced fatigue acceleration—which is investigated using an appropriate laboratory test-piece—arises in the presence of a flaw. In other words, safe pressurization and depressurization of the pipeline is not sufficient to assess pipeline reliability. Durability, reliability, and safety must be based on accident and failure scenarios. Such scenarios and their parameter space have not been identified by the project. In addition, regarding the integrity of the liner/reinforcement interface, visual inspection of pressurized and depressurized FRP samples is not enough, nor is it appropriate. If there is a flaw on the liner/reinforcement interface, the team needs to determine how it will behave during the pressurization and depressurization cycle.

### Recommendations for additions/deletions to project scope:

- The scope seems about right. It helps to keep an eye on pipelines and infrastructure.
- The team should include the permeation of water from the outer diameterouter diameter surface in permeation work and examine the effects of water absorption on the permeation of $H_2$.
- The project should justify the continuation of permeability studies. What new information is sought is not clearly stated. At this stage, the technical approach on how the project will achieve its goals and objectives (that is, the safety and reliability of FRP pipelines) is vague, and there are no clear targets and milestones regarding structural integrity. Assessing third-party damage as proposed on slide 30 is an important goal, but no technical approach is listed.

## Project # PD-025: Hydrogen Embrittlement of Structural Steels

Brian Somerday; Sandia National Laboratories




### Brief Summary of Project:

The objectives of this project are to demonstrate the reliability and integrity of steel hydrogen ($H_2$) pipelines under cyclic pressure and to enable a pipeline reliability and integrity framework that accommodates $H_2$ embrittlement. In fiscal year 2012, the project worked to quantify the effects of oxygen ($O_2$) impurities in $H_2$ gas on fatigue crack growth under high pressure and to determine the threshold level of $O_2$ impurities in $H_2$ gas.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The project examines the weak link in steel pipelines and $H_2$ embrittlement.
- Fully understanding the cause and potential ways to inhibit the potential for $H_2$ embrittlement in steel pipe has immediate usefulness to industries that transport $H_2$ by pipe.
- The demonstration of the reliability of steel $H_2$ pipelines for cyclic pressure is paramount to $H_2$ delivery meeting the DOE cost targets of $1–$2 by 2020 and establishing data-driven codes and standards. This project remains critical to the DOE Hydrogen and Fuel Cells Program (the Program).
- This project is clearly aligned with Program goals. Understanding and demonstrating the reliability and integrity of steel pipelines is critical for the initial rollout of $H_2$ infrastructure.
- The overall discussions and objectives of the project are good. The investigators need to expand the study to include the effects of $H_2$ toward the deceleration of embrittlement of welds, and not just the base material. This is especially true because seamless pipes are rare in installed pipelines.

### Question 2: Approach to performing the work

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

- The fundamental approach involves using fatigue crack growth based on pressure cycles of $H_2$.
- The project is well focused; the investigators have conducted thorough research on the subject and actively collaborated with others to determine cause and effect.
- The initial focus on examining stress fractures under varying $H_2$ pressures for X52 is very sound. This reviewer would suggest, however, that the ability to modify temperature is a much-needed added dimension. Also, plans to evaluate other types of steels based on industry recommendations and working with the National Institute of Standards and Technology (NIST) to speed up the evaluation process clearly offer additional benefits to the project.
- The approach is good. The team needs to share with projects currently underway at NIST-Boulder and the University of Tennessee funded by the U.S. Department of Transportation to study the effects of $H_2$ in a range of pipeline steels.
- The researchers have taken a good approach on this project. It included a balanced combination of experimental and modeling work mainly around the objectives of understanding and determining the threshold level of $O_2$ to inhibit accelerated fatigue crack growth.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The team is making good progress in determining how much $O_2$ is needed to slow the crack growth.
- The team is taking a good approach toward understanding the effects of $O_2$ on $H_2$ embrittlement. The researchers must explore other models.
- The achievement to date is very good, but this reviewer would have like to have heard if the theory was vetted by organizations that operate high-pressure $H_2$ pipelines to determine if they support or challenge the concepts presented.
- Identifying the positive effects of introducing small amounts of $O_2$ at specific load-cycle frequencies into the pipeline is showing significant progress. This information opens the door to investigate whether the effect carries over to other types of steels, and if other gaseous impurities, such as methane, might also have a positive effect by providing a barrier for the exposed area.
- Significant accomplishments have been achieved in this project. Researchers have successfully demonstrated the effects and impacts of $O_2$ impurities on the mitigation of $H_2$-accelerated crack growth on X52 steel. It would be very interesting to extend these studies to other types of higher-strength steels.

### Question 4: Collaboration and coordination with other institutions

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

- This project features good partnerships with the pipeline working groups.
- There is great collaboration among the research team.
- The research is excellent, but it is important to begin collaborating with organizations that install and operate high-pressure pipelines to thoroughly test the concept and validate it with empirical data. This reviewer likes the collaboration with I2CNER. This should be developed further.
- This is a nice assortment of relevant stakeholders. In particular, Exxon Mobil and Secat hopefully will provide for future analysis a variety of steels beyond X52 and X65.
- This project features very good collaboration, with a good mix of participants that include representatives from academia, industry, and national laboratories.

### Question 5: Proposed future work

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

- The future work plans to develop a better understanding of the $O_2$ effects seem well thought out.
- The proposal for future work is light. More rigor should have been given to test the theory with organizations that have a clear interest in this area and can help fund a more rigorous validation project.
- As stated earlier, the plans to access NIST's specialized equipment and to expand to other types of steel are logical next steps.
- The proposed future work on welds is a good approach. This reviewer wonders if the $O_2$ and $H_2$ are combining at the crack to form water or another composition, or if possibly the water is being absorbed by the steel.
- The researcher plans to complete the work around X-65 steel. The expansion of testing higher-strength steels beyond the X-52 studies is a strong plan moving forward in the understanding of $H_2$ embrittlement of steel pipes.

### Project strengths:

- This work could have a meaningful impact on existing and future pipeline designs.
- The project provides an understanding of both theory and practical application.
- The project features a good approach and future plans for inhibitors.
- The project takes a good, fundamental approach toward understanding $H_2$ embrittlement; the experimental work seems to complement the theoretical work.

- The project features a very sound approach and significant accomplishments. There is a good work plan for future work, with the fact that the testing will be expanded to other higher-strength steels. There is also good collaboration with a good mix of partners.

### Project weaknesses:

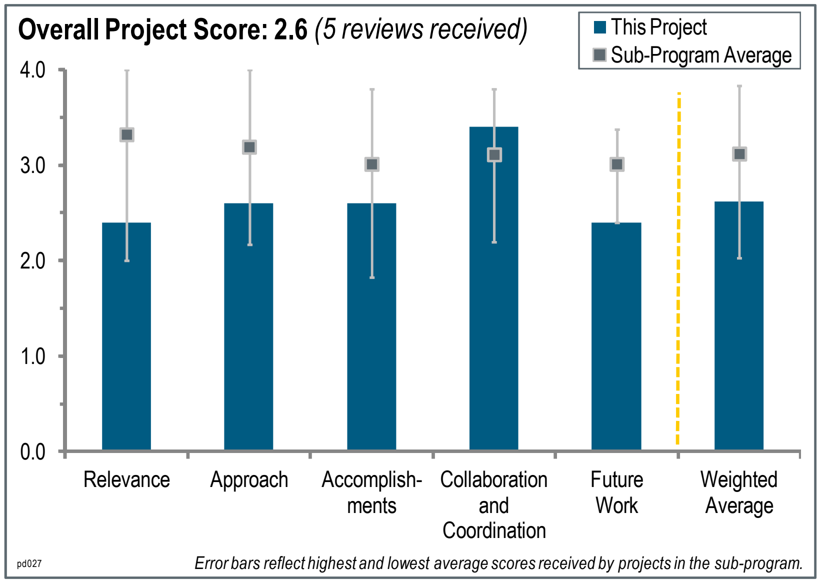
- The project requires collaboration with organizations that have an interest in proving the theories and employing the concept with near-term, funded projects.
- The lack of specialized equipment to test the effects of temperature, pressure, $O_2$ concentration, and frequency all at the same time is an area of weakness.
- This reviewer is not sure if the X52 ERW is low- or high-yield-strength material. This needs to be quantified. The work is stated to be at high-pressure $H_2$. This reviewer wants to know what the actual operating pressure is.
- The team should perform some experiments to understand the role of $O_2$ in $H_2$ embrittlement.

### Recommendations for additions/deletions to project scope:

- The team should push harder, reach higher, and leverage DOE connections.
- The researchers should add the ability to cycle temperature.
- The future work on expanding the studies to other higher-strength steels beyond X-52 is a great addition to this project.

## Project # PD-027: Solar High-Temperature Water Splitting Cycle with Quantum Boost

Robin Taylor; Science Applications International Corporation




### Brief Summary of Project:

The overall project objective is to conduct research, development, and demonstration on the viability of a new sulfur family thermochemical water-splitting cycle for large-scale hydrogen ($H_2$) production using solar energy. More specifically, the overall project goals are to evaluate water-splitting cycles that employ photocatalytic or electrolytic $H_2$ evolution steps, and to perform laboratory testing to demonstrate the feasibility of the chemistry. This past year's objectives were to: (1) complete the optimization of the electrolytic oxidation process, (2) complete the evaluation of the high-temperature $K_2SO_4$ sub-cycle, and (3) perform economic and solar systems analyses.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.4** for its relevance to DOE objectives.

- The cycle seems to be very complex and have significant thermal management issues. This reviewer wants to know what the key advantages of this system are as compared to other chemical cycles.
- The project is aligned with DOE objectives because this work's objectives are to improve the approach to making renewable $H_2$. However, it is unclear how relevant the project is because the team does not identify the performance required of each step in order to meet an ultimate technological/economic goal of roughly $2/kg of $H_2$ production.
- This project addresses the central production of $H_2$ via concentrated solar. It is in an early stage of development and has yet to perform Hydrogen Analysis (H2A) cost modeling to highlight its potential economics.
- Thermochemcial cycles are of great relevance to the DOE Fuel Cell Technologies Program. This is a hybrid process that produces $H_2$ and electricity. It is unclear whether the $H_2$ cost could be significantly reduced if only $H_2$ were produced. This is a very complicated process. Because of it's complexity, this reviewer feels it is highly unlikely that the process will be viable.
- The project is aligned with DOE's goals of $3/kg $H_2$ and >35% efficiency in 2017. However, more specific milestones must be laid out along the way to 2017. It is hard to connect the progress made this year to the assessment of the likelihood of achieving the 2017 goals.

### Question 2: Approach to performing the work

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

- The team has done an impressive job in attacking the different steps in a very complicated process. The researchers did a good job of using modeling to help the understanding of the process, efficiency losses, and sensitivity analysis. Some of the practical aspects should be addressed next.
- The approach is reasonable and well constructed. ASPEN modeling is a necessary and good step.

- It is not clear if this is the best approach because the critical performance requirement needed to achieve the DOE $H_2$ cost goal is unclear.
- This is a very complicated thermochemical cycle. It is unlikely that such a complicated system can be successfully developed for commercial application. The fiscal year 2012 scope focuses on the area that needs the most development. The molten salt work was a good start. It is important to get the viscosity; however, other quantities are required in order to understand the rheology. For example, this reviewer wants to know what type of fluid the molten salt is (Newtonian or Bingham plastic).
- The approach is still using a lot of electricity (high voltage for the electrochemical portion of the system). It would be prudent to perform early H2A analysis to determine the impact of voltage on the economics of the process. In central electrolysis, electricity use is a driving impact, and it would be helpful to know how low targets should be set for electricity use to drive research. The researcher commented that the theoretical voltage for the system is 0.113 V, while the operating voltage is 0.5 to 0.9 V. Thus, overpotential (parasitic losses) account for the majority of the electricity expenditure. The researchers need to determine what the root causes are for the overpotential (e.g., ionic conductivity, electric conductivity, contact resistance, anode activation, cathode activation, anode transport, or cathode transport). Without identifying the source and magnitudes of the overpotentials, the researchers will not be able to efficiently pursue improvements in performance.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- Progress has clearly been made, but this progress does not appear to be tied to the goals.
- It would be good to show the progress of each step in the process broken out to show where the most impact is being made and where the most future potential is. This reviewer wants to know what the storage capacity of the salts for $H_2$ are, and what pressure is needed for getting the $H_2$ into the media.
- The improvement in the voltage is a good step. The current density is very low. This will result in a very large electrolyzer that will be extremely expensive, particularly because the researchers are using Nafion and platinum (both very expensive materials). The researchers must improve the electrochemical performance and they may want to consider a high-temperature fuel cell, such as a solid oxide fuel cell. The use of ASPEN to do some flow sheet analysis is a nice addition from the previous work. The project is using a Nafion membrane but operating the system at neutral or basic conditions. The researchers may be able to significantly decrease costs by using a membrane from an alkaline type of membrane.
- The team fails to make the case for substantial and meaningful performance improvement. Ideally, there would be a set of metrics defined that lead to the ultimate goal of $3/kg and parameters, such as current density, voltage, lifetime, temperatures, capital cost of each component, etc. The project fails to show that the cost goals can be achieved at "current densities <100 $mA/cm^2$." Without simultaneous targets for cell voltage and current densities, it is impossible to track progress toward the ultimate goal. No results of the H2A analysis were presented.
- The project situation appears to be in a similar footing as last year. This reviewer was hoping to see more progress in identifying and tackling the electrochemical portion of the device performance. This reviewer wonders if the research team has adequate electrochemist staffing.

### Question 4: Collaboration and coordination with other institutions

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

- The presentation included a good description of the work from different partners and showed how the parts fit together.
- Collaborators appear to be well integrated into the project.
- There appears to be good collaboration, and the partners' roles seem well defined. The inclusion of a utility company is a nice addition.
- There is limited basis to judge the extent of collaboration.
- It would be desirable to have collaboration with fuel cell stack original equipment manufacturers (OEMs), such as Ballard, for example. While ElectroSynthesis Inc. may provide value to the project, stack OEMs might have a lot of relevant experience that could improve the understanding of this system's performance challenges.

## Question 5: Proposed future work

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

- Technical issues are being addressed, but for this project there should also be an analysis of how manufacturable a concept such as this really is given the complexity of the system.
- The proposed future tasks are reasonable for advancing the various pieces of the technology, but it was not clear if they are the critical steps needed to achieve the DOE cost target.
- It would be interesting to see a chart showing how the work will result in the reduction of cost. For example, the researchers indicate that they need 100 $mA/cm^2$ for their electrolyzer. It would be useful to see how much this would reduce costs.
- Plans for future work are not adequately specific. Specific goals for voltage/current density and performance after 500 hours should be defined as part of the future plans.
- It is unclear if catalyst development is the source of electrochemical performance challenges. This reviewer believes that there are multiple, large impactors to overpotential, and a single line of attack of the problem might be insufficient. Research should first quantify all of the effects on overpotential and should have a comprehensive strategy to tackle the largest contributors. For example, if this was a fuel cell or electrolyzer development effort, the research would have tackled four or five aspects of the reduction of overpotential. Electricity expense for $H_2$ production will be very significant and justifies the need for more effort related to understanding the fundamentals of the electrolysis step.

## Project strengths:

- The basic approach has the potential to meet DOE goals. Overall, the project construction is sound.
- There appears to be a strong team for this project. This project is funded very well. The researchers have developed a way for the process to operate close to 24/7.

## Project weaknesses:

- The thermochemical cycle chosen for development is very complicated. For the large budget and amount of time in development, it seems that progress has been a little slow.
- The overriding weakness relates to project framing. The team needs to clearly state the value proposition associated with this work, and what advances are necessary to achieve that value proposition. The researchers need to clearly state how the specific research activities address the barriers to achieving that value proposition.
- The metrics are not specific enough, and the economic analysis is not presented. Therefore, there is no clear statement of the performance parameters that must be achieved to reach economic goals. Thus, no meaningful assessment can be made of current status, as all parameters must be compared against their target values.

## Recommendations for additions/deletions to project scope:

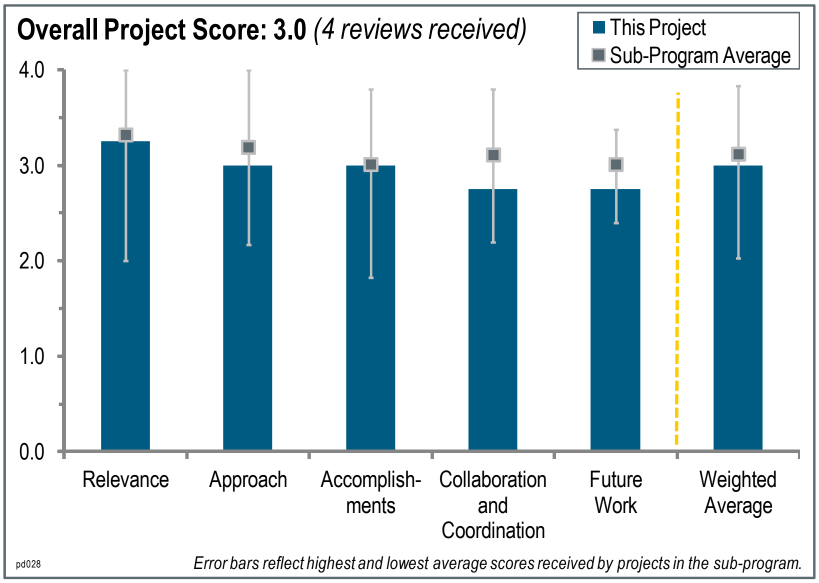
- This reviewer would not recommend supporting research that does not have a clearly defined value proposition and does not address the specific obstacles required to achieve that value proposition.
- H2A analysis needs to be conducted and included in the project. Specific targets need to be established for all parameters, which, when taken together, can be shown in H2A to lead to $3/kg $H_2$. A reevaluation of efficiency needs to be done. An economic sensitivity to current density and voltage needs to be conducted. It is not clear how sensitive the system cost is to cell voltage or to system current density. Much of the economic analysis centers on the electrolyzer. However, it may be that the other system components contribute much of the cost. Consequently, a clear assessment of the balance of plant costs should be conducted.

## Project # PD-028: Solar-Thermal ALD Ferrite-Based Water Splitting Cycles

Al Weimer; University of Colorado

### Brief Summary of Project:

The objective of the project is to develop and demonstrate robust materials for a two-step thermochemical redox cycle that will integrate easily into a scalable solar-thermal reactor design and will achieve the U.S. Department of Energy (DOE) cost targets for solar hydrogen ($H_2$). Research was conducted using three approaches: (1) laser-assisted stagnation flow reactor, (2) on-sun solar reactor, and (3) thermogravimetric analyzer.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.3** for its relevance to DOE objectives.

- This is a simple thermochemical cycle that is part of the long-term Hydrogen Production sub-program plan.
- This project aims to develop and demonstrate robust materials for a two-step thermochemical redox cycle that will integrate easily into a scalable solar-thermal reactor design and will achieve the DOE cost targets for solar $H_2$. The project addresses a number of barriers identified in the Fuel Cell Technologies Program Multi-Year Program Plan, such as High-Temperature Thermochemical Technology, High-Temperature Robust Materials, Concentrated Solar Energy Capital Cost, and Coupling Concentrated Solar Energy and Thermochemical Cycles.
- Solar thermochemical cycles, such as the Hercynite cycle being researched in this project, have the promise of being cost-effective, near-zero-carbon-emitting $H_2$ production options. The Hercynite cycle is a very attractive solar thermochemical cycle because it is a simple two-step cycle and because it operates at temperatures below 1,500°C. These two factors result in this solar approach to $H_2$ production potentially meeting the DOE cost targets.

### Question 2: Approach to performing the work

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

- Given the complexity of this project, the approach appears to be reasonable.
- The researchers have a very interesting approach that creates a porous matrix with high surface areas and a large number of pores. The structures do not seem to be very mechanically stable. In a large cell, this reviewer wants to know if the material would crush the pores. There are several materials in contact with each other that may impact the number of thermal cycles that the material can withstand without attrition.
- The project is focused on the development of atomic layer deposition (ALD) of ferrite materials. This is the heart of the system and is central to a successful project. The project is well thought out and appears to be integrated with the effort of Sandia National Laboratories (SNL). Resources for the project appear to flow excessively to SNL (compared to the funding for this project).
- Most aspects of the technical approach to this project are excellent. The Hercynite cycle has been well defined and characterized based on thermodynamic calculations and Raman spectroscopy. The kinetics have been demonstrated and studied using the novel laser-assisted stagnation flow reactor. The concept of generating and using porous alumina particles coated with the Hercynite to eliminate diffusion, sensible heat, and heat conduction issues is brilliant. The discovery and potential of running the redox reactions at the same temperature is very promising. The consistent attention to cost and the use of the Hydrogen Analysis (H2A) model for cost

analysis is outstanding. Not quite enough attention is being paid to the reactor design itself or the robustness of the packed bed in the reactor tubes. There also could be issues with the practical operation of the reactor design relative to two temperature regimes if needed, and with cycling the input of steam. The collaboration on this effort with the National Renewable Energy Laboratory's (NREL's) solar facilities is excellent, but beyond that, the collaboration seems limited to some interaction with SNL.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Satisfactory progress was reported.
- The project appears to have made significant progress in identifying an improved Hercynite active material and high-surface-area method of support. Researchers have successfully achieved all three milestones. Additionally, the milestones were more specifically defined than in the past.
- The researchers have done a good job of characterizing the process to make the particles. The use of $CO_2$ for the tests was a good innovation for understanding the material. Even though the $CO_2$ tests are interesting, there is still a need to do the tests with water because there are very significant differences in the chemistry. The material fabrication process seems to be very involved and may be expensive for large-scale synthesis. The researchers have not addressed the issue of the mechanical strength of the material. It may be thermodynamically stable, but the mechanical strength needs to be tested. This is a very porous material and alumina is not particularly strong. The mechanical strength needs to be measured. Even though the thermodynamics predict a stable material, the sample needs to be cycled to tested.
- Excellent progress has been made on this project, especially considering the modest funding level. The Hercynite cycle has been well defined and characterized based on thermodynamic calculations and Raman spectroscopy. The kinetics have been demonstrated and studied using the novel laser-assisted stagnation flow reactor. Researchers have demonstrated the generation of porous alumina particles coated with the Hercynite using ALD to eliminate diffusion, sensible heat, and heat conduction issues. The Hercynite-coated particles have been used in on-sun experiments at NREL, demonstrating the feasibility of the Hercynite cycle and project approach. Cost analyses have been done, demonstrating the feasibility of achieving the DOE cost targets. The discovery and potential of running the redox reactions at the same temperature is very promising. Not enough attention is being paid to the reactor design itself or the robustness of the packed bed in the reactor tubes.

### Question 4: Collaboration and coordination with other institutions

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

- There was good collaboration between the partners.
- From other presentations, this reviewer gathers that there is adequate-to-good collaboration between the SNL team and the University of Colorado. However, collaboration was not described in the presentation.
- Collaboration is sufficient, although the emphasis of the project should be on creativity and productivity, not on collaborations, per se. (The value of this entry as a means of assessing success is questionable. The Program may wish to rethink or rephrase this evaluation question.)
- The collaboration on this effort with the NREL solar facilities is excellent, but beyond that, the collaboration seems limited to some interaction with SNL.

### Question 5: Proposed future work

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

- The proposed future work is a reasonable extension of the present effort.
- The researchers must decrease the cycle time of the process in order for this process to be viable.
- The proposed work is logical but not fully detailed.
- The proposed future work is well thought through and includes investigating temperature and pressure ranges to reduce process costs, investigating Hercynite/alumina composition effects on redox performance and particle

robustness, and further investigating the potential to operate both the oxidation and reduction at one temperature. There is nothing specifically mentioned in the plan about reactor design and testing.

### Project strengths:

- This project features a very strong team. The researchers are developing a simple cycle that has some chance of success.
- Solar thermochemical cycles, such as the Hercynite cycle being researched in this project, have the promise of being cost-effective, near-zero-carbon-emitting $H_2$ production options. The Hercynite cycle is a very attractive solar thermochemical cycle because it is a simple two-step cycle and because it operates at temperatures below 1,500°C. These two factors result in this solar approach to $H_2$ production potentially meeting the DOE cost targets. Most aspects of the technical approach to this project are excellent. The Hercynite cycle has been well defined and characterized based on thermodynamic calculations and Raman spectroscopy. The kinetics have been demonstrated and studied using the novel laser-assisted stagnation flow reactor. The concept of generating and using porous alumina particles coated with the Hercynite to eliminate diffusion, sensible heat, and heat conduction issues is brilliant. The discovery and potential of running the redox reactions at the same temperature is very promising. The consistent attention to cost and the use of H2A for cost analysis is outstanding. Excellent progress has been made on this project, especially considering the modest funding level.

### Project weaknesses:

- It is not clear how this process can operate 24/7, which will significantly decrease its usefulness. There are cycle life concerns regarding the materials.
- Not enough attention is being paid to the reactor design itself or the robustness of the packed bed in the reactor tubes. There also could be issues with the practical operation of the reactor design relative to two temperature regimes if needed, and with cycling the input of steam. There is nothing specifically mentioned in the plan about reactor design and testing. The collaboration on this effort with the NREL solar facilities is excellent, but beyond that, collaboration seems limited to some interaction with SNL.

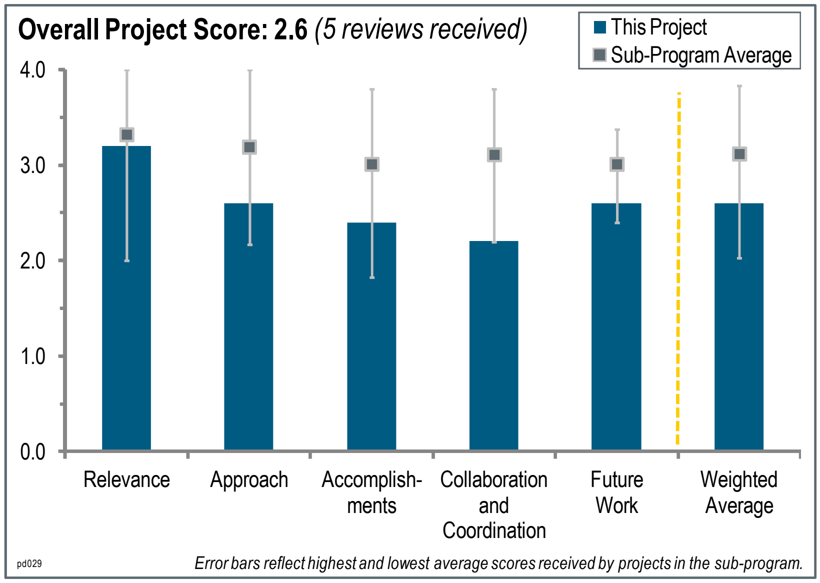
### Recommendations for additions/deletions to project scope:

- The project team should expand the future work to include efforts on reactor design and testing.

# Project # PD-029: High-Capacity, High Pressure Electrolysis System with Renewable Power Sources

Paul Dunn; Avalence LLC

**Brief Summary of Project:**
The current objective of this project is to produce a pilot plant (1/10th scale) design for use as a basis for an economic analysis of plant fabrication and operating costs for hydrogen ($H_2$) production using an electrolyzer with a nested cell. The operation and efficiency of the pilot plant will be demonstrated through laboratory testing at Avalence and field testing at the National Renewable Energy Laboratory. The project also strives to prepare a site location to accept the completed plant for commercial (300 kg/day, 750 kW) operation. The project addresses capital cost, system efficiency, and renewable power integration of $H_2$ production.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- This project is definitely working toward DOE project goals.
- The delivery of $H_2$ at 6,500 psi is relevant to the objectives.
- High-pressure electrolysis is of great importance to the DOE Hydrogen and Fuel Cells Program (the Program).
- The overall objective for a low-cost electrolyzer is a good objective.
- The project is clearly aligned and relevant to DOE goals, but the presenter could have done a better job of quantifying how the features being developed will result in economic gains.

## Question 2: Approach to performing the work

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

- One reviewer was favorably impressed with the way the company is addressing the challenges that have come up in the course of the project, and its innovative approaches in overcoming them. The company’s solutions are well thought out, and it has made good progress in overcoming them.
- The team learned a number of lessons from the ideas that did not work, and then moved on to the design that worked.
- Another reviewer cannot say that the approach is outstanding, because at some level it failed. However, the team seems to have learned substantial lessons from the failure, and has developed a new approach that appears to be well suited to the barriers.
- The approach is an interesting idea. However, it has been clear that the original design would not work since almost the inception of the project. It is good to see the changes made in the last year. The new approach may work, but it is not clear that substantial cost reduction will be achieved. It seems that each cell will still require its own valves, etc., so the balance of plant seems to be very large (and expensive). The controls may be more complicated than needed.
- There is very little analysis of the cell performance capability or the ability to withstand pressure differential. The approach seems entirely empirical, which is terribly risky and inefficient.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Given some of the difficulties the researchers have experienced, they have made good progress. It sounds like they have solved the major problems and are in a good position to deliver an interesting device. They have developed some very innovative solutions.
- Given the experimental difficulty with this innovative approach, the team is making good progress.
- The project is a blend of a failure of the original plan and outstanding progress on the new plan. Unfortunately, expending so much effort on finding the right structure and containment approach for the cell has reduced the team's ability to look for broader advances for the cell, which might also be needed to push this over the top.
- It takes a lot of courage to indicate that the initial design was wrong and that the project had to be significantly changed in focus. The presenters should be congratulated on this. The fact that the composite wrapping is expensive should not have been a surprise. This has been pointed out by reviewers since the first time this project was reviewed. It is not clear whether the system will be easily sealed.
- No cells or stacks with commercial promise have been developed. No analysis is available that shows a credible path has been identified. The efficiencies are inferior to state-of-the-art polymer electrolyte membrane cells.

### Question 4: Collaboration and coordination with other institutions

This project was rated **2.2** for its collaboration and coordination.

- The researchers are collaborating with companies that have the required expertise to solve the specific problem. This is a good use of their resources.
- The project features good use of collaborators, but they are mostly in the form of vendors.
- The collaboration seems to be mostly in the form of subcontracting others to do some very specific work. It is not clear how additional collaborations were done.
- There appears to be little collaboration with other parties.
- This project features no collaborations.

### Question 5: Proposed future work

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

- No analysis was identified, and no safety study was prioritized.
- The researchers are on the right track. A 10-kg pilot plant is a good size for their next step, and they can either use it as a building block module or scale-up based on the results of the pilot plant operations. Reviewers will need to assess the durability of the pilot plant before going to a larger unit.
- The team has good plans for the future work after solving a number of technical problems.
- The delayed delivery pushes this toward "get it out the door" mode, but this project also needs to think more broadly about what is the next phase—assuming success—to take this technology to the next level.
- The future work seems reasonable. There are concerns about how the many tubes will be sealed to ensure that there is no leaking at the entry/exit points of the tubes. A decrease in cost by a factor of five still makes this a very expensive electrolyzer. It would be useful to see the researchers' plan for getting to the DOE target capital cost.

### Project strengths:

- This is a high-risk, high-payoff project that is worthy of support.
- This is an interesting approach to achieving high-pressure $H_2$ without a compressor. Alkaline electrolysis has the potential to use low-cost materials.
- Strengths of this project include the researchers' ability to identify novel solutions to difficult problems, and their dedication to solving their problems. They are not quitters. They have intellectual honesty, and are making significant progress. The project has accomplished a lot on a relatively small budget. Ultimately, this is a low-cost solution that has the potential to meet Hydrogen Production and Delivery sub-program goals.

- This reviewer felt that this project has no strengths.

### Project weaknesses:

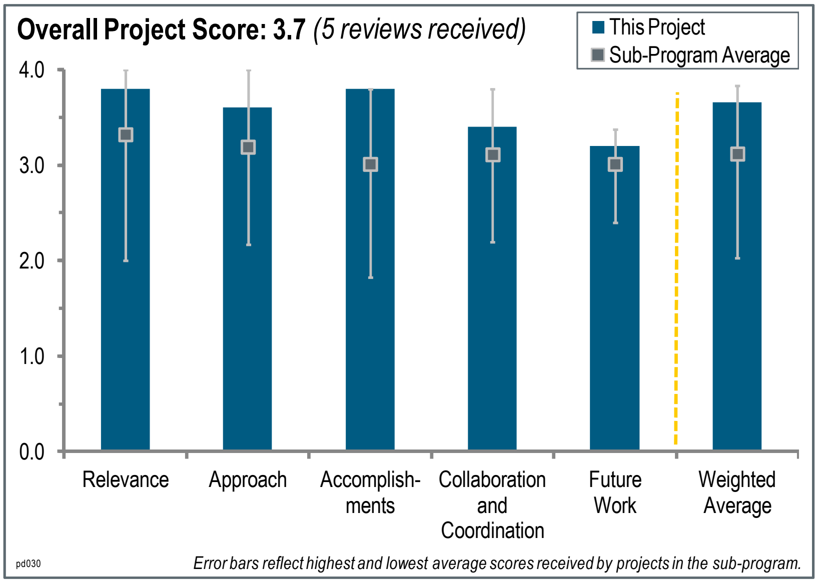
- This reviewer is concerned that the researchers will run out of budget. They seem to have done a lot of work on a small budget.
- The team can benefit from some collaboration with an academic team that can apply the latest science.
- This project features no analysis, poor performance, and significant safety concerns associated with a balanced pressure cell that were brushed off.
- This presentation was poor. Too many slides are holdovers from prior years, used without much thought of what they add to this presentation. There was some excellent spoken discussion of the failings of the first approach, but there was little in the presentation to support that discussion. Frequently, the critical information (such as why purity increased) was described verbally, but was nowhere to be seen in the slides. The team needs to do a much better job of figuring out what it wants to say before creating the presentation, and then create the slides to support that message.
- The multiple tube approach will require very complicated sealing and the use of many valves and controls. This will drive up cost, so it is not clear that the cost reduction will be enough for the researchers to achieve the DOE cost targets. There is a need for long-term testing. They have shown a lower level of contaminants in the product gas during initial production and at lower pressure (2,000 psi). They need to test at the higher pressures and for longer periods of time.

### Recommendations for additions/deletions to project scope:

- The project scope is appropriate.
- This project should not receive additional funding at this time. The project represents a very high risk for the Program, since there is a non-trivial risk of a safety incident and very little likelihood of a good set of technical results.
- This reviewer had no recommendations.

## Project # PD-030: PEM Electrolyzer Incorporating an Advanced Low Cost Membrane

Monjid Hamdan; Giner Electrochemical Systems, LLC




**Brief Summary of Project:**
The overall project objective is to develop and demonstrate an advanced, low-cost, moderate-pressure proton exchange membrane water electrolyzer system to meet U.S. Department of Energy (DOE) targets for distributed electrolysis. This task involves developing: (1) a high-efficiency, low-cost membrane, (2) a long-life cell separator, and (3) a low-cost prototype electrolyzer stack and system. The objectives for the fiscal year (FY) 2012 are to: (1) complete an electrolyzer stack and system assembly, (2) evaluate the electrolyzer's performance and efficiency, and (3) deliver and demonstrate a prototype electrolyzer system at the National Renewable Energy Laboratory (NREL).

### Question 1: Relevance to overall DOE objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The project has good relevance to the DOE objectives.
- This project is clearly relevant to DOE goals, with a very clear problem statement and value proposition.
- This project is directly responsive to DOE Hydrogen and Fuel Cells Program goals and objectives.
- The project is in line with the DOE targets for distributed electrolysis to produce hydrogen ($H_2$). It addresses the critical issues of PEM electrolysis.
- This project meets DOE's stated objectives. Cost reductions at this small scale can be applied to larger-scale units.

### Question 2: Approach to performing the work

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

- The approach is well designed, with good integration of the different steps from the membrane to the system.
- The group is looking at individual aspects of the proposed electrolyzer carefully.
- The presentation clearly identified how the features studied are designed to impact the important project and technology goals.
- This project features a very well-thought-out approach and a good blend of analysis, design, and experiments.
- This project features an excellent focus on critical components. The design for high-volume manufacturing is essential. Teaming with a large-volume commercial manufacturer is beneficial and brings reality to the project. Researchers are hitting their milestones.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.8** for its accomplishments and progress.

- The project is making good progress.
- Very encouraging progress on cost reduction has been made over the past year.
- The project made significant progress on membranes, demonstrating durability and improvements in efficiency. In addition, developments resulted in cost reductions in the stack and system. The talk gave a detailed presentation of the performances.
- Cutting the cost of the membrane by orders of magnitude is impressive, as is the separator performance lifetime estimate of >60,000 hours. The team made excellent progress on stack cost reductions. An efficiency of 47 kWh/kg for $H_2$ production is equal to $1.83 per kg at $0.039/kWh.
- A lot of the heavy lifting in these developments is from prior years. Progress has been outstanding over the course of the project, but this last year the team seems to be in a finishing-up mode.

### Question 4: Collaboration and coordination with other institutions

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

- The coordination is good, covering the various competencies needed to work on the different components.
- The project has involved major industry players such as 3M and Entegris. There has been good collaboration with AREVA, which provides independent third-party performance verification.
- This project features excellent utilization of skill sets outside of the company.
- The partners are well identified and recognized, but they seem to be mostly in the mode of vendors as opposed to partners.
- This project could probably show more evidence of tight collaborations with the supply base.

### Question 5: Proposed future work

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

- The proposed work for further development is well thought out.
- There is a good map of future plans based on the remaining issues to address.
- The project is nearly complete and the future plans to improve are identified. The team will continue the developments with longer testing time to deliver the prototype to NREL for evaluation, and it will also address future challenges such as the labor cost.
- This is an excellent project with good results. Manufacturing cost reductions to take out the labor component is the way to go.
- The future plans are good. At this stage of the project, it is hard to provide an outstanding plan unless Giner were to identify some bold new direction in which to work that would again dramatically improve the performance.

### Project strengths:

- The team appears have a strong hold on various aspects of the technology.
- A strength of this project is its focus on design improvements with respect to key cost and product lifetime challenges.
- The team is focused on the right things. Strengths of this project include its good use of industrial partners, how its reduction of part count and design for manufacturing will further reduce costs, and its reduction of material costs by orders of magnitude.

### Project weaknesses:

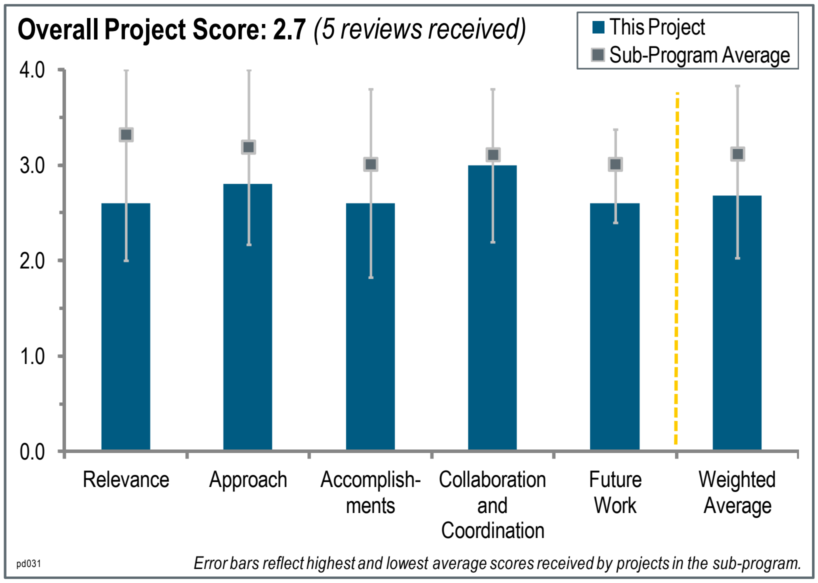
- A better focus on the reduction of the overall cost is needed, as opposed to optimizing individual components.
- This project has relatively limited partnerships.
- This reviewer did not detect any weaknesses.

### Recommendations for additions/deletions to project scope:

- The quality of water seems important to achieve the performances. This issue has to be addressed in the future.
- This reviewer recommends a significant scale-up to utility scale after the current project has been completed. Hundreds and thousands of kilograms of $H_2$ are needed. It is not certain that PEM can meet the challenge.
- As the capital cost drops, these systems are becoming more and more limited by the cost of electricity. There is a need for some out-of-the-box thinking about how this technology could contribute to achieving lower-cost $H_2$.
- The project team should consider additional partnerships to enable a more commercial program scope.
- This reviewer had no recommendations.

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

Kevin Harrison; National Renewable Energy Laboratory




## Brief Summary of Project:

This project strives to test, demonstrate, and analyze renewable electrolysis integrated systems. It will test the characterization and performance of electrolysis systems and electrolyzer stack and system response with typical renewable power profiles. Demonstration of renewable resources integration includes: (1) identifying system cost reductions and optimization for electric utilities; (2) characterizing, evaluating, and modeling the integrated systems; (3) characterizing electrolyzer performance with variable stack power; and (4) designing, building, and testing shared power electronics and direct-coupled renewable-to-stack configurations. The analysis of wind-to-hydrogen ($H_2$) involves developing cost models for renewable electrolysis systems and quantifying capital costs and efficiency for wind- and solar-based electrolysis scenarios.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.6** for its relevance to DOE objectives.

- The project does not have good relevance to DOE objectives.
- Overall, this project is relevant to the DOE Hydrogen and Fuel Cells Program (the Program) objectives. However, the project title and presentation content give the impression that the researchers were addressing system integration and testing of renewable electrolysis systems. The work presented, though still relevant to the ultimate DOE objectives, was in fact about grid integration of electrolyzer systems. None of the four stated barriers was directly addressed this year or in 2011.
- Independent third-party testing and validation of technical and economic claims by component manufacturers is critical to supporting the integrity of the Program. Wind models and wind-to-$H_2$ analysis are critical to understanding the economic performance of the overall system and to determining where cost-cutting efforts are needed. The overall electrolyzer efficiency reported on slide 4 is at variance with reported results by industry partners.
- There is general alignment between the project and the Program goals. However, there is no value proposition discussed for the stated objectives—there is no identification of how completing these objectives quantifiably advances the mission. Also, a disproportionate part of the progress seems to have little relationship to the stated program objectives.
- The three projects reported (membrane durability under varying current flow, mass flow measurement, and grid frequency support) support the Fuel Cell Technologies Program Multi-Year Program Plan (MYPP) very well. Spinning reserve to support grid operation is expensive; if electrolysis can quickly and economically reduce spinning reserve, it could provide a more environmentally benign approach to grid stabilization.

### Question 2: Approach to performing the work

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

- The approach is good if the objectives were revised to reflect the realistic impact of the project and drop any claim to renewable energy.
- The approach is adequate for the task and seems straightforward. The team has good testing laboratory and equipment.
- The approach is weak, and the project is not likely to add significant value.
- It is not clear how this approach impacts the stated barriers of cost or system efficiency. The objectives of the project are largely geared towards renewable integration, but not much progress has been made in this area this year.
- Long-term monitoring of electrolyzer performance under "real-world" conditions will improve models and provide more accurate, reliable, and credible performance and economic forecasts that will reduce risk for those considering investment in this technology, which, when positive and competitive, will assist adoption of this technology. This monitoring addresses capital cost and system efficiency barriers by providing better quality data.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- There is not much to show for this project.
- The project team has achieved good results despite budget reductions. The mass flow measurements and the grid frequency support results are significant.
- The project team is obtaining interesting results on stack performance, including results related to decay utilizing variable power input versus constant current. This is an area that needs considerable attention. This reviewer wants to know how long an electrolyzer will last when coupled to wind. Mass flow measurement experiments are critical to the commercialization of $H_2$. The National Renewable Energy Laboratory (NREL) test rig is making an important contribution. The involvement of the National Institute of Standards and Technology is very helpful. The analysis of an electrolyzer for grid frequency support is an important area for evaluation.
- There is a lot of progress, particularly on mass flow measurements and grid frequency support. However, these topics are not aligned with the project objectives, and it is not clear how they impact the barriers. The work is not necessarily bad, but the team needs to lay the groundwork for the value proposition around the work, and explain why this work is pertinent to the objectives, goals, and barriers. There is a major disconnect between the objectives and the actual work done. The principal investigator can look at the barriers and believe this work addresses them, but that connection is not made for this reviewer.
- The project appeared to make significant progress during the review period, but the presenter offered little objective basis against which to compare; the presenter did not share any milestones or other schedule references against which to compare progress, nor any updated cost or efficiency numbers to compare against targets that have a time reference.

### Question 4: Collaboration and coordination with other institutions

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

- The project features excellent collaboration; especially with the electrolyzer vendors and utility company.
- There is collaboration with a wide range of entities: equipment suppliers, utilities, and researchers.
- This feels like a customer-supplier type of relationship.
- There is good information sharing with some entities. The use of a dedicated website to share information is not mentioned. This would be a good feature to add if not already in place. It would be interesting to see more international electrolyzers evaluated to see how the United States compares.

### Question 5: Proposed future work

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

- The proposed future work is not commensurate with the original project scope.
- Only part of the future work clearly addresses the barriers. The project team needs to draw clearer lines between this work and the barriers.
- The proposed future scope of work is reasonable, but again it does not match the stated barriers.
- NREL provides a valuable test platform, of which future work will take good advantage, to validate a variety of renewable electric sources and electrolyzer designs.
- The future work is basically a continuation of this past year, with a few additions. This may be budget-limited and is understandable. It would be interesting to see what could be done if there was more funding available. It is unclear what the designation “medium pressure” means. The Avalence stack will be high pressure (6500 psi), not medium pressure.

### Project strengths:

- The work seems to be well appreciated by industry partners.
- The collaboration with stack vendors is a strength of this project.
- This is a solid project. Strengths include the third-party validation and analysis work, the good laboratory test rigs, and the good modeling tools.

### Project weaknesses:

- The team is doing work that will be of low utility. The mass flow metering is not any better than those that are commercially available.
- The project appears to have veered off from the stated renewable-energy-specific objectives.
- One reviewer did not detect any weaknesses.

### Recommendations for additions/deletions to project scope:

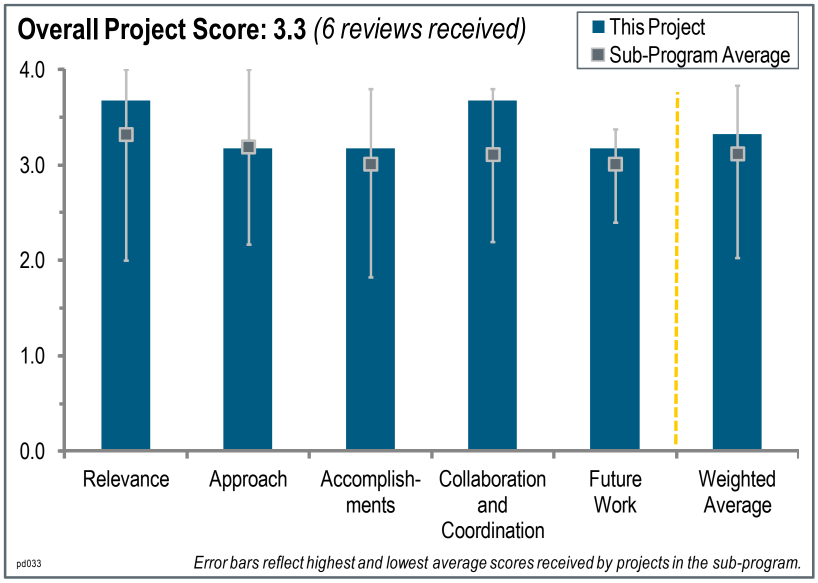
- The team should focus on renewable $H_2$, consistent with the original project focus.
- The project objectives should match the efforts. Although the project claims it is renewable-focused, grid integration is not specific to renewable resources.
- If not already in place, a dedicated website reporting results and giving access to the modeling would be helpful to industry and other research institutions. The project team should also develop a “bucket list” of additional work that could be done if more funding was available.
- This work needs a careful statement of the value proposition; the researchers need to clarify how the research and development components add quantitative value to overcoming the barriers. Only with that framing can reviewers identify which pieces should be emphasized.

## Project # PD-033: Directed Nano-scale and Macro-scale Architectures for Semiconductor Absorbers and Transparent Conducting Substrates for Photoelectrochemical Water Splitting

Thomas Jaramillo; Stanford University/National Renewable Energy Laboratory

### Brief Summary of Project:

The main objective of this project is to develop third-generation materials and structures with new properties that can potentially meet U.S. Department of Energy (DOE) targets (2013 and 2018) for usable semiconductor bandgap, chemical conversion process efficiency, and durability. Specifically, the project will develop: (1) photoelectrochemical (PEC) substrates consisting of macroporous, high-surface-area, transparent, conducting oxides upon which PEC materials can be loaded; and (2) new PEC materials based on nanostructured $MoS_2$ that can potentially meet DOE performance targets.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- The project is well aligned with goals articulated for the PEC portfolio.
- PEC water splitting/electrolysis does meet DOE Hydrogen Production Roadmap objectives.
- PEC hydrogen ($H_2$) production is a long-term technology in DOE's portfolio. The objectives are in line with DOE targets to develop efficient materials performance.
- The project is clearly aimed toward improving the solar-to-$H_2$ conversion efficiency for PEC systems.
- This project may provide some of the key technologies related to enabling a viable PEC system. The high-surface-area scaffold can ameliorate the carrier transport issues associated with many semiconductor materials that otherwise have an appropriate bandgap for efficient PEC $H_2$ production.
- The reported work is outstanding, but the emphasis on engineering frameworks without efficient and durable active materials is not consistent with earlier program priorities. Neglecting the material efficiency of $MoS_2$ in the future work plan is a deficiency that should be addressed. That topic does not appear in future work plans. Apart from plans to address efficiency, the project goals fully support the goals of the Hydrogen and Fuel Cells Program (the Program).

### Question 2: Approach to performing the work

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

- The approach is well presented and well designed to meet efficiency, durability, and cost targets. It is very relevant and different from conventional concepts.
- The approach is clear and seems well thought out. More integrated modeling and analysis could be used, as it seems a lot of the work rests on instinct.
- The project team has responded to DOE listings of critical barriers to PEC $H_2$.
- The technical approach involves engineering transparent, high-surface area electrodes (HSE) with both conventional PEC materials and some novel active materials ($MoS_2$). It is not clear if this technology is feasible.

Even though this project is still in the materials discovery and development phase, there appear to be major technical challenges with this technology.

- This effort has strong materials expertise, which will be essential to solving the many issues regarding PEC $H_2$ production. Of significant interest is advancing the state-of-the-art for non-precious-metal catalytic materials, as this will be a fundamental drive for the cost benefit for not only PEC systems, but also across a spectrum of other important technologies related to energy production.
- The focus on HSE prior to certifying high-performance, durable PEC materials is in violation of earlier Hydrogen Production and Delivery sub-program priorities. Materials of implementation directly affect process engineering and high-throughput manufacturing development. Such development might be a premature, absent definition of active materials. The $MoS_2$ focus may be flawed because efficiency remains low and the future research and development plans do not explicitly address this deficiency. Significant priority should be given to assessing the possibility that $MoS_2$ will never prove successful.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project has completed two of its four technical barriers, as well as 70% of the work on the other two.
- The project made progress in developing a route to produce a fully tunable, high-surface-area, transparent electrode with a patent filed in 2011. It also demonstrated long-term stability of nanostructured $MoS_2$ $H_2$ evolution catalysts. However, the PEC efficiency is currently low. Details were given during the talk on the way to increase it in future work.
- There is a tight focus on the development of these materials, with advances taking place on two main fronts—the development of a process to produce a high-surface-area scaffold to serve as the backbone for thin films of PEC materials, and the development of a hydrogen evolution reaction (HER) catalyst ($MoO_3$-$MoS_2$ nanowires). The results shown demonstrate a continued advancement in the state-of-the-art for these technologies.
- The work needs to be calibrated with the scale of the program, which is pretty modest. While a lot of data has been generated, it remains unclear how far away the goal remains. This is tricky, as reviewers do not want to punish anyone for honestly stating that the goals are difficult to achieve. However, it would be good to better articulate how many things have to improve by significant factors to hit the goals.
- This reviewer is glad to see an 81-fold increase in surface area for indium-tin-oxide (ITO). Of course, this reviewer questions whether Graetzl demonstrated this effect with $TiO_2$ back in 1991. It is unclear what depositing $MnO_x$ on an HSE support was supposed to accomplish, other than to demonstrate the extremes in achievable optical density. This reviewer hopes not too much time was spent on it. The investigator needs to better understand why increases in surface area and wider bandgap with decreased particle size have not necessarily translated into proportionately higher solar-to-$H_2$ conversion efficiency. Nevertheless, improved current-voltage characteristics due to the combination of high-surface-area support and nanoparticle arrays of $MoS_2$ were demonstrated.
- The statements that hollow-core $MoS_2$ nanowires are “100%” stable and that the $MoO_3$ core is “completely protected” are possibly misleading. It is apparent that performance after 10,000 cycles is unchanged, but it is possible that the material is modified at the electronic level, which could lead to problems at a later date or under different conditions. The apparent material stability could be significantly affected by implementation of photocatalytic activity. Electrocatalysis performance shows no discernible change after 10,000 cycles. There is no apparent basis for the assertion that $MoS_2$ “high efficiencies” will accompany future incorporation into an HSE framework. Such improvement depends on why $MoS_2$ PEC efficiency is poor. HSE incorporation would not affect efficiency if low efficiency derives from the observed $MoS_2$ indirect bandgap.

### Question 4: Collaboration and coordination with other institutions

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

- This project features a good set of collaborators in the science domain.
- The project is well coordinated and has good collaborations with partners that are experts in the critical areas of focus.

- Meaningful collaborations have taken place with a number of groups. Future collaborations will be important with respect to evaluating the HSE scaffold with a variety of PEC materials.
- The Stanford effort is thoroughly integrated with the PEC working group.
- More characterization of interface energy states is needed to confirm stability assertions. More attention to theoretical investigations of the effects of indirect bandgap on performance might be useful. Both of these activities can be effected through collaboration partners in the PEC Working Group.

### Question 5: Proposed future work

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

- The project has identified future work to expand the HSE composition, perfect absorber contact via interfacial engineering, and integrate different nanomaterials with HSE.
- Details on the next steps were missing in the slides, but more information was given during the talk. This point could be improved to convince one reviewer that efficiency can be really improved in the future work.
- The proposed future work is a natural extension of the promising results presented in 2012. Focusing on understanding the interface between the semiconductor material and the HSE is important.
- The proposed future work is a thoughtful definition of what would be useful to do in the future. More gap analysis would be useful to ensure that the efforts are focused on the right gaps.
- The emphasis should stay on nanoparticle photovoltaics. Another reviewer is a little concerned that developing HSEs based on other transparend conductive oxides (TCOs) could become an all-consuming effort.
- The emphasis on continued HSE development represents the pursuit of a solution in search of a problem. It is this reviewer's opinion that PEC priorities must continue to address the search for an effective and durable PEC material before making significant investment in implementation frameworks. The proposed future work should explicitly address the low PEC efficiency observed for $MoS_2$.

### Project strengths:

- This project offers some novel and outside-the-box thinking.
- This project features good collaborations and a sincere focus on improvement to the materials under study.
- The work is of high quality. The team is working hard to face the PEC issues. The choices made in processes and materials are relevant to reducing cost.
- This project has a very well-defined scope, with a well-thought-out approach that provides a meaningful way to advance the technology. Processes and methods are amenable to scaling, so there should be minimal issues for volume manufacturing.
- The project team has excellent control over nanoparticle structures. Another strength of this project is its continued improvement in non-noble metal $H_2$ evolution.
- HSE development will likely prove useful, regardless of its relative priority in program objectives. Nevertheless, PEC is a long-term technology option, so the emphasis on materials discovery should be retained. The strong collaboration with other institutions, organizations, and the PEC community is evident. Technical proficiency is evident throughout the project, and necessary facilities and equipment are available either through the home institution or via the PEC Working Group. Outstanding progress in defined tasks is evident.

### Project weaknesses:

- There is little modeling of the phenomenology.
- The project risks being pushed by DOE to cover too much ground too fast, before accomplishments are understood.
- The Hydrogen Production and Delivery sub-program portfolio, through either management or the PEC Working Group, should revisit the earlier priorities for PEC investments and ensure that all PEC projects adhere to those investment priorities.
- According to the 2011 work, producing one metric ton of $H_2$ per day requires 0.03 square miles of colloidal dual-bed suspensions or 0.02 square miles of planer PEC cells. The mass production element, cost effectiveness, final system configuration, location where such a system can be installed, and balance of plant (fuel collection, fluid

system, dryers, compression, etc.) put this technology at a lower priority level compared to other technologies, such as PEM electrolysis.
- Although the project made progress with a team of high experts, the PEC efficiency is still low. The gap between the current results and the 2013 DOE targets is large. It raises the question of how these targets can really be achieved next year.
- This reviewer is not convinced that supporting a thin PEC material on an HSE will ultimately improve efficiencies. If the bulk of the losses for electron transport primarily occur in at defect-rich grain boundary/interface regions, then for a given optical density (assuming a constant particle size), the volume of defect-rich material will be conserved.

### Recommendations for additions/deletions to project scope:

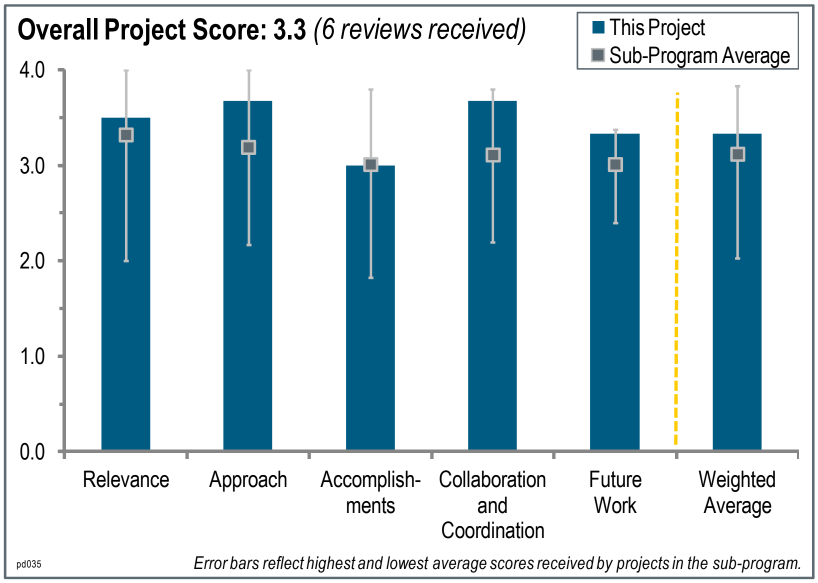
- The practicality of such technology needs to be evaluated by DOE. There are many technologies that provide significant technical advancements over the current technologies, but the majority of them end up not being practical due to mass production and fabrication challenges, being integrated in an end-to-end system, and high costs.
- The project team should develop overall models that will help estimate required improvements in materials properties.
- It is doubtful that the 2013 DOE technical targets (efficiency, durability, cost) will be achieved next year because there are still challenges. Nevertheless, the project is still at an early stage. The approach is relevant and different from others, and the results are encouraging, so the work has to continue next year to further investigate the concept.
- The HSE is an exciting development with applications across a wide spectrum of technologies. Regarding PEC, it is important to identify where the losses occur. When measuring photocurrents, to effectively compare materials, it is more telling to measure the absorbed photon to current efficiency (i.e., internal quantum efficiency). This would more clearly show how the material is performing on a fundamental level (e.g., trap states, improved photoconductivity, etc.). Using an APCE (absorbed photon conversion efficiency) metric, this reviewer wants to know how the HSE with $Fe_2O_3$ would compare to the planar thin film.
- The Hydrogen Production and Delivery sub-program, through either management or the PEC Working Group, should revisit the earlier priorities for PEC investments and ensure that all PEC projects adhere to those investment priorities. The project work scope should include specific attention to $MoS_2$ PEC efficiency issues.

## Project # PD-035: Semiconductor Materials for Photoelectrolysis

John Turner; National Renewable Energy Laboratory

### Brief Summary of Project:

The objective of this work is to discover and characterize a semiconductor material set or device configuration that: (1) splits water into hydrogen ($H_2$) and oxygen spontaneously upon illumination, (2) has a solar-to-$H_2$ efficiency of at least 5% with a clear pathway to a 10% water-splitting system, (3) exhibits the possibility of 1,000 hours of stability under solar conditions, and (4) can be adapted to high-volume manufacturing techniques. The main focus this past year has been to work with state-of-the-art materials that meet the U.S. Department of Energy's (DOE's) near-term efficiency targets and investigate surface treatments that promote durability.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.5** for its relevance to DOE objectives.

- The project is well aligned with Hydrogen and Fuel Cells Program (the Program) goals.
- This project is clearly related to the Program's goals and objectives.
- This project matches up quite well with the Program's objectives for photoelectrochemical (PEC) $H_2$ production. The continued development of low-bandgap materials with a focus on improving durability is relevant to achieving the 2013 technical targets.
- The project fully supports DOE research and development (R&D) objectives in PEC $H_2$ production by advancing materials development with appropriate and effective semiconductor bandgap, chemical process efficiency, and projected solar-to-$H_2$ plant efficiency with materials durability.
- Semiconductor materials for photoelectrolysis meet DOE Hydrogen Production Roadmap objectives.

### Question 2: Approach to performing the work

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

- The three-pronged approach to identifying and determining systems that meet DOE targets is excellent.
- The approach includes a good mix of experiments and modeling. Some consideration should be given to the trade-off associated with the cost of high-efficiency solar cells.
- The ability of the base material to produce with high-efficiency solar-to-$H_2$ production has already been demonstrated. The focus on improving durability is a meaningful approach, and the methods and collaborations provide the best opportunity for success.
- It is understandable that this project is still in the materials discovery and development phase. But, it is not feasible; there are major technical challenges with this technology.
- $GaInP_2/GaAs$ has no rival for efficiency, so concentrating on aqueous stability makes sense.
- The approach incorporates long-planned combined theoretical materials analysis, leading to the understanding of behavior through materials characterization and performance observation and demonstration. Further attention to material cost and mitigation strategies might be warranted. Such strategic effort should consider the trade-off

between extending the search for equivalent or better materials and continuing the focus on the current material that performs well beyond any other known material.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The team continues to make good progress in demonstrating high efficiency.
- There are significant similarities among the 2011 and 2012 project results. It is not clear why DOE funded two projects under the same title to the same group. One started in 1991 and ended in October 2011, and one started in 2004 and is projected to end in September 2012.
- The new (preliminary) results on nitrided $GaInP_2N$ appear to be a major breakthrough with respect to improving the durability of the semiconductor material. There now needs to be a focus on further fabrication of this material to validate that these results can be repeated across larger sample sets. It appears that the samples that showed the significant progress were the same as presented in 2011. This reviewer would have expected (in light of the promising 2011 preliminary results) that a significant effort would have been made toward fabricating and characterizing a larger sample set.
- One accomplishment is the endurance of nitride surface passivation treatments, which showed >100 hours with minimal decay. All milestones have been met or are on track for completion. The team also performed efficiency benchmarking testing at the National Renewable Energy Laboratory (NREL) under real-world sunlight for comparison with laboratory light spectrum. The team is making continued progress in PEC reporting standardization.
- The major discussion was over whether etching with $N_2^+$ was a good idea. It appeared that the trade-off between decreased performance and increased corrosion resistance was acceptable. Unifying testing protocols is a good idea, but the researchers need to be cautious to not make the process so complicated that NREL is the only laboratory on the planet that can do the characterization.
- The demonstration of >100 hours of durable electrocatalysis operation without observable degradation in performance and with no immediately obvious material deterioration is an outstanding achievement and is presently unmatched in the field. Nevertheless, continued study, especially at the atomic level, of interface effects under conditions of full-spectrum photocatalysis should be undertaken before material durability concerns for this active substance can be said to have achieved intermediate performance goals. Additional effort is needed to better characterize the nature of nitrogen incorporation in treated samples by addressing distribution and distinction between nitride formation and nitrogen embedded freely within the treated material matrix. Such work would advance the understanding of the nature of this corrosion protection and would help focus materials stabilization schemes for this and other complex active materials. Collaboration in developing and validating theoretical studies of more complex material formulation should be emphasized. Such work should include the material for which progress is reported in this review.

### Question 4: Collaboration and coordination with other institutions

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

- The investigators make good efforts to collaborate with others.
- As a central organization to this effort, NREL has established significant collaborations with many members of the PEC community and across a wide spectrum of technical expertise, including fabrication, modeling, and characterizations.
- This project features an extremely corroborative team that very logically divides the work based on who can conduct each aspect most efficiently. It is a model of collaborative effort.
- All of the institutions affiliated with the Fuel Cell Technologies Program (FCT Program) PEC portfolio look to NREL for leadership, but only a few (the University of Nevada, Las Vegas and Lawrence Livermore National Laboratory) had much of an impact on NREL's effort on $GaInP_2/GaAs$.
- This project has taken full opportunity for collaboration and coordination provided through the PEC Working Group and makes excellent use of the skills and products afforded by other Working Group members.

### Question 5: Proposed future work

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

- The project has well-laid-out plans for the future.
- All of the stated goals are fine. Based on the new results concerning nitriding of $GaInP_2$ surfaces, investigators should take a more narrowly focused effort on fabricating and characterizing a larger sample set of material in the near term, focusing on understanding the mechanisms for improved durability.
- Demonstration of a tandem cell with >1,000-hour durability (made possible by the surface nitriding treatment) would be a major achievement. One reviewer is glad to see that the investigators are pursuing this benchmark.
- The proposed future work is nothing overly exciting, but what was proposed was a logical extension of what the investigators presented.
- The stated plans in the review implicitly incorporate some of another reviewer's suggestions. Nevertheless, the progress reported should be amplified through explicit planning to fully comprehend the nature and basis of the observed performance improvement. This project, along with its collaborators in the PEC Working Group, should address issues regarding prioritizing investments between new material searches and continued improvement of current materials.

### Project strengths:

- The project offers some novel and outside-the-box thinking and R&D approaches.
- This project features good cell performance and a good combination of theory and experiment.
- This is clearly the flagship facility for the FCT Program's PEC portfolio. The NREL group is the champion of the III-V semiconductor material—the material that has shown the most promise to date. The primary issue has always been the poor durability of the base material, and results over the last year indicate that a nitrided surface can alleviate this issue. Recent results for the surface nitride $GaInP_2$ and for the experimentation with pure InGaN are encouraging.
- The strength of this project lies with its logical organization and collaborative spirit. Substantial progress in PEC performance and endurance is required to meet DOE goals. However, the project is well constructed and uses an approach that is broad enough to encompass several different pathways to the goal.
- The project is focused on those high-priority objectives established earlier for the PEC portfolio. Outstanding technical proficiency is evident and extraordinary progress is reported in overcoming high-priority barriers to PEC performance. Coordination and collaboration with other institutions, organizations, and community expertise is outstanding, and this component of the research effort has directly contributed to the progress reported.

### Project weaknesses:

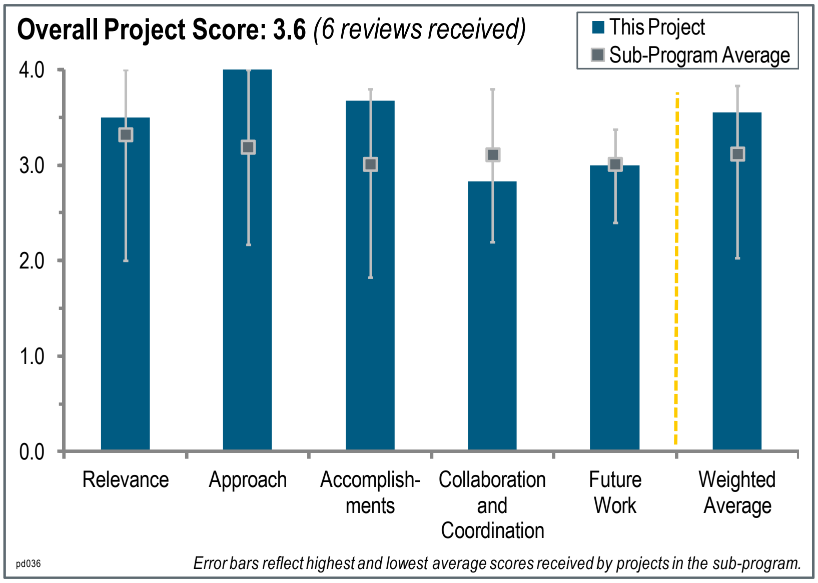
- No significant progress was made with this project since 2011. It is understandable that this project is still in the materials discovery and development phase. However, there are major technical challenges with this technology.
- The team could better address what it thinks the upside potential will be for the nitriding process. It is not clear whether it thinks the approach can yield a long-term solution or if it will only be a mid-term durability solution.
- It is unclear if high-efficiency, very expensive solar cells will be the right platform to develop.
- Surface nitriding may help, but it represents yet another process step in an already expensive fabrication technology.
- Funding levels and project workforce levels will limit the rate of progress.
- Although DOE encourages collaborations, which are wonderful opportunities to advance technologies, collaborations should not consume the effort to the point of distraction. A significant portion of the presentation dealt with the effort of other groups, either material fabrications (e.g., Williamson, Los Alamos National Laboratory) or characterizations (e.g., Heske, Ogitsu). It was therefore difficult to extract the message for significant accomplishments over the last year. Again, this reviewer would have thought, based on 2010's (2011 presentation) results, that the nitrided $GaInP_2$ would have been pushed and more fully explored.

### Recommendations for additions/deletions to project scope:

- The practicality of such a technology needs to be evaluated by DOE. There are many technologies that provide significant technical advancements over the current technologies, but they end up not being very practical due to mass production and fabrication challenges, being integrated in an end-to-end system, and high costs.
- In the short term, the team should work on the GaInP-N with laser focus to determine if it is indeed a viable material when the durability issue has been solved. The PI mentioned that the cost for these materials can be offset using 10-X concentrated conditions. If this is the envisioned use for the III-V material class, then durability testing should be performed under these 10-X conditions to verify that photo-corrosion is not problematic at these elevated intensity levels.
- The project team needs to address cost issues of nitrided gallium PEC cells. Even if the durability is improved, this reviewer wonders if these cells would be cost effective. Investigators need to evaluate the performance and durability of cells under solar concentration. Concentration may be a solution to high material costs. However, investigation of treatments under solar conditions should be examined.
- A question was made about scale-up issues with regard to metal-organic chemical vapor deposition (MOCVD) It was pointed out that light-emitting diode technology seems to be doing fine with essentially the same fabrication technology. That may be true, but to make a dent in U.S. electricity demand, a household will need a photovoltaic array considerably larger than their television and computer screens. Such studies likely already exist, but it would nice for the Program to have the numbers for large-scale production at hand.
- Continued study, especially at the atomic level, of interface effects under conditions of full-spectrum photocatalysis should be undertaken before material durability concerns for this active substance can be said to have achieved intermediate performance goals. Additional effort is needed to better characterize the nature of nitrogen incorporation in treated samples by addressing distribution and distinction between nitride formation and nitrogen embedded freely within the treated material matrix.

## Project # PD-036: Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures

Tasios Melis; University of California, Berkeley




### Brief Summary of Project:

The objectives of the project are to: (1) identify genes and associated molecular mechanisms that confer a truncated light-harvesting antenna (Tla) property in the Tla2 and Tla3 strains of *Chlamydomonas reinhardtii*, and (2) develop protocols for the targeted truncation of the light-harvesting antenna size in cyanobacteria. These objectives are accomplished through cloning *tla2* and *tla3* phenotype genes, performing functional analysis of the transformants, and applying the TLA concept to cyanobacteria.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This project is clearly directly related to the objective of the DOE Hydrogen and Fuel Cells Program (the Program) to produce cost-effective renewable $H_2$. Increasing the utilization of captured sunlight to split water supports this objective.
- This is an outstanding piece of work, but, in the general scheme, it addresses only one of a myriad of issues affecting efficient and enduring photobiological $H_2$ production. Plans for integrating products from this effort were made evident during the presentation, but the lack of collaboration and coordination for program implementation were also evident, both in the project presentation and in audience participation in the review.
- The project improves the efficiency of photosynthetic organisms whose photon collection apparatus exceeds its processing capacity. In the case of algae-based $H_2$ production, this project strongly supports DOE objectives related to the increased efficiency of light utilization and the reduced cost of biological $H_2$ production.
- The project is based on increasing the efficiency of photobiological hydrogen ($H_2$) in the green algal *Chlamydomonas reinhardtii* to reduce the amount of heat dissipated from bright sunlight and increase the amount of transmittance of light through the growing algal culture by truncating the chlorophyll antenna size. This project aligns with the longer-term pathway goals and objectives of the Hydrogen Production and Delivery sub-program in the biological production of $H_2$. Determining the genetics behind antenna size may be seen as basic research; however, the principal investigator (PI) is doing a good job of focusing the project on the potential applications.
- The presenter's work focused on optimizing photosynthetic efficiency of a model organism of $H_2$ production, *Chlamydomonas reinhardtii*. The PI has exceeded the targets set for the project. The researcher has made efforts to protect intellectual property, giving rise to the potential commercialization of the discoveries. The discoveries of this presenter could have major ramifications in many fields that work with photosynthetic organisms.
- The project focus on increasing light utilization efficiency and $H_2$ production in a biological system (microalgal cultures) is clearly relevant for DOE. The study of genetic determinants influencing antenna size could potentially be considered basic research, but the PI clearly has long-term goals relevant to $H_2$ production in mind in the design of this project. While developing microorganisms with optimized antennae for light harvesting is an important goal, the project, as presented, seems to be directed more toward generally enhancing photosynthetic efficiency than enhancing photobiological $H_2$ production.

### Question 2: Approach to performing the work

This project was rated **4.0** for its approach.

- This project is well designed and focused on accomplishing the Program's objectives.
- The success of the project has demonstrated the effectiveness of the approach in addressing the efficiency and, consequently, cost barriers. The interest in extending this to photobacterial systems demonstrates its integration with other efforts.
- The approach detailed in 2011 involved identifying genes to enable a truncated antenna size by performing a forward mutant screen and then using fluorescence imaging analysis to screen the mutants. The researchers identified three *tla* mutants and are currently in the process of phenotyping and genotyping the mutants. All of the methods are appropriate for analyzing genetic mutant strains, both genetically and biochemically.
- The presenter had a clear plan of action that had a high chance of succeeding. The PI used classic genetic techniques to identify the genes of interest and has identified structural motifs of those proteins.
- The experimental approaches are well designed, logical, relatively straightforward, and focused on identifying and characterizing genes/proteins involved in determining chlorophyll antenna size. The PI is expanding the studies in *Chlamydomonas* to cyanobacteria, which is appropriate. It is not clear whether experiments are addressing potential environmentally induced regulation or signaling, but this may be outside of the scope of the project and related funding. The focus on creating uniformly small antennae seems appropriate because these strains will most likely need to be limited to closed photobioreactor systems where light intensity, quantity, and quality can be controlled.
- The approach to achieving the objective has proven very effective. Nevertheless, it is based only on what is known, and no effort is apparent to develop greater understanding of photoactive processes that could identify alternative routes to improving light utility by the light harvesting ingredients of the same or other photosystems.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.7** for its accomplishments and progress.

- The progress is significant and consistent toward the project goals.
- This project has achieved all of its milestones ahead of schedule.
- The accomplishments and progress are clearly outstanding, but the rate of achievement seems remarkably slow. That perception could well be a figment of the admittedly deep ignorance of this reviewer. It is not possible for this reviewer to judge whether the progress is limited by attention and effort level, technical difficulty, or the inherent slowness of research steps.
- It is clear that the PI has made great strides in what is presented as a key element to the project goals. However, the same protection of intellectual property rights may have prevented the presenter from presenting as complete of a story as perhaps might have been otherwise presented. This is not a criticism of the progress, however. It is clear that the project goals are being met.
- For all three *tla* mutants, it appears the researchers have made significant headway. *tla1* appears to be tentatively identified as a variant of MOV34/MPN-containing proteins. With *tla2*, there appears to be the most significant progress with regard to determining where the genetic insertion occurred and what genes were affected and responsible for the phenotype. A patent application has been filed for the gene. The *tla3* effort is making headway, with genetic and physiological characterization completed and biochemical analysis to be finished soon. The researchers have published several papers in well-recognized journals in the past funding year. The elucidation of the genes that affected *Chlamydomonas reinhardtii* antenna size could be applied to other microalgae because, as the researcher states, *tla1* and *tla2* are highly conserved. It also appears that the PI is making great strides in achieving DOE targets for sunlight utilization efficiency and reduction of chlorophyll antenna size, specifically with the *tla3* strain.
- The team has made excellent progress overall in identifying and characterizing genes that are important for chlorophyll antenna size and reaching (and potentially exceeding) the targets. As stated at the presentation, this project was the first to appreciably improve photosynthetic efficiency. The PI also notes that interest in this research has broadened from microalgae to higher crop plants. These studies could lead to increased efficiency of solar energy capture and, in turn, an associated increase in photobiological $H_2$ production. The only issue is whether the project has evolved to focus more on photosynthetic efficiency rather than $H_2$ production, leading to

a question of relevance and direct impact for the Hydrogen Production and Delivery sub-program. Perhaps this concern is more a matter of how the project was presented, because light capture can certainly be considered a barrier in regard to increasing photobiological $H_2$ production.

### Question 4: Collaboration and coordination with other institutions

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

- The project is well coordinated with NREL and others that will use the technology to produce $H_2$ and other products.
- This reviewer does not recall the researcher describing collaboration or coordination with other institutions. This has not seemed to hamper his progress.
- This project has not required significant collaboration to achieve its goals. However, others have sought collaboration with it to achieve their goals.
- There are no specific collaborators, although the mutant strains and techniques are being used by others, including the National Renewable Energy Laboratory (NREL), for $H_2$ production and for increasing the biomass and production of polyunsaturated fatty acids. The *tla1* strains are shared among numerous universities, industries, government laboratories, and high schools.
- The research is primarily conducted at the University of California, Berkeley, with some collaboration with researchers at NREL (which appears to be more providing strains). However, strains are being made available through the *Chlamydomonas reinhardtii* resource center, which allows them to be used by other university laboratories, industry, and government laboratories (and even high schools). There has also been commercial use of the Tla approach.
- There is clear awareness within the Program of work undertaken in this project. There is further indication of intention by others to incorporate advances in this project in other work. At the same time, there is no evidence of effort on the part of the PI to involve other institutions or organizations in furtherance of the Program's objectives. Additionally, there is no evidence of effort on the part of either the Program or other institutions or organizations to attempt collaboration or coordination that might serve to accelerate project progress or to accelerate integration of its progress with activities that would presumably benefit from improved light harvesting efficiency.

### Question 5: Proposed future work

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

- With the exception of some final analyses and publishing the results, the project is complete. The extension of the approach to improving the efficiency of photobacteria is commendable.
- One reviewer is looking forward to the completion of the work on Tla3.
- Currently the researchers plan to finish analysis of *tla3* strain to proceed to eventual publication. The TLA concept in cyanobacteria is currently in progress. No description was given of the extended photosynthetically active radiation (ePAR) concept mentioned on the slide.
- It was not clear whether the proposed experiments for the *tla* project match the scale of the previous year's work. The researcher might be underestimating the challenges of changing model organisms. Finally, the proprietary designation of the ePAR project prevents another reviewer from accurately assessing the short- or long-term plans.
- The proposed future studies clearly build on previous results and will extend the project to cyanobacteria. These studies are relatively narrowly focused, which is appropriate considering the size of the project in general.
- Mention of the ePAR concept without any further description is of no value whatsoever to this review. The demonstration of feasibility of TLA in cyanobacteria could be valuable, but the absence of planned tasks provides no management tools for the Program to measure or judge progress.

### Project strengths:

- The project seems very well focused, and it is obvious that the PI has a direct plan for completing the project with achievable goals.

- The PI has demonstrated rapid progress and has compiled expressive results.
- This ongoing project has shown significant progress over the last years and is contributing to Program goals toward producing cost-effective renewable $H_2$.
- Strengths of this project include its excellent progress, straightforward and logical experimental approaches, and efforts geared toward developing and sharing tools for modifying chlorophyll antenna size.
- The technical proficiency is clearly outstanding. The researchers have made clear progress toward the project objective.

### Project weaknesses:

- There is some concern about the feasibility of the TLA concept in cyanobacteria, which is significantly different than *Chlamydomonas reinhardtii*.
- This reviewer hopes that more of the proprietary information will be released by the next presentation; the PI may run into stumbling blocks changing organisms.
- There is a question of relevance, specifically whether the project is directed more toward photosynthesis rather than $H_2$ production.
- One weakness is the collaboration and coordination with the photobiological hydrogen production portfolio. The future work planning description is inadequate.
- This reviewer stated that the project has no weaknesses.

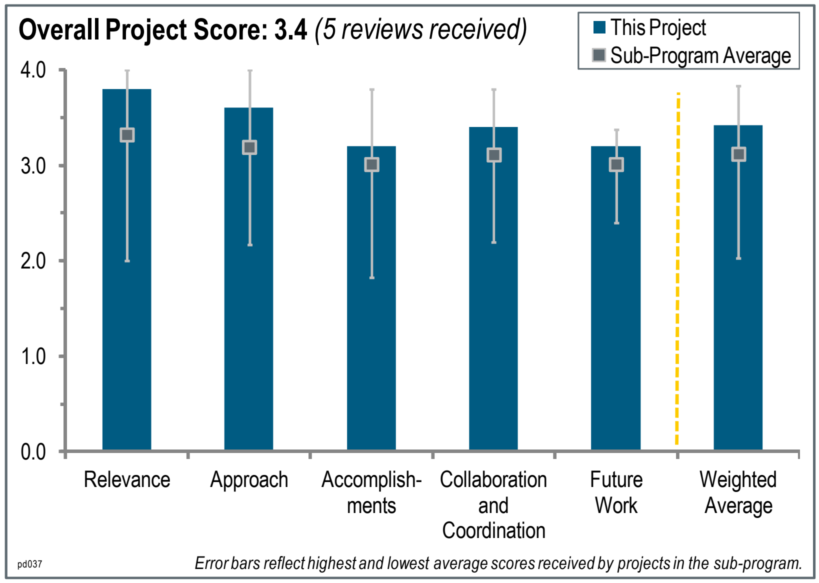
### Recommendations for additions/deletions to project scope:

- The PI should develop collaborations with experts for cyanobacteria experiments and potentially use available mutant libraries for analysis. It would also be nice to see the research on Tla1, Tla2, and Tla3 translated into commercial algal strains.
- The PI's work could be revolutionary and have implications in many fields; however, the project needs to be more linked to $H_2$ and present more data on how these changes to the photosynthetic structures change $H_2$ yields.
- The PI should seek appropriate intellectual property protection and then disseminate ePAR information that is adequate to enable recommendations regarding continued or expanded research and development support.

# Project # PD-037: Biological Systems for Hydrogen Photoproduction

Maria Ghirardi; National Renewable Energy Laboratory

**Brief Summary of Project:**
The objective of this project is to develop photobiological systems for large-scale, low-cost, and efficient hydrogen ($H_2$) production from water. Specifically, the project's two tasks are to: (1) address the oxygen ($O_2$) sensitivity of hydrogenases that prevent continuity of $H_2$ photoproduction under aerobic, high solar-to-hydrogen (STH) light conversion conditions; and (2) utilize a limited STH-producing method (sulfur deprivation) as a platform to address or test other factors limiting commercial algal $H_2$ photoproduction, including low rates due to biochemical and engineering mechanisms.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- This researcher has clearly stated goals regarding $H_2$ production. The principal investigator (PI) has clearly presented goals and tasks from genetics and physiology.
- The project's objective to develop a photobiological system for large-scale, low-cost, and efficient $H_2$ production from water is a clearly stated objective of the DOE Fuel Cell Technologies Program (FCT Program) Multi-Year Research, Development, and Demonstration Plan.
- This project is critical to the Hydrogen and Fuel Cells Program (the Program) and fully supports the objective of cost-effectively producing renewable $H_2$.
- Low STH energy conversion efficiency is the primary shortfall in biological $H_2$ production. This project is directly relevant in that it squarely addresses this issue.
- The project is consistent with and supports DOE research, development, and demonstration objectives for biological $H_2$ production .

## Question 2: Approach to performing the work

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

- The PI assessed that one branch of the project had no chance of success after exhausting all reasonable options and ceased work on it, demonstrating good judgment. The other tasks seem to use classical techniques to solve real challenges in the project and, to this reviewer, seem to be taking the right approach.
- This project is well designed and focused. The researchers are to be commended for recognizing and incorporating innovative new directions to overcome unexpected results.
- After the December 11 no-go decision, the work approach has centered on genetic engineering expressions to characterize in vivo $O_2$ sensitivity, and testing using a sulfur-deprived platform as a testbed.
- The project's suspension of targeted random mutagenesis work to acquire $O_2$ tolerant hydrogenases is commendable and demonstrates risk mitigation by the pursuit of lower-risk alternatives.
- In general, the approaches seem to be reasonable. For instance, attempts to minimize the $O_2$ sensitivity of hydrogenases by introducing the Ca1gene encodons into *Chlamydomonas reinhardtii* have yielded some valuable results. The finding that there are multiple $O_2$ production sites further exposes the challenge of

producing a completely $O_2$-tolerant hydrogenase. Further attempts to identify biochemical and engineering conditions by which STH production using suspension algal cultures can be enhanced are in progress and have shown that headspace volume of $H_2$ is one manageable component to improve rate and yields.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has made steady and significant progress toward project and Program's goals.
- The project has well-defined milestones and decision points and appears to have completed them on schedule.
- The PI understands when to cut a project where no more progress can be made. The PI has an alternate plan in place where results can again be assessed quickly. Progress on task 2, the testing of ATP synthase mutants, seems more limited. Mutants are generated and one is expressed, but any new physiology results were not presented. Results from the continuous $H_2$ production were limited to a single statement about one experiment, and the significance of the headspace observations could be questioned; more understanding of why the organisms change production with different amounts of headspace is needed.
- The finding that there are multiple $O_2$ production sites further exposes the challenge of producing a completely $O_2$-tolerant hydrogenase; however, given that one site has been switched off, it gives hope that through multi-gene stacking or other approaches the team may ultimately find success. Also, demonstrated progress is found in the 565 ml of $H_2$ gas per liter of the suspension culture, which is, according to the investigators, the highest yield ever reported for wild-type strain in a time period of less than 180 hours.
- Researchers have been able to express a double hydrogenase knockout mutant, insert the Ca1 gene, and observe substantially increased light-dependent $H_2$ production. Continuous flow of the medium did not produce the desired results. More should be explained as to what operational conditions might be changed to improve the results. Headspace volume dramatically improved the $H_2$ yield, although the reason is unclear.

### Question 4: Collaboration and coordination with other institutions

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

- The PI seems to have good working relationships with the collaborators; there seems to be a lot of feedback between them.
- Appropriate collaboration exists and appears to be well coordinated.
- Given that the project has been ongoing since 2000, at some point in the near term it would be good to add an industry partner or at least understand why there is no interest. This will be helpful in terms of understanding commercial feasibility.
- This project appropriately and closely collaborates with scientists from Russia, North Carolina State University, Johns Hopkins University, and the Massachusetts Institute of Technology, along with related projects associated with the FCT Program portfolio.
- The collaborators' roles were not well defined in the presentation, but they were expanded upon in the reviewer notes.

### Question 5: Proposed future work

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

- The plans are reasonable and focused on continued progress.
- Continued project funding is determined annually. If funded, the project continues current Ca1 and sulfur-deprivation work.
- One reviewer feels that progress in each task was not obvious, and it was unclear how progress will be made in each task. This reviewer wants to know how the researchers will explore the headspace result. The go/no-go points for many of their other experiments were unexplained.
- Another reviewer very much liked the no-go decision on the more $O_2$-tolerant Ca1 hydrogenase.

### Project strengths:

- The people working on this project have a keen eye for innovation and seem to be good judges for the progress of projects. The collaborations are active and seem to help move the progress of the project along both in terms of physical work and intellectual discussion.
- The project features an excellent team of researchers.

### Project weaknesses:

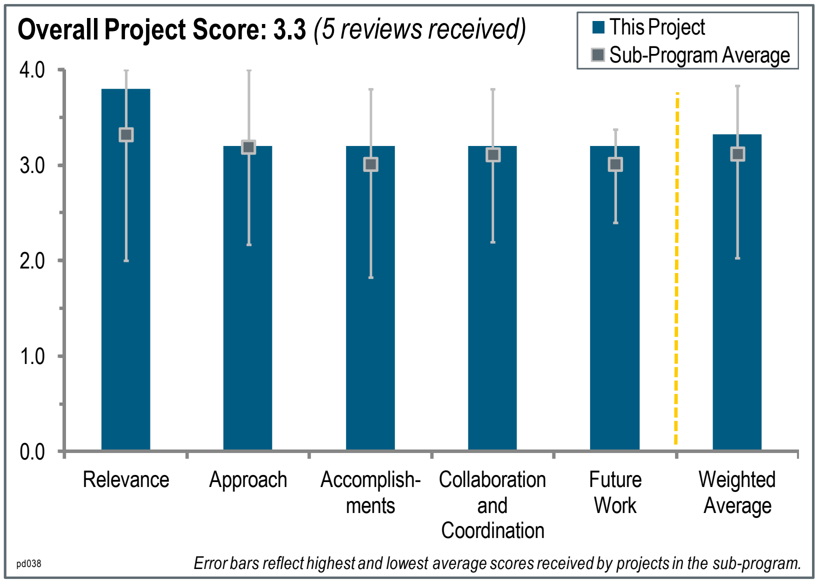
- Task 2 projects need to make more progress, and more explanation is needed of where they are going.
- Researchers have been slow in making significant progress in meeting the DOE goals. This reviewer guesses this is why this project is appropriate for government funding as opposed to industry taking on the risk.
- There is a lack of specificity in the interim goals that makes it hard to assess whether the project is on track or not. Consequently, there should be a fuller self assessment of progress compared to where the PI thought the project would be and what the PI's internal goals are.

### Recommendations for additions/deletions to project scope:

- The headspace experiment seems to be where progress could be elusive. Sometimes great results can come from a simple change that no one has thought of before. More often, though, the reason why the result is new to the researchers is because other's efforts to explain phenomena have gone unpublished due to the inconclusiveness of the results, or because there is a more mundane explanation of the results.
- It is unclear what conditions other than headspace will be modified in order to increase $H_2$ production yields in suspension algae. Also, an industry partner might be helpful in terms of understanding what is commercially reasonable as opposed to being strictly technologically successful.
- The headspace results need to be better examined. Headspace is likely not the correct parameter; rather, gas composition, mixing, pressure, or something else seems to be having an effect. This reviewer recommends that researchers propose a theory and then experimentally confirm that theory.

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

Pin-Ching Maness; National Renewable Energy Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) develop direct fermentation technologies to convert renewable lignocellulosic biomass resources to hydrogen ($H_2$); (2) address feedstock cost and improve the performance and durability of bioreactors for $H_2$ production via fermentation of lignocellulose; (3) improve $H_2$ molar yield (mol $H_2$/mol hexose) via fermentation; (4) improve plasmid stability in *Clostridium thermocellum*; (5) develop an alternative forward-evolution strategy to block ethanol production; and (6) improve $H_2$ molar yield (mol $H_2$/mol hexose, N) by integrating dark fermentation with a microbial electrolysis cell (MEC) reactor to convert waste biomass to additional $H_2$.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The presenters had clearly stated goals involving $H_2$ production.
- This project is clearly aligned with the DOE Hydrogen and Fuel Cells Program (the Program).
- This project is clearly relevant to the Program's objective of cost-effectively producing renewable $H_2$.
- The project is consistent with and supports the objectives of The Fuel Cell Technologies Program Multi-Year Research, Development, and Demonstration Plan.

### Question 2: Approach to performing the work

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

- The integrated approach with the National Renewable Energy Laboratory and the Pennsylvania State University is a highly effective way to increase the molar yield of $H_2$ production using more of the biomass.
- The approach is logical and well constructed. It is still unclear whether the desired approach is a co-culture or a single organism for the fermentation. The presentation presented results for a co-culture, but this reviewer comments imply that only *Clostridium thermocellum* is being pursued.
- The project is an interesting blend of bioreactor optimization, metabolic engineering, and a novel science and engineering approach to $H_2$ yield improvement that has very different risk profiles, which provides some confidence that at least the lower-risk objectives (if not all) will be achieved in a timely fashion.
- The principal investigator (PI) had a great grasp of the mechanics involved in the production of $H_2$ and applied that knowledge to build a better bio reactor. The approaches on the metabolic engineering side were, at times, confusing. It is not clear why the PI chose to make a *dcm* strain when many are commercially available. It is not clear to this reviewer how exposure to acrolein alone will result in inactive alcohol dehydrogenase (ALDH) without further rounds of mutagenesis, or how these mutants will be isolated if components of the media are detoxifying the acrolein, as was implied by the PI. Furthermore, it is unclear what effect shutting off the ethanol

pathway will have on the organism. It is not clear whether this is a guarantee of more acetate production, as the conversion of the acetyl-CoA path to ethanol may serve a physiologically relevant function.
- The trend data for *Clostridium thermocellum* and co-culture degradation of cellulose comparing cellulose concentration versus $H_2$ rate and yield provides very useful information. The results from modifying the sequencing of cellulose in the fed-batch reactor are also helpful. Also, the attempts at genetically modifying the cellulose to switch off ethanol production so that more $H_2$ is produced seem reasonable. However, it is not clear why MEC has been added to this project.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Each partner in this project has made significant progress on its own and collaboratively.
- The project has well-defined milestones and, with the exception of the MEC work, appears to have completed them on schedule. The MEC encountered contract and funding issues that created understandable delays, but it appears to have recovered very well.
- The findings that the co-culture produced 31% more $H_2$ than the *Clostridium thermocellum*, and that $H_2$ output of co-culture on untreated corn stover is 74% of that of *Clostridium thermocellum* alone advances the project toward the DOE cost and yield goals. Also, the production of the stable mutant strain *Clostridium thermocellum* defective in alcohol dehydrogenase for pathway knockout construction is a key advancement.
- Demonstration that the active microbes are immobilized on cellulose, and thereby allow easy separation of the growth medium from the acclimated culture, is a significant step toward scale-up and efficient, low-cost operation. The team achieved a milestone with the 20% increase in $H_2$ production and demonstration of scalability. The use of co-culture allows greater $H_2$ production and use of untreated cellulose. The team also developed protocols and mutants to increase $H_2$ yield and block ethanol production.
- Clearly the author has progressed in the batch reactor experiments, which is to be commended. It is unclear to this reviewer why the *E. coli* used had to be the conjugation strain. Many strains are capable of conjugations, and other techniques, such has tri-parental matings, solve the problem of transferring plasmids to different species. It is concerning that researchers in the presenter's laboratory are re-inventing wheels (making unnecessary strains) rather than exploring other published options for moving plasmids around. That being said, the presenter's laboratory deserves credit for developing this genetic tool. The researchers now need to exploit their tools as quickly as possible to perform their genetic experiments. This reviewer cannot tell whether the authors are on track to achieve the milestone of making an ALDH pathway knockout construction. It seems that as presented, the project is in its very early stages. The presenters do seem to be on track for the electrochemically assisted microbial fermentation, and the compositional analysis.

### Question 4: Collaboration and coordination with other institutions

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

- This project features excellent collaboration.
- The researchers might need closer contact with Dr. Levin and Sparling to keep the genetics project on track.
- Project collaboration appears to be well coordinated and feature strong partners.
- It is unclear why there are not more collaborators on this project. It would be helpful to have industry interest.

### Question 5: Proposed future work

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

- The results from the pathway knockout mutants and the bioreactor synthesis optimization make logical sense.
- Both collaborators in this project have plans that are clearly built on past progress and are reasonable and focused toward reaching the project and Program goals.
- There is a well-specified listing of future work. However, this reviewer would like to see more specific performance targets for each task.

- Continued project funding is determined annually. If funded, the project will continue bioreactor performance optimization, metabolic engineering, and electrochemically assisted fermentation efforts.
- The fermentation experiments have clearly defined goals and expected outcomes. The researchers have built their *Clostridium thermocellum* strain with the stable plasmid, now they need to execute their counter screen for their *pyrF* knockout. This reviewer would like to see more details of how that will be accomplished. This reviewer wants to know what media will be used and what phenotypes are expected. Once they isolate their mutant, the measurement of $H_2$ yields should be completed rapidly. It is not clear to this reviewer, given the researchers' media conditions and lack of strategy for generating mutants, how they will select for allyl alcohol killing survivors, what phenotypes they expect, or at what point they know that the project is a dead end. The goals for task 3 seem unambitious and seem to not solve a central problem of how this system will ever produce a net gain in usable energy.

### Project strengths:

- There are some excellent collaborations that allow presenters to create new ideas.
- It looks like the PI is accomplishing a lot in spite of inconsistent funding.
- The demonstration of a cyclically operating batch reactor showing the scalability of $H_2$ production is a major accomplishment of the project.

### Project weaknesses:

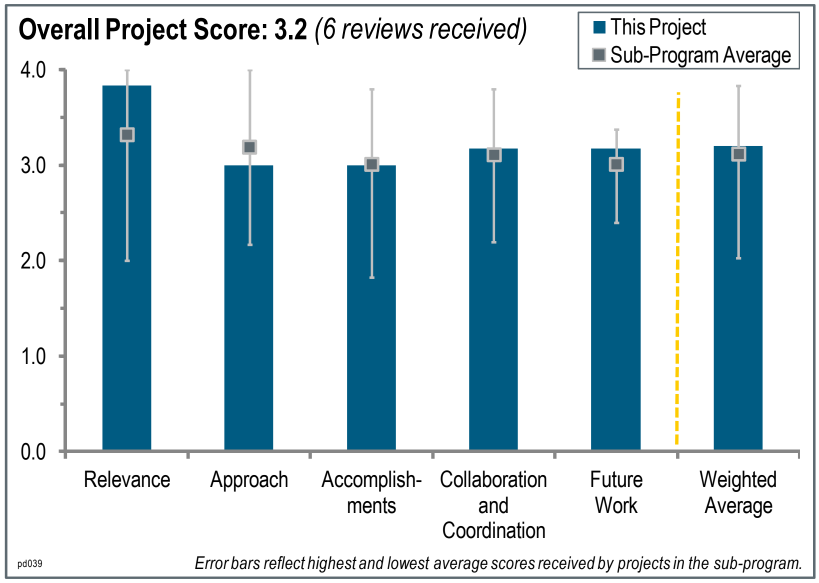
- Some of the historical decisions made by this group can be questioned. The researchers have accomplished some of their goals; they need to capitalize on those successes to continue making progress toward their goals.
- It is not clear why MEC is necessary. This reviewer wants to know whether this adds complications to the fermentation process.
- This reviewer is concerned about the size (and thus cost) of the MEC reactor.

### Recommendations for additions/deletions to project scope:

- Given that the researchers have developed a genetic tool for *Clostridium thermocellum*, they need to focus on all they can accomplish with that system. New mutant hunts may not be in the laboratory's best interest unless the researchers can identify the locus of each mutation. There needs to be clear go/no-go checkpoints for task 3.
- This reviewer recommends that more detailed metrics be established for the MEC reactor, specifically dealing with the size of the required electrode system. This reviewer is concerned that the diluted liquid feedstock coupled with a low electrode current density will require a large reactor system that is cost prohibitive. Current density should be a reported parameter from the MEC research. Instead of applying an MEC voltage bias, it was suggested that heat could serve a similar purpose. The team should expand on this possibility as a potential pathway to system cost reduction.

## Project # PD-039: Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacterial System

Phil Weyman; J. Craig Venter Institute




### Brief Summary of Project:

The objective of this project is to develop an oxygen ($O_2$)-tolerant cyanobacterial system for continuous light-driven hydrogen ($H_2$) production from water. The project hopes to demonstrate a fivefold increase in hydrogenase activity from environmental hydrogenase and cyanobacteria as measured by in-vitro $H_2$ evolution assay, and to improve hydrogenase-ferredoxin (Fd) electron transfer to enable a 25-fold improvement in Fd docking to hydrogenase.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The identification and optimization of $O_2$-tolerant hydrogenases is important for the ultimate goal of producing $H_2$ during photosynthesis. This is very relevant to the long-term pathway goals and objectives of the Hydrogen Production and Delivery sub-program related to the biological production of $H_2$.
- The project goal is to identify $O_2$-tolerant hydrogenases and move them into model cyanobacterial strains, or to improve $H_2$ production by the native enzymes.
- Project objectives are clearly focused on the DOE Hydrogen and Fuel Cells Program's (the Program's) objective of cost effectively producing renewable $H_2$.
- The project's focus on developing $O_2$-tolerant hydrogenases should provide significant tools and insights for improving the continuity of $H_2$ production. Thus, it has clear relevance for the Program.
- Resolving the continuity of $H_2$ production is only one of many barriers to effective photobiological $H_2$ production, so the lack of coordination of this element with other facets of the problem could waste resources and prevent the wise allocation of resources according to prioritized needs.
- The project is consistent with and supports DOE research, development, and demonstration (RD&D) biological $H_2$ production. The discovery or engineering of a cyanobacteria possessing an $O_2$-tolerant hydrogenase would be an enabler to continuous biological $H_2$ production, overcoming a major DOE RD&D barrier.

### Question 2: Approach to performing the work

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

- The J. Craig Venter Institute (JCVI) and National Renewable Energy Laboratory (NREL) groups have a systematic path detailed in the milestones that is appropriate for the work. The groups are well equipped to complete their objectives in a timely manner. Their genetic approaches are suitable, although this reviewer feels that looking at levels of RNA and protein produced may be beneficial to understanding what genes are "more important" in the environmental hydrogenase project instead of just putting more promoters in front of genes. From the western blot of pre-HynL to HynL, it would further suggest that something else is potentially limiting and needs to be determined. The T7 polymerase strategy seems to be quite interesting and will hopefully be useful for this project.

- Parts of the principal investigator's (PI's) approach are excellent. The identification of hydrogenases and the synthesis and cloning of the genes is efficient and effective. The second part of the project, increasing expression of the genes of interest, failed to be logical as presented. It was not clear why breaking up the single operon into four operons would have changed protein yields. The use of the T7 promoter system will likely increase protein yields, but, unless modulated, the levels of individual proteins may increase to an amount such that other cell functions would be impeded. A much more complex question not addressed in the presentation is the question of what ratio of the various protein components is required to allow functional hydrogenase complexes to be formed.
- The two collaborating research groups have individual approaches that are integrated to address the necessary barriers—the project is well designed, feasible, and integrated.
- The research strategy is logical and straightforward with clearly stated milestones. Both JCVI and NREL have access to unique resources that have been valuable to the research. The criteria for describing the enzyme as a thermostable, $O_2$-tolerant hydrogenase were not discussed in detail. For instance, it is unclear what temperature range resulted in the term "thermostable." While relatively well designed, it seems that in some instances, the approaches could use less "brute force" and be a bit more strategic. Alternative approaches were not considered in detail—at least in the presentation. For instance, methods to increase gene expression seem to focus more on adding more promoters, operons, etc., than on tweaking the expression of different components. Perhaps it is not a matter of adding more, but of changing protein ratios. There was also little discussion or consideration of physiology, although this can play an important role in $H_2$ production in these systems.
- There is no particular evidence provided that underscores the need for simultaneous pursuit by separate institutions of the same objective through development of two distinct constituents with the same purpose. Such lack of focus hinders accelerated progress. There is no a prior evidence presented to select one over the other, but it is unlikely that two parallel efforts will make a difference in the face of the millions of possibilities. There might be cogent arguments commending the approach and the selection of options, but such arguments were not apparent to this reviewer. From a management perspective, prime recipient milestones are essentially absent for nearly two years, preventing any effective oversight of project progress. In the same spirit, a complete lack of quantitative performance milestones for both participants could permit this project to continue indefinitely without significant contribution to the photobiological $H_2$ production portfolio. Whereas hydrogenase activity has been improved through promoter engineering, it remains unclear "how much is enough." A "100-fold increase" might be well below useful levels of activity, depending on the starting point. The prime recipient approach to seeking natural $O_2$-tolerant hydrogenase organisms appears to be significantly deficient. One might, for example, assume that $O_2$-tolerant hydrogenase organisms would prevail in $O_2$-rich waters of the global ocean so that a search for such should be driven by the distribution of ocean chemistry, a distribution that is well documented through years of surveys and samplings by physical and chemical oceanographers. No apparent effort to coordinate the sampling with ocean chemistry is evident. If, as suggested by the presenter, it is true that all samples were taken from "oxygen-rich surface waters" then one must wonder why the global sampling project should have been tapped to gather samples at all.
- NREL and JCVI are well integrated in pursuing their parallel efforts to acquire a cyanobacteria possessing an $O_2$-tolerant hydrogenase. NREL is developing two *Synechocystis* recombinants that possess different promoter arrangements. The pursuit of two approaches, as well as two recombinants in one approach, to develop an adequate organism is a valid risk-management strategy for higher-risk research.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The PIs appear to be making good progress toward reaching their milestones, and it is commendable that they have a go/no-go decision established with a definitive hydrogenase activity goal. The *Rubrivivax gelatinosus* Casa Bonita Strain (CBS) is a difficult system and it appears they have made good progress. The identification of homologous hydrogenases from the global ocean sampling project is quite interesting, although it appears that there is much work ahead to see whether it will be a viable option. Although this reviewer is aware of the difficulty of the potential project, this reviewer does wonder how many truly novel hydrogenases are being overlooked because they are not homologous to known hydrogenases.

- The presenter claims 80% completeness, but some of the final goals to be achieved will be the hardest, for example, bringing $H_2$ yields to the target number. There is no guarantee of a linear extension of the current data. Some of the "accomplished" goals—cloning and expression of genes—are not noteworthy.
- The project has made significant progress toward the objectives and overcoming the barriers.
- Overall, the progress toward the stated goals has been good and steady; however, there is some question whether alternative approaches are being adequately considered that could improve progress. CBS is a challenging organism in that tools are lacking compared to model systems, but the availability of the sequence should enhance progress. Indeed, analysis of the genome revealed a new set of hyp genes. While appreciating the value of using homology to potentially identify novel environmental hydrogenases, this reviewer feels that the researchers may be missing some truly novel hydrogenases that do not necessarily look like a conventional hydrogenase at the sequence level, but nevertheless function as an $O_2$-tolerant hydrogenase. Identification of such an enzyme could be extremely challenging, but if it could be found, it could provide some very important insights into $O_2$ tolerance and enzyme structure and function.
- Progress is made evident in the development and displayed activity of both of the $O_2$-tolerant hydrogenase choices, but there is insufficient evidence for this reviewer to judge the likelihood of useful success because of the lack of quantified barriers.
- The project has well-defined milestones and decision points against which to compare progress. The project appeared to complete this period's milestones on schedule. Task 2 had only one milestone during this period—and a 20-month interval between that and the next milestone—so progress against milestones was difficult to judge.

### Question 4: Collaboration and coordination with other institutions

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

- The overall milestones and presentations indicate that the two institutions are working in parallel, but that information is being shared between the two groups. The two primary institutions are also collaborating with other universities and companies for needs outside of their expertise.
- The presenter detailed collaborations that are spread out over many institutions.
- There is significant and fruitful collaboration between the project researchers and also with other institutions.
- The research appears to be well coordinated between JCVI and NREL, with good communication and sharing of results. The integration between the two institutions could be better illustrated, as they seem to be working more in parallel and synergism between the two groups is not obvious. The additional collaborations appear to be appropriate.
- It appears to this reviewer that the two institutions are preceding pretty much independently along two separate paths toward a common objective. Whereas there is some level of supportive participation by others, there is no strong evidence of collaboration between the two primary institutions. There is evidence of coordination and communication, but the accelerated progress that could be afforded by true collaboration and by focused work on a single pathway is not evident.
- In addition to the project partners, NREL and JCVI, collaborations exist with Vanderbilt University and Michigan State University, as well as with free genome sequencing from Pacific Biosciences.

### Question 5: Proposed future work

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

- It appears that there is a definitive, logical plan to build on previous progress. There is still much work to be done on engineering cyanobacteria and increasing the $H_2$ amounts to levels that would be suggestive of viable production strains.
- The project has many significant challenges ahead, and therefore it has clear goals. It is no small feat to increase the protein expression 100-fold, achieve the level of $H_2$ production targeted, and maintain the viability of the culture. Generating gain-of-function mutants that have improved electron transfer is a difficult genetic task and has a strong possibility of failure. If the foreign hydrogenases are not linked to the natural systems of the model organisms, linking the two may require another round of mutagenesis or more engineering of foreign genes.
- The future work is well laid out and reasonable.

- The proposed research is logical and clearly builds on previous studies. The PIs have set some quite challenging research goals and appear to be cognizant of the challenges of the systems under study.
- The proposed future work is much along the same lines as the current work and is subject to concerns.
- Continued project funding is determined annually. If funded, Task 2 will pursue existing milestones and Task 1 will extend the existing work.

### Project strengths:

- The researchers have developed a well-planned path with milestones. It is nice that they have an applied goal of increasing hydrogenase activity from the environmental hydrogenase in cyanobacteria by fivefold for the continuation of the project.
- The researcher has a large library of genes to work with to find other genes that may be required.
- Development of a stable, $O_2$-tolerant hydrogenase is an important goal. Overall, the project has a logical approach and complementary expertise between the research groups.
- Strengths of this project include its technical proficiency, equipment, and facilities.

### Project weaknesses:

- While there has been progress on the goals set forth by the investigators, the translation of this work to a scaled-up system seems to still be very far off. It appears there is much ground to be covered with both the CBS and environmental hydrogenases expression and the overall effect on the physiology of the cell due to the expression of these genes. The investigators mentioned that a reviewer hit upon this last year, and that they are addressing the issues. It would have been nice to hear more about what avenues they are taking in that regard.
- It seems that some of the biggest challenges of the project are ahead of the researchers. Obtaining the activity goals is not guaranteed. If the foreign and native systems are not linked, that will be a tremendous undertaking, and the undertaking of a “forward screen” mutagenesis project to improve electron transfer is a potential non-starter.
- Alternative approaches and strategies to fine-tune expression could have been better addressed. While the researchers clearly appreciate the complexities of the pathways, it was not evident from the presentation whether the experimental approaches were effectively addressing these possible complications. Physiological effects were not addressed in a comprehensive way.
- The collaboration within the project and with other institutions, organizations, and the community of experts was noted as a weak point. The project also failed to provide future planning and work descriptions with sufficient information to provide program management with metrics to judge progress. The project strategy to pursue a two-track path is questionable because that hinders progress without any clear basis for such an approach.

### Recommendations for additions/deletions to project scope:

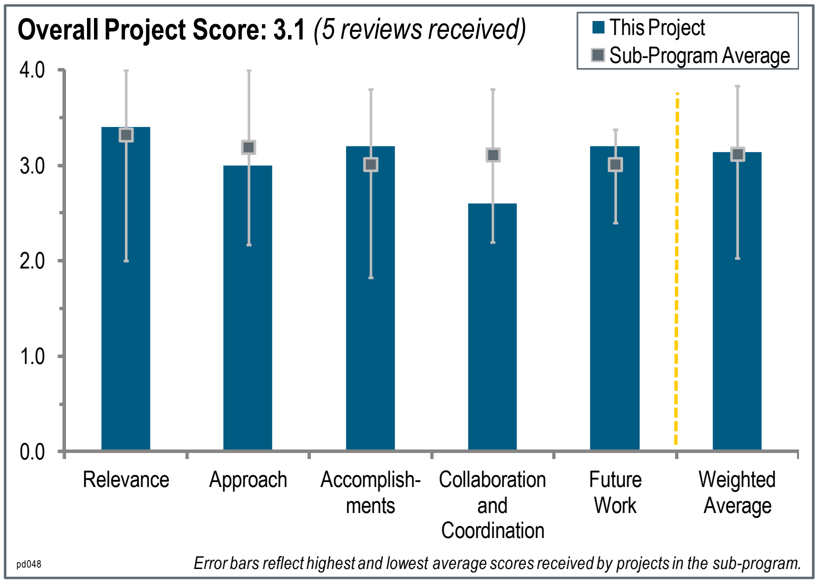
- The current position of the project is such that there is no guarantee that trimming and refocus will improve the chances of success. The authors have many irons in the fire, but many of them strike this reviewer as being high risk. Success in any of the stated goals will be significant, but focusing on one over the other may be too much of a gamble.
- Absent a compelling basis for independent development of distinct $O_2$-tolerant hydrogenases, the project should be encouraged to select one path and establish effective collaboration, and thereby accelerate progress to test and demonstrate a single $O_2$-tolerant material.

## Project # PD-048: Electrochemical Hydrogen Compressor

Ludwig Lipp; FuelCell Energy, Inc.

### Brief Summary of Project:

The overall objectives of this project are to: (1) design and develop an electrochemical hydrogen ($H_2$) compression system that is more reliable than mechanical compressors; eliminate contamination from lubricants, thereby increasing $H_2$ quality; (3) improve compression efficiency (goal 95%); and (4) reduce the cost of gaseous $H_2$ delivery to meet the U.S. Department of Energy (DOE) long-term targets.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.4** for its relevance to DOE objectives.

- The goal of a highly reliable and low-cost compressor is highly relevant to the DOE Hydrogen and Fuel Cells Program (the Program) objectives.
- From this reviewer's point of view, this is only a long-term solution.
- The technical concept is brilliant, but this project needs to be funded under a Basic Energy Sciences budget that recognizes a 20-year development horizon. For the Program, the project has little relevance.
- It is great that DOE is funding turbo pumping as well as the electrochemical $H_2$ compressor (EHC). It is too soon to tell which will win or, more likely, where the boundaries are that favor one approach over the other. The authors presented very credible work with good prospects for scaling up. The possibility of EHC incorporating the rapid technological advancements in fuel cells adds impetus to the project.
- Hydrogen compression is definitely one of the critical and relevant aspects on $H_2$ refueling. Electrochemical compression is a very promising technology, and this project in particular is addressing several key areas, such as reliability, efficiency, and cost, in order to make this technology a very competitive one.

### Question 2: Approach to performing the work

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

- The technology is on the right path and development work will be slow, but the progress to date is meaningful and the potential is huge for many industries.
- The approach seems focused on the right issues and appears to be disciplined. The researchers are making good progress with small cells. Scale-up to somewhat larger systems could be as simple as adding units, at least for the near term.
- There are a lot of possible knobs to turn; the researchers should identify the most critical ones.
- The approach presented in this project was well defined, but the key parameters should have been highlighted and presented in more detail, along with discussion of how these parameters will impact the performance, durability, and cost of the system.
- The general approach appears to be correct, but the specifics of the research are not provided. As an example, the list on slide 5 is nice, but details are not provided regarding progress to these goals. This would be equivalent to indicating that the onboard fuel cell stack needs to lower costs without explaining the specific technology used to achieve that goal. In particular, the progress made by EHCs 1–7, as referenced in slide 11, should be further explained.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The accomplishments in terms of pressure, current density, and design are magnificent, but the overall rate of development is slow.
- The project showed good progress with regard to durability and energy consumption, and it demonstrated the feasibility of 12,000 psi. The cost target is far off.
- The future is very bright. The theoretical specific energy consumption is a big improvement over mechanical compressors; however, other losses reappear, so the two technologies seem fairly similar in performance. This reviewer worries a little about whether the three-stage mechanical compression numbers include all of the tricks, such as intercooling, etc. However, the simplicity and the lack of moving parts are terrific.
- Several technical accomplishments were presented for this project, including the demonstration of single-stage pressure capability above 12,000 psi. It would have been very beneficial to present more details on the improvements made that had the greatest impact on this accomplishment.
- Achieving 12,000 psi compression in a single stage was a very good accomplishment. The pressure needs to be greater for the current assumption for 70 MPa onboard tanks.

### Question 4: Collaboration and coordination with other institutions

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

- It may be due to intellectual property issues, but the collaboration seems limited.
- Only one collaborating partner was presented. It would be very beneficial to partner with an electrolyzer company with experience in high-pressure electrolyzers.
- Given the potential for this technology, it seems that the project team is not doing enough to develop collaborations with organizations that understand the technology’s potential and want to help fund commercialization.
- This reviewer rates this as “good” because of the potential to utilize advances in fuel cells. This reviewer would really like to see collaboration or a test at Linde or another specialty laboratory, or at a refinery (they use a lot of high-pressure $H_2$).
- The collaboration with Sustainable Innovations appears to be effective for the cost analysis. It may be worthwhile to seek other collaboration with electrolyzer suppliers to understand the full extent of commercialization costs.

### Question 5: Proposed future work

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

- This reviewer strongly recommends that the future work plan includes collaboration with well-funded, interested parties that can help accelerate the development and commercial awareness of the technology.
- The future is bright. This reviewer is very curious about other applications the researchers may be interested in besides pipelines. This reviewer would encourage funding opportunity announcements that would promote small-scale applications of high-pressure electrolysis systems (which are related to EHC).
- The team should perform cost analysis based on a “perfect world” assumption and include life-cycle cost compared to other technologies.
- The proposed future work seems reasonably good, especially in the areas of scaling-up and stack designs for higher pressures. Durability and cost analyses at higher pressures should be considered in future plans.
- The future work appears to be focused on durability and cost reduction, which are the key barriers for this technology.

### Project strengths:

- This is a truly passive compressor.
- The small-scale modular approach is very versatile and can provide inexpensive analysis and development.

- This project features a simple operating principle with no moving parts.
- This is a very strong and promising technology that could have a significant impact on the $H_2$ refueling stations rollout and addresses the challenges associated with conventional compression technologies in terms of reliability and efficiency.
- The project has made significant progress in advancing toward higher compression ratios.

### Project weaknesses:

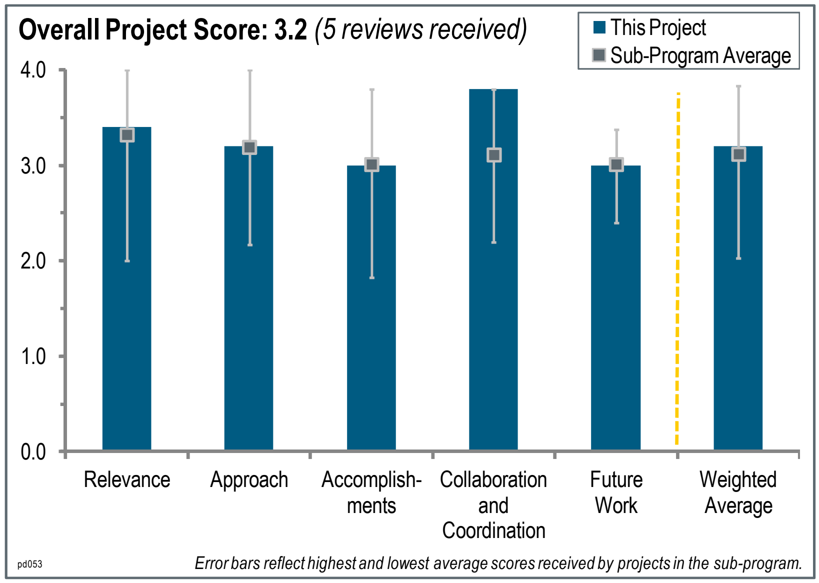
- There is a lack of development funds and capable technologists to accelerate development and commercialization.
- Sometime soon, the issue of the purity of the feedstock will have to be addressed. More realistic long-life tests are also needed, for example switching between on and off, temperature excursions, and impurities. It is a very large multidimensional space of horrors.
- The hard-to-reach cost target is an area of weakness.
- The project has limited partners.
- The project does not have a clear path to achieving the DOE cost targets. The project overview could be improved by providing specific details regarding the past and future design changes. Also, it would be useful to provide additional test data for the units being tested.

### Recommendations for additions/deletions to project scope:

- One reviewer recommends keeping this project alive, but finding a more productive development path.
- Another reviewer would encourage more effort in small-scale applications. The modular approach allows for easy scale-up (even if not perfectly elegant or ideal); therefore, if a little one is working, it will be easy to advance to larger applications. Small units should be attractive to a wider market, so economies of scale (manufacturing a lot of small units versus a few big ones) could get the ball rolling faster. The team should think laptop and cell-phone-scale units, not big centralized utilities.
- The team should compare the technology to other compression technologies in PD-014. The researchers should also perform cost analysis based on a “perfect world” assumption and include life-cycle cost compared to other technologies.
- The team should add additional details regarding the cost analysis, including the possible best case projections. It should also add the evaluation of scale-up to higher per-day capacity to assess potential issues.

## Project # PD-053: Photoelectrochemical Hydrogen Production

Arun Madan; MVSystems/Hawaii Natural Energy Institute




### Brief Summary of Project:

The objective of this project is to develop a monolithic hybrid photoelectrochemical (PEC) device powered by MVS' low-cost amorphous silicon-based tandem solar cell. The hybrid devices will test three materials: (1) amorphous silicon carbide (a-SiC), (2) metal oxides, and (3) copper (Cu)-chalcopyrite-based materials. The goal is for the device to have a solar-to-hydrogen ($H_2$) efficiency of 5% and a durability of 500 hours.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- The project is clearly aligned with DOE Hydrogen and Fuel Cells Program goals.
- Photoelectrolysis $H_2$ production does meet DOE Hydrogen Production Roadmap goals.
- This project matches up well with the DOE objectives for PEC $H_2$ production. The continued development of low-bandgap materials with a focus on improving durability is relevant to achieving the 2013 technical targets.
- Researchers are addressing several of the major photoelectrode contenders in the PEC $H_2$ field.

### Question 2: Approach to performing the work

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

- The group has a focus on developing a variety of materials for PEC $H_2$ production. This effort has a materials focus, which is important because there is currently no particular materials class that has been shown to have both efficiency and durability. Each material also uses a hybrid device design, where an underlying solar cell can used transmitted photons to provide the voltage assist to overcome overpotential issues for the PEC material.
- This effort focuses on three different material classes using the same a-Si tandem solar cell engine. The structure of evaluating the photo anodes and photo cathodes together, and all three material classes in the same laboratory, is excellent.
- Several different pathways are under evaluation—likely too many for this size project. It is unclear whether there is synergy between the different elements of the project.
- The researchers are well attuned to the barriers for each of the three technologies presented and have adopted different strategies to deal with each one accordingly.
- Even though it is understandable that this project is still in the materials discovery and development phase, it is not feasible; there are major technical challenges with this technology.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- It has been tough going on some of the experiments, but overall the researchers have done a good job achieving the stated milestones.

- There are significant similarities among the 2011 and 2012 project objectives and results (slide 4 in the 2011 and 2012 Annual Merit Review presentations). The project milestones on slide 5 show that no significant progress has been made since the 2011 project report. The catalytic activity of a-SiC PEC hybrid devices shows improvements with Ti-$CH_3$.
- In the past year there has been a continuation of effort with respect to advancing the three materials classes under exploration. There was both materials exploration and studies on materials' durability. For a-SiC, modest improvements have been realized using functionally catalyzed surfaces to reduce overpotentials. Exploratory research on the $CuWO_3$ shows that it has an ideal bandgap but poor carrier transport. Preliminary results using carbon nanotubes indicates that providing a high-conductivity scaffold can ameliorate some of the transport problems.
- The team has made very good progress improving the a-Si system using surface treatment. The metal oxide durability issues are still unresolved. The team identified novel device integration structures for the Cu-chalcopyrite system and conducted durability tests for all three material systems.
- Clearly a lot of data is generated. It remains unclear how close the team is to having a viable device or technology; one does not expect the technology to be ready, but one does want to understand the gap.

### Question 4: Collaboration and coordination with other institutions

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

- The project features well-established and functioning partnerships.
- The project appears to have excellent collaboration among team members. The overall PEC Working Group interaction is a model of collaboration.
- This project does a good job of leveraging the extended expertise in the PEC Working Group. These collaborations enable a better understanding of the underlying material properties and help provide direction for future research.
- The investigators seemed to have legitimate interaction with nearly all of the PEC Working Group participants.

### Question 5: Proposed future work

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

- The team has identified well-constructed and specific plans for each material system.
- The proposed future work seems to suggest a more focused approach in the future.
- More detail on how to deal with $CuWO_4$ conductivity issues would have been good.
- The proposed future work takes place on three fronts: (1) a-SiC, there is a general plan to improve the performance via further experiments on lowering overpotentials; (2) $CuWO_3$, accepting that the transport properties are poor, it is proposed to combine this material with Stanford's high surface area electrode; and (3) CIGSe, it is proposed to continue efforts for co-planar integration, which this reviewer views as a photo-catalytic non-precious metal electrolyzer. Although this is a nice exercise, it is not scientifically very interesting if the work is not moving the materials in new directions.

### Project strengths:

- The project offers some novel and outside-the-box thinking and research and development approaches.
- Strengths of this project include the collaboration among team members, organization of the approach, and logical pursuit of each material system.
- Strengths of this project include its partnerships and link to the Program goal.
- The team has made positive developments in three distinct PEC technologies.
- Strengths of the project are its capability to fabricate a wide variety of materials using different fabrication techniques, including the low-cost spray pyrolysis to plasma-enhanced chemical vapor deposition (PECVD)/sputtering in a cluster tool, and the fact that it provides a sophisticated suite of characterization to understand the fundamental properties of each material as it relates to PEC $H_2$ production. The investigators covered a lot of technological ground along three fundamentally distinct materials classes.

## Project weaknesses:

- There are too many material systems.
- No significant progress was made in this project since 2011. Even though it is understandable that this project is still in the materials discovery and development phase, there are major technical challenges with this technology.
- One reviewer would expect the proposed future activities to yield only incremental improvements, not the large jumps needed to meet the DOE benchmarks. Regarding the co-planar photovoltaic (PV)-CIGSe, the promoted benefit of the PEC system relative to a PV-electrolyzer is a reduction in the complexity for the top of the cell (e.g., transparent conducting oxide layer and silver grids). This benefit is lost with the co-planar approach.
- The project is almost exclusively focused on the pursuit of near-term DOE goals (of 5% efficiency and 500 hours) without much discussion of the pathway or retirements needed beyond these goals.
- Another reviewer still wonders about laterally deployed integrated PV systems, but the presenters had references to back them up, so until this reviewer reads the studies, this reviewer will not offer any critiques. This reviewer supposes that much of the project's effort involves new materials development, which has inherent risk attached, but somebody has to do it.

## Recommendations for additions/deletions to project scope:

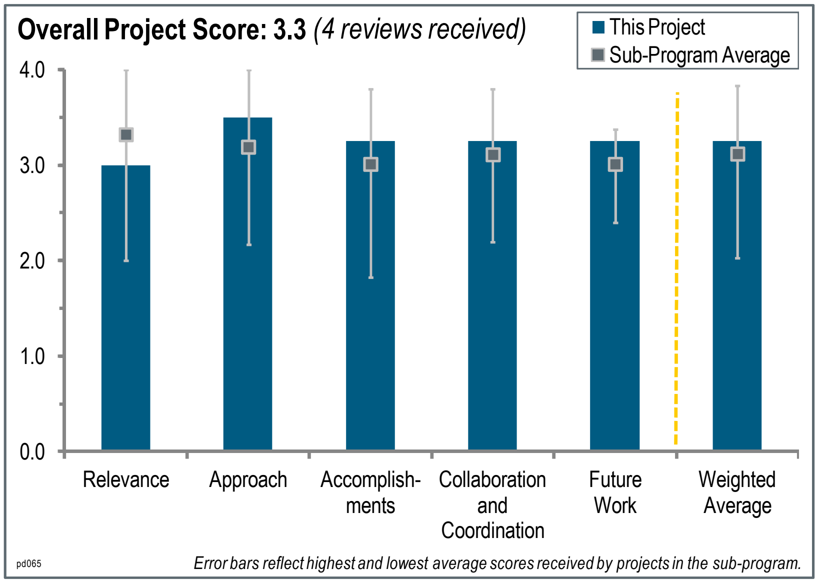
- The team should down-select materials systems.
- The practicality of such technology needs to be evaluated by DOE. There are many technologies that provide significant technical advancements over the current technologies, but they end up not being very practical due to mass production and fabrication challenges, being integrated in an end-to-end system, and high cost.
- In addition to clear targeting of the near-term goals, the team should lay out a pathway to assess and pursue achievement of the longer-term goals (i.e., higher efficiency and longer endurance). There should be an assessment (both quantitative and relative to each material system) of the prospect for meeting the long-term goals.
- It needs to be clearly determined if a-SiC is indeed stable. There are confirmed changes after 500-hour durability tests that need to be fully characterized and understood. There has been an emphasis on giving PEC data as simply photocurrents or incident photon-to-current efficiency. With respect to the ultimate goal of using these materials in an eventual system, this is an extremely important metric. However, as these materials are significantly removed from viability in a system, this data has limited usefulness. As an example, a twofold improvement in photocurrent was presented for the $CuWO_3$ material from 2010 to 2011. At face value this looks impressive, but it is not clear that there was a true improvement (maybe the absorber layer was simply twice as thick). More telling would be data that shows the absorbed photon to current efficiency (i.e., internal quantum efficiency). This would more clearly show how the material is performing on a fundamental level (e.g., trap states and improved photoconductivity).

## Project # PD-065: Unitized Design for Home Refueling Appliance for Hydrogen Generation to 5,000 psi

Timothy Norman; Giner Electrochemical Systems, LLC

### Brief Summary of Project:

The project objectives are to: (1) detail, design, and demonstrate subsystems for a unitized electrolyzer system for residential refueling at 5,000 psi to meet DOE targets for a home refueling appliance; (2) design and fabricate a 5,000-psi electrolyzer stack; and (3) fabricate and demonstrate a 5,000-psi system. The project will develop and evaluate membranes; develop, fabricate, and evaluate electrolyzer cell and stack technologies; and improve the safety and reliability of systems for home refueling appliances.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The project addresses both the technical and economic objectives. It is very focused on the economic objectives.
- The project fully supports the DOE Hydrogen and Fuel Cells Program (the Program) and its goals align with DOE research, development, and demonstration objectives.
- Home refueling supports many of the Program's hydrogen production goals. However, due to its scale, it will have greater difficulty achieving the Program's technical targets (e.g., cost); thus, it inherently does not support the goals to the extent of larger-scale technologies, including distributed production.
- The project has merit, but it is not critical to the Program in this reviewer's opinion. While the idea that refueling can occur overnight using hydrogen ($H_2$) generated at home is a great concept, there are other aspects of the Program that have higher priority, such as reducing the cost of fuel cells and fuel cell vehicles. In addition, the technology for home refueling appliances does not appear overly difficult, and its development now is not timely given the shortage of research and development (R&D) dollars.

### Question 2: Approach to performing the work

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

- The project features an excellent emphasis on safety and meeting codes and standards. It is very focused on meeting the economic objectives. The team has made good innovations that take a whole-systems approach. The investigators also leverage what they have learned in other projects.
- The project has adopted a higher-risk and higher-reward approach by developing a new design. The project has encountered some technical issues.
- The project offers an innovative design for addressing issues such as not requiring a compressor or storage, appropriate new robust materials for higher efficiency, and low cost.
- Giner Electrochemical Systems' approach is well designed and well executed. The team completed R&D in specific components to reach the higher pressures, and also integrated the system. Safety issues were addressed and a preliminary economic analysis of a commercial home refueling appliance system provided projected costs within DOE's targets for when there is a significant sales volume.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The team has made significant progress toward its objectives.
- The presentation included the definition of barriers and showed progress in overcoming the barriers.
- The project features a very good project plan that is being well executed. The project is cost driven, which is excellent, and it is making significant progress toward accomplishing its goals. The investigators have made some technical breakthroughs to reduce costs.
- The project is less than 60% complete with about 85% of the time expended. Critical integration and testing efforts remain.

### Question 4: Collaboration and coordination with other institutions

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

- The project has several industrial partners and a consulting partner.
- The team placed good emphasis on safety using an outside consultant. The project featured good interface with suppliers to drive costs down, and it did a good job of leveraging efforts of other projects.
- Partners include people or companies with expertise in $H_2$ safety codes and components (e.g., nanostructured thin film catalyst and membrane and carbon cell separators) as well as system controls design. The work is well coordinated and complements Giner Electrochemical Systems' expertise.
- Some collaboration exists and partners are fairly well coordinated. Collaboration with the National Institute of Standards and Technology (NIST) or national laboratories could offer more advantages.

### Question 5: Proposed future work

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

- The project is well planned and executed. The investigators will probably need an extension. Two years was probably unrealistic to accomplish the whole project.
- This reviewer thinks that there should be a greater focus on safety because the general American public will use the system; there should be multiple redundancies in the safety system. Costs have to come down.
- The project's proposed future work is to complete the remaining tasks.
- The proposed future work should include collaboration with NIST or national laboratories to make the project more effective.

### Project strengths:

- Strengths of the project include its solid approach, smooth execution, focus on cost reduction, and good technical innovation.
- The project offers an innovative design to address technical barriers and reduce the manufacturing cost.
- Comparison of presentations from Giner Electrochemical Systems and Proton Energy Systems shows that Giner Electrochemical Systems has focused its efforts and is getting the job done. This comparison is valid because both companies started work in August 2010 and have similar funding levels.

### Project weaknesses:

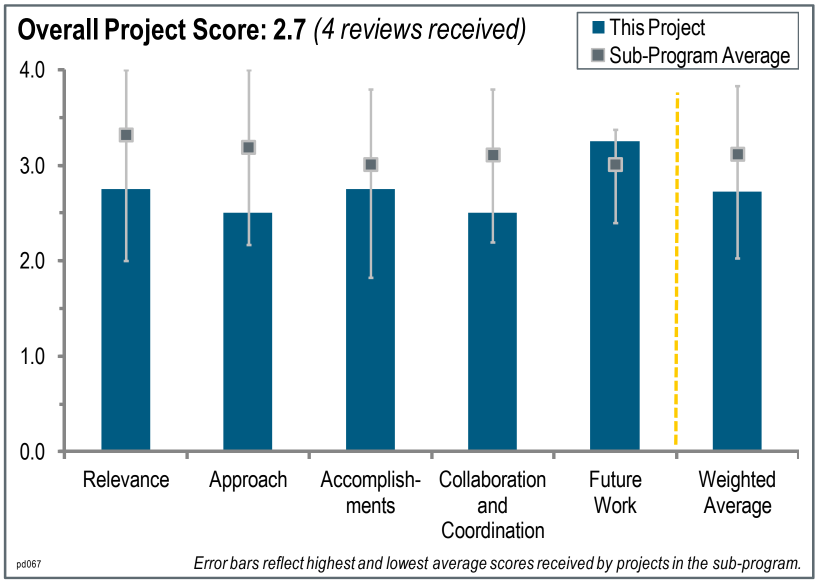
- The acceptance of pressurized $H_2$ systems in the garages of the American public and the associated costs has not been addressed.
- The project is lacking technically strong collaborators such as NIST or national laboratories, which could help in standardizing the process as well as providing technical input to the design.
- This project has no weaknesses.

### Recommendations for additions/deletions to project scope:

- The team should keep going.
- It is a good project, and the project scope is sufficient.

## Project # PD-067: Hydrogen by Wire – Home Fueling System

Luke Dalton; Proton OnSite




### Brief Summary of Project:

The objective of this project is to develop key technologies that will enable home fueling. Specifically, the project focuses on the design and fabrication of a polymer electrolyte membrane (PEM) electrolysis cell stack and balance-of-plant components for 350-bar operation. The project's goals are to develop a system capable of 350-bar differential pressure electrolysis and to demonstrate prototype operation in terms of both hydrogen ($H_2$) generation and fueling capability.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.8** for its relevance to DOE objectives.

- The project aligns with DOE-stated objectives.
- Home refueling supports many of the DOE Hydrogen and Fuel Cells Program's (the Program's) $H_2$ production goals. However, due to the project's scale, it will be difficult for the project to achieve Program technical targets (e.g., cost); thus, the project does not support DOE goals to the extent of larger-scale $H_2$ production technologies, including distributed production.
- The project has merit, but it is not critical to the Program. While the concept that refueling overnight using $H_2$-generated in one's garage is great, there are other aspects of the Program that should have higher priority, such as reducing the cost of fuels cells and fuel cell electric vehicles (FCEVs). The technology for home refueling appliances does not appear to be overly difficult. At this time, using very limited research and development (R&D) dollars for development of this technology is not cost effective. Of concern are the acceptance of the American public and safety issues related to in-home deployment (e.g., kids, weather-related events, and collisions with cars).
- The work is presented by Proton OnSite and demonstrates a prototype home refueling system for $H_2$ wire delivery. The project presented is based on PEM stack technology at 350-bar differential pressure for home delivery to support FCEVs. There is no doubt that the project itself is well aligned to the DOE Fuel Cell Technologies Program's (FCT Program's) mission and goals by demonstrating an $H_2$ refueling system for the homeowner. In fact, a DOE Office of Energy Efficiency and Renewable Energy (EERE) FCT Program Funding Opportunity Announcement (FOA) was recently released on the topic of "Validation of Hydrogen Refueling Station Performance and Advanced Refueling" (FOA 626). This project is in the prototype and validation stage, but the team should consider applying to the FOA to collect and test real-world data for DOE. This week the team won a Small Business Innovation Research (SBIR) project (Phase I, release two) to support its membrane technology. The DOE Hydrogen Production and Delivery sub-program target is to reduce the "delivery" portion of this cost to \$1–\$2/kg by 2020. At this stage, the project does not present a cost analysis of the delivery system.

## Question 2: Approach to performing the work

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

- This is a straightforward systems engineering approach to the technical solution. This reviewer does not see a strong focus on meeting cost objectives. It is not clear if the solution is going to be cost effective. The investigators have not addressed the $H_2$ codes and standards.
- Most of the effort in the last 1.5 years has been expended on design, and comparatively little effort has been spent on testing, identifying, and overcoming the barriers. There was little technical detail on component research (e.g., membranes, membrane electrode assemblies, and seals and separators suitable for 5,000–7,000 psi operation). This work may have been done, but, if so, it was not presented adequately on the poster. Preliminary economic analysis and safety issues were touched on but not addressed.
- The project approach controls risk by extending concepts applied and demonstrated in an existing 2,400 psi design that has more than 20,000 hours of operation.
- The approach to the work appears to be sound and reasonable; however, much information was given on the details of the PEM technology, or stacking details, to assess the relatively improved production performance. Still, the overall approach based on the information released appears to be sound and based on PEM technology as appropriate. Per product development, the system is designed, prototyped, and validated. No concrete details on R&D involved were given, and the project is at present more of a prototype unveiling and demonstration potential stage. Demonstration does not include performance details or production output, which would have been nice for this evaluator.

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The accomplishments and progress appear to be relevant and productive. The team has demonstrated completion of the system design/fabrications component procurement and completed the $H_2$ phase separator. The stack design pressure tests have been completed and the prototype design has been finalized. The stack embodiment hardware is validated along with the cell flow fields and differential pressure testing. Integrated testing systems including power supplies and pump testing are completed. No detailed data was presented on the component output data, but the project presents tasks as complete.
- The project is making good progress toward producing a technical solution. Researchers are hitting their technical objectives. The project has not made any cost projections. The economic viability of the solution will be a challenge. It is too early to determine if the economic goals will be met. Slides 7 and 8 do not match up regarding the status of progress.
- A successful test of the 5,000 psi electrolyzer was completed after hours on May 15. However, the overall rate of progress has been slow. The barriers with respect to the performance of the various components were not addressed specifically. Considering that this project was started in August 2010 (the same time as Giner Electrochemical Systems' project), this reviewer believes that more details on these issues should have been presented. No preliminary cost analyses were completed and it is impossible to determine if progress was made toward meeting DOE's cost and efficiency targets.
- The project is more than 90% complete with less than 90% of the time elapsed. System design and fabrication, stack design, and the majority of component verification is complete, and integrated testing has begun. Testing, packaging optimization, and any economic evaluations that are performed will demonstrate the extent of progress against DOE goals.

## Question 4: Collaboration and coordination with other institutions

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

- The project features a good use of Oak Ridge National Laboratory (ORNL) capabilities for an important aspect of the design work. The team is working with parts suppliers to address costs.
- ORNL is listed as a collaborator for assessing the durability of metallic and coated separator materials. No details were provided either for ORNL or for the industrial suppliers.

- Limited collaboration exists; given the progress made, more collaboration appeared to be unnecessary.
- The team is working with ORNL to provide the characterization and analysis of the metallic separator and its durability. It is not clear to this reviewer why ORNL was selected for this task. It would be nice to understand this selection, and why the ORNL expertise was needed or why the required analysis tools could not be found elsewhere, or in house. No R&D was presented on the stacking design or PEM, but a university collaborator might in the future support a larger project effort. Proton OnSite is well integrated in the community and with its industry component suppliers and end-product users, including DOE.

### Question 5: Proposed future work

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

- The future work is well planned.
- The only remaining project activities are integrated testing, package optimization, and fueling demonstrations.
- The team proposes the future work under the DOE award to include further integration of the operational testing, scale-up of cell count to increase total output, and optimization of the system packaging for siting requirements and cost effectiveness. This appears to be a logical next step for the future work, but no output data was given to assess the need for future optimization. The team also presents various future ideas for new prototypes and products outside the scope of the funding. The team presents a very nice future roadmap for its products that is very impressive and includes a 5,000 psi home fueling system and a nice loop into a DOE trade study on home fuelers. This will fit nicely with a response to the DOE FOA on validation and data collection.

### Project strengths:

- This is a good technical solution. The team is making good progress on getting to a system for testing.
- The summary slide indicates that Proton OnSite has the experience with commercial products to complete the design and safety analysis.
- The strength of the project is its alignment with the FCT Program's interests in validating $H_2$ refueling systems. This product will be a nice addition to the community to advance the use of FCEVs in the home, which is an urgent need for infrastructure. The product will also be well suited to be tested under the active DOE FOA 626 on validation and data collection in real-world environments of $H_2$ fueling stations. This project/product should participate. It is commendable that the team is continuing to leverage other DOE support avenues and recently won a prestigious SBIR Phase I, release 2 project to advance its membrane technology.

### Project weaknesses:

- The team needs to provide cost projections and show how the system will meet the cost targets. The project needs more focus on safety issues.
- Presenters do not provide enough details for reviewers to evaluate the project's strengths and weaknesses. This reviewer cannot believe that all of the technical details are proprietary.
- This is not necessarily a direct weakness, but it would have been nice to see more performance and output data. This reviewer realizes this may be proprietary at this point. It is not clear why ORNL was the selected team member and, without the more detailed explanation, it could appear a bit "thrown in," to have a national laboratory in the presentation.

### Recommendations for additions/deletions to project scope:

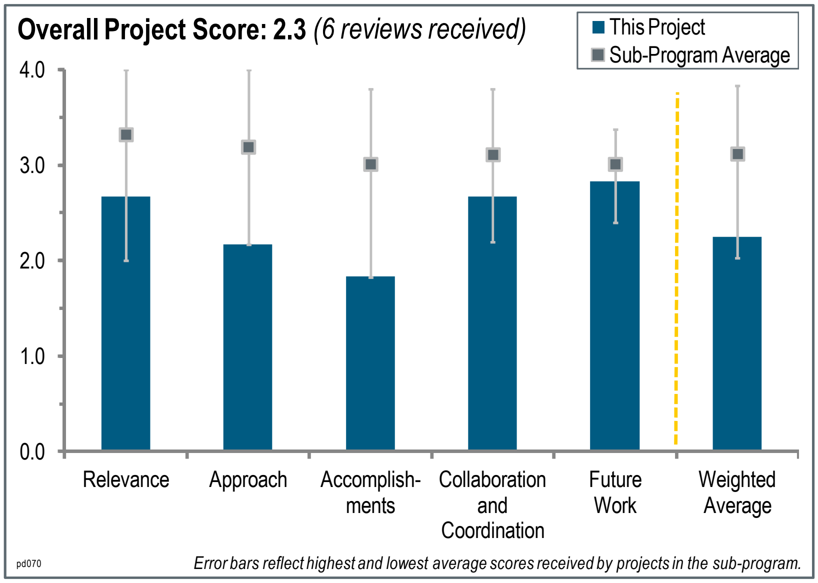
- The scope is fine. The team should work on cost reduction.
- This project should continue to be funded as much as possible. The future product plans are impressive, will contribute to the fuel cell community, and are well aligned with the DOE mission and FCT Program goals.

# Project # PD-070: One Step Biomass Gas Reforming-Shift Separation Membrane Reactor

Mike Roberts; Gas Technology Institute


## Brief Summary of Project:

The long-term objective of this project is to determine the technical and economic feasibility of using the gasification membrane reactor to produce hydrogen ($H_2$) from biomass. The short-term objective is to evaluate synthesized metallic and glass ceramic membranes to fabricate a module for testing with a bench-scale gasifier. The project scope includes membrane material development, gasification membrane reactor process development and economic analysis, bench-scale biomass gasifier modification, integrated testing of the initial membrane with the gasifier, and integrated testing of the best-candidate membrane with the gasifier.


## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.7** for its relevance to DOE objectives.

- The long-term project goal meets the $H_2$ production cost targets, but not the $H_2$ purity targets.
- $H_2$ production from renewable sources is important to the overall sustainability of $H_2$ fuel cells.
- Low-cost membranes for $H_2$ separation are a key component to achieving the $2/kg cost for $H_2$.
- The concept of using a gasifier membrane to produce $H_2$ is novel and potentially interesting. However, there were many questions about the actual operation, or planned operation, that were not addressed in the slides or by the presenter.
- The economic analysis indicates that the one-step biomass gas reforming-shift membrane reactor has no significant advantages in power requirements because the excess power generated is required for gas compression. Producing $H_2$ from biomass with power cogeneration therefore does not increase efficiency. It is not clear whether there is a cost advantage to this process. The cost analysis did not specify that the Hydrogen Analysis (H2A) model was used, as slide 19 implied that the detailed capital cost estimate is from the ASPEN Plus Model. The summary slide showed a comparable cost estimate for the conventional biomass reactor using pressure swing adsorption (PSA), a well-known technology. The H2A version three estimate was $2.00/kg for the PSA process while the H2A version three estimate for the membrane reactor was $1.82/kg. Whether this difference is significant depends on the performance of the membrane in terms of durability, flux, contaminant effects, etc., about which nothing has been reported. Therefore, it is not clear if the project will meet DOE research and development objectives.
- This reviewer wants to know why the long-term $H_2$ flux and purity goals are less than the 2015 DOE targets, and why durability is not a goal of this project. If the goal is to get to 270 SCFH/$ft^2$, it was unclear why the bar has been lowered for this milestone. It also appears lower than the previously achieved 2006 status. Unfortunately, the presenter did not provide satisfactory answers at the DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR).

### Question 2: Approach to performing the work

This project was rated **2.2** for its approach.

- In 2011, the researchers tested the performance of the metal glass ceramic membrane and found disappointing flux and low $H_2$ permeability as compared to metallic membranes and advanced process simulations. The $H_2$ flux membranes module is in fabrication and should be complete by August 2012.
- The approach as provided was generic, and gave no indication of specific difficulties or areas where greater focus should have been applied. Moreover, the approach in terms of milestones went back as far as 2008, suggesting that this slide has been recycled. It was difficult to tell how much the approach might have been modified based on earlier years' work.
- One of the comments from the 2011 review was that greater focus was placed on the modeling effort compared to the experimental effort. The same comment applies in 2012. Work related to factors that will control overall performance such as durability and the effect of contaminants on the membrane surface has not been addressed. The project was started in February 2007, and the final candidate membrane was just selected. Total project funding was $3,396,186. This amount seems high for the economic analyses, membrane development work, design of the membrane reactor, and fabrication of the membrane module without other testing.
- The project is 75% complete to date and the progress is disappointing. Having the initial candidate selected in 2008 become the best candidate in 2012, with only marginal results demonstrated on alternates, does not reflect a reasonable approach based on the funding. The same is true with the design of the module. It was completed in 2010, but it will not be fabricated until August 2012. If there are assembly problems, it may be delayed. This appears to be a poor-performing project with unrealized expectations on how it will perform. Costs are still modeled, and the manufacturing costs are still of a rough order of magnitude and have not been validated to show they can get to the $1.82 $H_2$ cost per kilogram.
- The project title, "One Step Biomass Gas Reforming-Shift Separation Membrane Reactor," is unclear. It was unclear if this means the approach is to have reforming-separation-shift processes all in one step. If so, the approach may need serious revision. This reviewer is not sure how a separate stream to membrane and gas cleaning works and would like to know if the researchers are integrating the shift within the membrane. The presentation suggests that there is removal of the shift reactor with the membrane system (page 8). It was unclear if retentate is sent to gas cleaning, and what the mechanism is for the cyclone to "split," compared to the "conventional" approach, where only a single stream exits the cyclone. Again, the presenter did not provide satisfactory explanations.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **1.8** for its accomplishments and progress.

- It looks like mostly modeling work was performed over the past year, along with some membrane testing.
- The researchers have made good progress in cost analysis, but the fabrication and validation of hardware has been slow.
- It appears that the membrane modules were still in the process of being fabricated, and no apparent testing has been carried out. Actual membrane results, primarily carried out at the National Energy Technology Laboratory (NETL), appear to be the same as last year, or possibly earlier. Most of the accomplishments and progress seemed to be based on HYSYS and ASPEN modeling. The presenter was insufficiently knowledgeable to answer specific questions about the accomplishments.
- There does not appear to be an efficiency advantage in terms of power consumption. The summary slide shows a cost comparison for the conventional biomass reactor and the one-step membrane reactor. Three refinements were presented. The third set of values was $2.00/kg and $1.82/kg, respectively. The PSA technology is well known, while the performance of the membrane reactor is still under development. It is possible the costs of the former will be relatively stable, while the costs for the latter will increase. The only experimental work reported is the selection of the $Pd_{80}Cu_{20}$ as the final membrane, and it appears that the work was done at NETL, in a clean system presumably. No details are given in the NETL slide. The unit for performance criteria is STCH/ft$^2$, not mol/m/s/Pa$^{0.5}$ (for permeability), as in the NETL slide. Data on the flux rate in the presentation are very confusing. For example, the flux rate for 2006 status (slide 4) was >200 STCH/ft$^2$. The milestone reported

completed on 6/15/2011 was for developing the membrane with a flux of 125 SCFH/$ft^2$. Finally, the summary slide reports the module is capable of a flux rate of 80+ SCFH/$ft^2$. Significant improvements will have to be made in the module. It is not clear why performance decreased from 2006 to 2011. Some of the inconsistencies may have been better addressed if the scheduled presenter had been able to attend.
- Laboratory results are very limited with long delays in the fabrication of the module. Based on the funding level, it would have been anticipated that at least 5–6 modules would have been built and tested to date to verify the performance of the assembly. Based on other program gasification projects, this may have significant performance issues that might not be addressed because most of the funds have been expended.
- It was not clear why the presenter showed permeability and not flux or $H_2$ selectivity, as these are measured goals, and what has been accomplished in this cycle. Even the permeability data (apparently, the only experimental data shown) was presented at the last AMR (page 11). Since this reviewer is not sure of the researchers' proposed process (shift or gasification integrated with membrane), this reviewer is also unclear on temperatures (page 12). Gasification temperatures may be as low as 750°C, but 900°C and higher are better. High temperature shift is about 400°C. Membranes at 800°C do not appear to be a good temperature for integration. Unfortunately the presenter was not able to clarify this issue. There is a fairly broad range of temperatures (700°C –980°C) for the gasifier and reformer. The syngas has a small amount of tars (heavy py-oil, essentially). The reformer is there to convert to more syngas. The purpose of the metal-glass ceramic membrane effort with Schott is unclear, because the palladium-copper (Pd-Cu) membrane had already been selected. An inconsistent dollar reference was given—both 2005$ and 2007$.

### Question 4: Collaboration and coordination with other institutions

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

- This project features a nice mix of industry, academia, and national laboratories.
- NETL provided work on the membrane selection.
- It is not clear why the major (metallic) membrane players are not involved.
- The collaboration with NETL, Schott, and ATI Wah Chang seems to be adequate, in principle. The collaboration with NETL does not seem to be recent, however, and ATI Wah Chang was not cited in terms of the membrane module fabrication or assembly. Therefore, the true extent of collaboration on this project is not clear.
- Based on the presentation, the collaboration was average. Plans as to how the team will address the critical parameters and the construction of the module are lacking. The modeling approach seems reasonable, but this should have been a very small component of the project.

### Question 5: Proposed future work

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

- One reviewer is looking forward to the results from the fully integrated unit.
- The summary of the proposed future work is logical, but the list is long. It seems it will be quite challenging to achieve the final milestone by June 2013, given what appears to be somewhat slow progress to date.
- The future work includes completion of fabricated parts, testing, and H2A analysis. All of these are critical to correctly assess the value of this project.
- Integrated testing with the gasifier will be very helpful to validate the performance and cost estimates.
- There is a go/no-go decision in June, but this has not been incorporated into any of the future plans. It was unclear what will happen if the modules cannot be sealed or the membranes get damaged or do not perform as modeled. Also, the difference between $1.82 and $2.00 per kg for the cost of $H_2$ is within the statistical accuracy of the estimates, and is probably not relevant at this point of the project.
- It was unclear where the researchers stand on $H_2$ purity, selectivity, durability (membrane life), and flux (SCFH/$ft^2$), and what their plan is to reach the goals. The future plan is vague. There appears to be no milestones for 2012.

### Project strengths:

- The work is lead by Gas Technology Institute (GTI), a leader in gasification technology.

- The potential novelty of the approach is a strength of this project.
- GTI has experience with gasifiers.

### Project weaknesses:

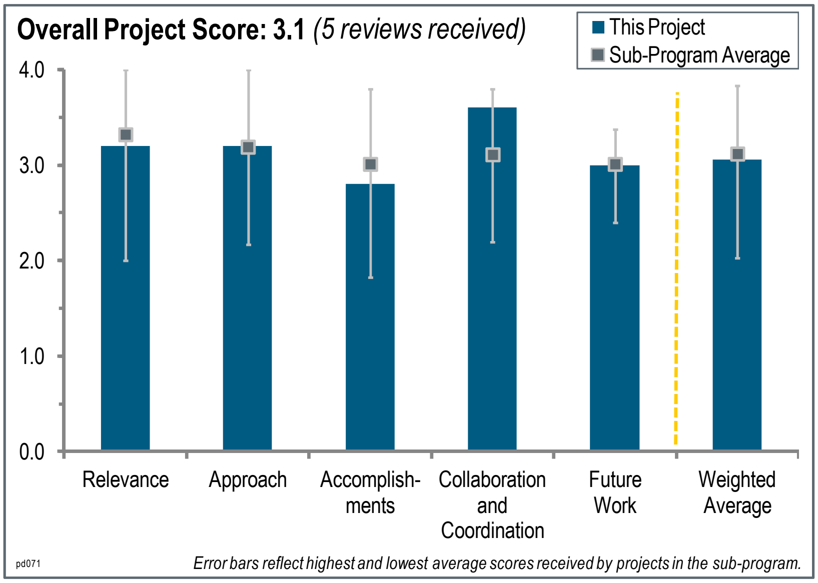
- There were long delays in testing and developing components for modules. There was also a long delay in selecting membranes for continued evaluation. The go/no-go was too far into the project to make any difference.
- The five stages are unclear. This reviewer wants to know how the syngas moves from the membrane (800°C) into the water-gas-shift (WGS) (400°C) over the five-stage process. This reviewer also wants to know if there are heat exchangers in each stage between the membrane and WGS. It is unclear as to which membrane composition was selected.
- One weakness is the lack of a clearly identified path to success. The proposed steps are fairly generic. Methods to be employed, for example to avoid fouling of the membrane with tars, were not clear.
- There is very little preliminary experimental work on the reactor itself and on testing the membrane with a simulated stream of gases from the gasifier. The figures in the slides were not as good as they could have been. For example, slide 28, which shows more details of the membrane reactor, should have been included in the body of the presentation so that the audience could understand the concept.

### Recommendations for additions/deletions to project scope:

- Although the 80 Pd-Cu showed the best results, the accomplishments for 2011 highlight the performance with the Ag/Pd metal-glass ceramic membrane. This is confusing.
- Progress as reported is quite slow for the amount of time and effort expended. If the H2A analyses of the membrane reactor do not show a significant advantage to the membrane and conventional reactor, this reviewer would recommend that the project be terminated unless there are data that have not been reported due to time constraints.
- It is unclear if this project was to select membrane materials, develop a module design, or actually test a module in realistic conditions. The results shown appear to demonstrate that the researchers have spent more effort on developing analytical models and running economic analysis than on completing experiments and incorporating the results in hardware designs.
- One reviewer is not confident the project will achieve its own lower goals, let alone the DOE goals. This reviewer recommends that the project be stopped or have the milestones seriously revised with a meaningful and accountable plan in the upcoming go/no-go meeting. The presentation should have been given by someone familiar with the project.
- Another reviewer cannot identify any recommendations. All of the proposed steps seem to be important, although whether they can be accomplished within the next 12 months is questionable.

## Project # PD-071: High Performance, Low Cost Hydrogen Generation from Renewable Energy

Katherine Ayers; Proton OnSite




### Brief Summary of Project:

The project objectives are to improve electrolyzer cell stack manufacturability through consolidating components and incorporating alternative materials, and to reduce the cost of electrode fabrication by reducing precious metal content and employing alternative catalyst application methods. The project addresses high-impact areas of flow-field cost and labor reduction.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- This project features excellent alignment with DOE's Hydrogen and Fuel Cells Program (the Program) goals and objectives.
- The development of a better electrolyzer system that meets DOE goals is within the objectives.
- Water electrolysis is a near-term technology for the production of hydrogen ($H_2$). The project (polymer electrolyte membrane [PEM] electrolysis) is well aligned with the needs of the Program and fully supports the objectives to increase efficiency and reduce cost. This project is mainly focused on cost reduction; another project is mostly focused on efficiency.
- The project is clearly aligned and relevant to $H_2$ goals. The team clearly showed how features under development contribute to cost, and clearly identified the value proposition of the work. This reviewer did not rate the project as "outstanding" only because this particular project impacts only a part of the key features of the cell.
- Slide 2 indicates that system efficiency is being addressed, but slide 3 states that the project addresses high-impact areas of flow-field cost and labor reduction. The presenter stated that efficiency is not being addressed. Slide 4 states that this project supports Proton OnSite's overall roadmap for cost-effective renewable $H_2$ production. Slide 5 is basically a marketing slide, and this reviewer thinks that it is irrelevant. Slide 6's last bullet once again promotes Proton OnSite's objectives. Overall, Proton OnSite should present how it is supporting DOE objectives and not its own.

### Question 2: Approach to performing the work

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

- The approaches seem very well designed to address the issues for this project.
- This project features a very well-laid-out and structured approach.
- The approach of the project is well designed. The project exploits the results of the past work to make progress on the cost reduction and the scale-up through the consolidation of components, the incorporation of alternative materials, and the use of advanced application techniques.
- Given the limited scope, this approach is satisfactory. Task 1 looked at catalyst loading, which appeared to be outside of the scope described in slide 3. This is sending a mixed message
- The approach seemed to involve lots of combinations of efforts, which may not provide a quantum enhancement in the design.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Very good progress has been made on cost reduction and efficiency improvement.
- The project has made good progress in validating stack cost reduction with a new design, has a prototype on test, and has a clear path for continued progress.
- It is unclear why noble metal loading is being reported if it is not in the scope of the project. The investigators have made good progress in reducing flow-field cost, having demonstrated the feasibility of a 40% reduction.
- This reviewer is not sure about the accomplishments because the presentation was more of a marketing effort and lacked scientific/technological descriptions.
- The team has made great progress on alternative designs, but the rate of progress on the coatings work is a little disappointing (i.e., good but not great). It is not clear how critical the nitride work is to achieving the goals, so it is hard to rate this progress higher.

### Question 4: Collaboration and coordination with other institutions

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

- The collaborations are well coordinated.
- The project features a good mix of industry, academia, and national laboratories.
- There is good collaboration with academia and industry.
- This project takes an aggressive approach to leveraging partner capabilities.
- The project features really appropriate collaboration, taking the form of a partnership with Oak Ridge National Laboratory and perhaps some of the early Entegris work, but perhaps some of the other collaborations were more of vendor-type relationships.

### Question 5: Proposed future work

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

- The project features very well-laid-out future plans.
- The project has a clear plan for the end goal of the Program and is focused on scale-up, manufacturing, and test qualification.
- The emphasis on scale-up to a 50 kg/day stack is a good approach for future work.
- From the presentation, it is not clear what kind of solid plan the company has for this project.
- The future work appears to involve the important efforts of finishing up the current tasks, but little here is aimed at overcoming the next barrier or making the next cost or efficiency breakthrough.

### Project strengths:

- The approach of the project is good, and it has good partners to address the critical issues.
- The team made good progress on reducing plate costs. It is scaling-up to a larger stack.
- The team appears to be strong.
- Strengths of the project include its structured approach, effective execution, and aggressive leveraging of partner capabilities.

### Project weaknesses:

- As the project enters its last year, the testing conditions should be more detailed to evaluate if they will allow a full qualification of the performances of the prototype. This is critical to drawing clear conclusions on the real performances of the prototype at the end of the project.
- For the amount invested by DOE in this project ($3.4 million), it seems that more could have been accomplished. The project is narrowly focused on plate design. The presentation was confusing—efficiency was not part of the

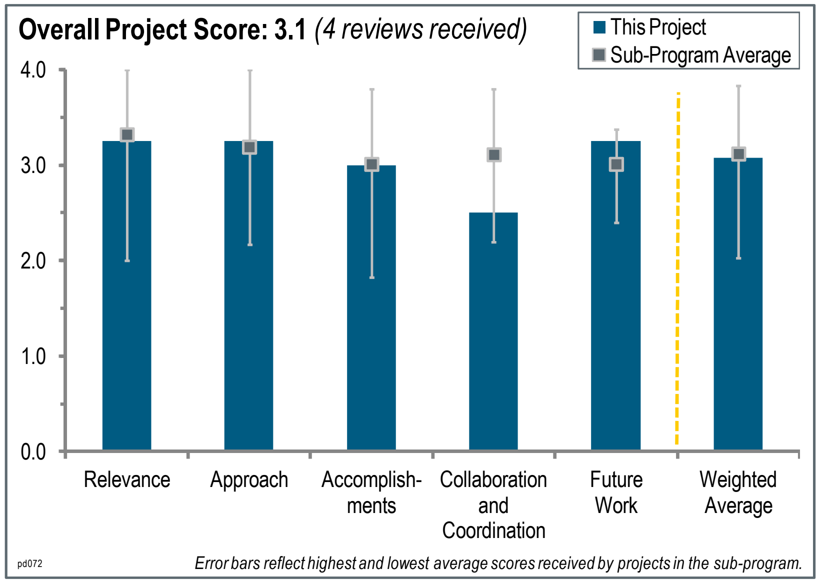
project, but it was reported on. There was too much selling of Proton OnSite and not enough technical discussion. The presenters should leave the sales pitch to the sales people.
- The presentation was highly lacking in the technological innovations. This reviewer was simply asked to believe the cost saving. In the future, the presenter should describe the relevant technological innovation that led to the cost saving.
- The project could probably use some more modeling to predict the performance or stability of the materials.

### Recommendations for additions/deletions to project scope:

- Long-term testing is needed to validate the prototype.
- The team should consider where modeling might be useful.
- The project should have a rationale design for cost savings based on sound science.
- As stack capital drops, the cost of $H_2$ from these systems becomes more and more dominated by electricity cost. Some out-of-the-box thinking is needed regarding how to break out of those constraints with these electrochemical systems.
- This reviewer could not identify any recommendations.

## Project # PD-072: Development of Hydrogen Selective Membranes/Modules as Reactors/Separators for Distributed Hydrogen Production

Paul Liu; Media and Process Technology Inc.




### Brief Summary of Project:

The general objectives of this project are to: (1) develop, fabricate, and demonstrate field implementable hydrogen- ($H_2$) selective membranes and modules; (2) intensify and improve conventional the $H_2$ production process via a membrane reactor; and (3) prepare field-test modules and conduct a field test for $H_2$ production and purification. The specific objectives for the 2012 fiscal year are to: (1) develop improved palladium membranes with cooling stability in the presence of $H_2$, and (2) design and fabricate a catalytic membrane reactor for field testing.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The combination of a low-cost, $H_2$-selective membrane coupled with an integrated water-gas-shift (WGS) catalyst appears to be an attractive alternative to conventional upgrading of reformate for $H_2$ production.
- Improved $H_2$ purification from reformate streams is important to improving overall system efficiency.
- The project supports the cost performance targets for $H_2$ well, and it has the potential to improve the system performance of $H_2$ reforming.
- Process intensification is often employed to reduce overall production costs. This reactor/separator technology targets process intensification for syngas-based approaches (e.g., steam reforming, authothermal reforming, and partial oxidation) for producing $H_2$. To date, only methane-based technologies have been shown to be capable of meeting DOE targets related to the cost of $H_2$. Renewable bio-based technologies being pursued under the Fuel Cell Technologies Program (FCT Program) are having a difficult time meeting the DOE cost targets, and this technology could potentially benefit these projects.

### Question 2: Approach to performing the work

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

- The presenter described a logical progression to the development of a membrane on a ceramic support that is thin, relatively low in cost, and able to withstand the heating and cooling cycles. The progression from identifying problems to identifying solutions seemed to be very good.
- The overall approach is very strong and is well designed to keep the project on track toward meeting its targets. This reviewer’s only concern is how the cooling stability studies are being conducted. It is not clear that cooling in $H_2$ alone is sufficient to determine membrane stability, given that water will be present and may condense out depending on whether the lowest temperature experienced is below the dew point.
- The use of bundles and cooling subsystems is a rational approach to this performance problem. Improving thermal performance coupled with lower manufacturing costs is a good approach. The 600 hours of stable thermal performance was a significant accomplishment.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The team built and tested a prototype membrane bundle reactor.
- It seems that there is still significant work to be done in integrating the membrane with a WGS catalyst; moreover, dealing with the possible deactivation of the WGS catalyst in heating/cooling cycles does not seem to have been considered. However, progress appears to be quite good during this recent one-year period.
- Accomplishments to date are impressive in terms of improved designs, performance, and durability, although there still seem to be unresolved issues with meeting durability targets during cooling cycles. It is not clear if there is a path forward for solving the stability issues during cooling.
- The technical progress is good. The researchers have tried a number of membranes and approaches to use a ceramic substrate to control cost and porosity, and the laboratory performance demonstrated by their membranes showed improvement over published data. They have developed a sealing mechanism that should work in their bundled configuration. This reviewer still believes the cost data has not been validated and is still an estimate that has a statistical variance of 25%.

### Question 4: Collaboration and coordination with other institutions

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

- Although the list of collaborators is good, it was not obvious how much any of these collaborators contributed to the recent work on this project. There was no specific discussion regarding their contributions.
- Collaboration was discussed, but it was not clear how the partners interface to discuss and resolve critical issues and design concerns. It appears that most of the organizations operate independently.
- The project features strong industrial partnerships such as Chevron, which is identified as a potential end user of the technology, Ballard, which is identified as the partner that will integrate this technology into its fuel processing technology, and Johnson-Matthey Fuel Cells Inc., which will provide the shift catalysts. It would be beneficial to better understand the nature of the collaboration with Ballard and the extent of interaction, because this is the key interaction. In particular, this reviewer would like to know if Ballard's implementation of this technology has had an impact on the cost of their $H_2$ fuel processing technology.

### Question 5: Proposed future work

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

- Validating the laboratory results in a field test is very important.
- The project appears to be approaching an end. The proposed future work plan appears to be good. It is not clear whether the forward work plan also includes the WGS catalyst integration with the membrane.
- While the overall work plan is sound, the effort toward integrating the WGS reaction into this membrane system seems to be lagging behind. It is not clear whether an optimal system will be ready for the field test, given the project is in its final months.
- The future work is a reasonable approach based on previous accomplishments. The fact that the investigators are using a third-generation membrane presupposes it has a much higher probability of success. The field test with laboratory manufactured synthesis gas is not as important as actual synthesis gas produced from a working gasifier. However, this information can validate the thermal design.

### Project strengths:

- The team has a good understanding of membrane development and implementation on ceramic supports, and is building on previously developed capabilities. The team has made good progress and the work plan is good.
- The project has made tremendous progress toward developing the membrane system to meet its performance and durability targets.
- The project features a good engineering approach to achieving the desired flux, a good engineering approach to sealing, and a good experimental database.

### Project weaknesses:

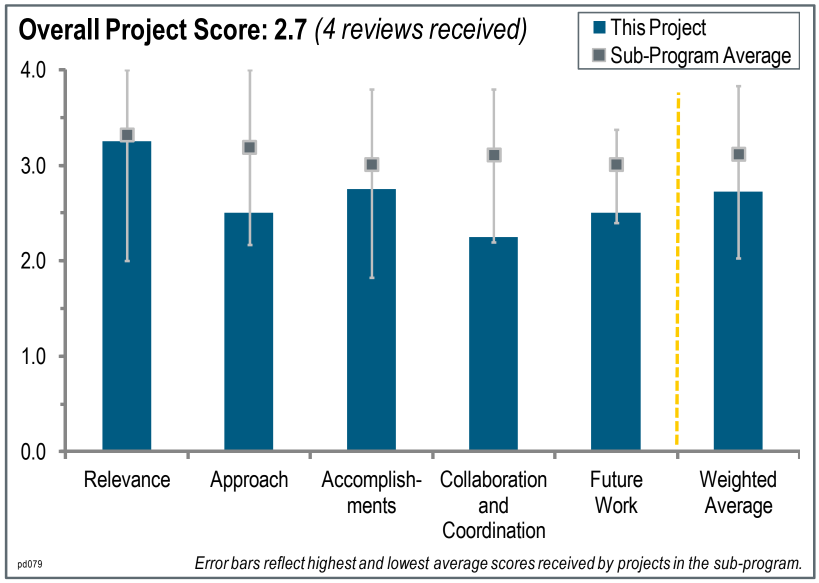
- No outstanding weaknesses were identified. The work might benefit from stronger support on the WGS catalysis side; it is not clear if anything other than catalysts will be provided by Johnson-Matthey Fuel Cells Inc..
- It is difficult to quantitatively evaluate the impact of this technology on the cost of producing $H_2$ using fuel processing technology, given that this is not a stand-alone technology. In particular, it is difficult to determine if this technology will have a significant bearing on improving the cost performance of biomass-based reforming processes, which to date have been unable to meet the DOE cost targets. Natural-gas-based technologies have already been demonstrated to meet the DOE cost target, so the impact on this technology may be minimal. The integration of the WGS reaction into the membrane unit has progressed extremely slowly. There is no clear path forward for addressing the stability issue during cooling under $H_2$. It is not clear if these tests were done under dry $H_2$ or in the presence of moist $H_2$, which will be the case for the operating unit. It is not clear what impact condensation would have on the stability of these membranes, particularly during turn-up if the turn-up rate is high. This reviewer would recommend working with Ballard to understand how its system will operate during periods of stand-down. This reviewer's impression is that it will continue to operate, but at a significantly lower capacity to maintain the catalysts near their optimal operating temperature. There are significant benefits for doing this in terms of maintaining catalyst performance and inhibiting catalyst deactivation due to physical processes associated with rapid turn-ups.
- There are insufficient publications and peer-reviewed articles on the work.

### Recommendations for additions/deletions to project scope:

- One reviewer suggests that the WGS reactor is also tested with cycling.
- The team should test the final configuration in an actual synthesis gas stream from a pilot- or semi-works-scale gasifier to show that the laboratory and modeled performance will be validated.
- Another reviewer had no recommendations.

## Project # PD-079: Novel Photocatalytic Metal Oxides

Robert Smith; University of Nebraska at Omaha




### Brief Summary of Project:

The principal objective of this project is to develop improved solid-state photocatalysts for the decomposition of water into hydrogen ($H_2$) gas using solar radiation. The near-term objectives are to: (1) engineer the bandgap of cesium niobate ($Cs_2Nb_4O_{11}$) through computer modeling for optimum photocatalytic activity in the visible portion of the solar spectrum, and (2) fabricate and experimentally examine the materials identified.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The project seeks to reduce the cost of $H_2$ via solar photoelectrochemistry by taking a stable oxide material and lowering its bandgap energy to make its optical absorption more responsive to solar illumination.
- The project objective addresses only one of the four outstanding issues in photoelectrochemical (PEC) $H_2$ production. Bandgap modification in the absence of band edge alignment is unlikely to provide PEC performance that meets the ultimate goals. This work should be represented within the PEC Working Group to permit its participation in the full scope of PEC technical targets.
- This project certainly supports the objectives of the DOE Hydrogen and Fuel Cells Program (the Program). However, the research is still at an early stage of the technology.
- This project to designed to engineer advanced bandgap of $Cs_2Nb_4O_{11}$ catalysis through both computer modeling and experimental fabrication processes to improved solid-state photocatalysts for the decomposition of water into $H_2$ gas using solar radiation. The project is directly aligned with the Program's goals, including both delivery and production goals for 2020. The Fuel Cell Technologies Program (FCT Program) proposes to improve fuel cell electric vehicle competitiveness by reducing the cost of producing, delivering, and dispensing $H_2$ to below a threshold of \$2–\$4/kg of $H_2$ by 2020. FCT Program production goals include the development of distributed and central technologies to produce $H_2$ from clean, domestic resources at a dispensed cost threshold of \$2–\$4/kg of $H_2$. This project drills down on an advanced catalysis system aimed at increasing $H_2$ production. The novel approach to advancing PEC catalysis of water is named as a critical target area for the Program. While the presentation does not include projected cost, the presentation targets improved production and the cost of materials, such as niobium, should be minimal.

### Question 2: Approach to performing the work

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

- The approach is generally effective, but it could be improved.
- The approach to the work is comprehensive and includes a detailed computational analysis complemented by an experimental approach. The work uses a physical approach to examine and tailor the bandgaps and band edges of the proposed materials to optimize the energy absorption efficiencies. Computer simulations are used to design and modify the bandgap structures. The work includes an experiment aspect that measures absorption spectra for comparison with the model outputs.

- Lowering bandgap energy is only a small part of the PEC problem. This reviewer wants to know what the extinction coefficient is for charge carrier generation. Reasonable conductivity within the newly generated or altered conduction bands must be demonstrated. Long-term stability of the doped materials must be readdressed.
- There are significant deficiencies in the technical approach to calculating and engineering bandgaps for a complex material, $Cs_2Nb_4O_{11}$. Bandgap relevance to photoactive materials is primarily confined to the interface, and the density of states (DOS) determined through density functional theory (DFT) calculations at interfaces are untrustworthy because of the cyclical boundary conditions required for the DFT numerical framework. Furthermore, the extraction of in-bulk bandgap information from DFT-DOS calculations is uncertain, especially in regions of sparsely occupied states. The uncertainties described above are compounded by proposed "doping" of the $Cs_2Nb_4O_1$ with various ancillary materials in the absence of molecular dynamics calculations that ensure stable doped configurations. The evidence of widely distributed and sparsely occupied states is suggestive of a configuration that is unlikely to be maintained under any significant local disturbance, as would likely accompany charge displacement through photon absorption or other energetic phenomena.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- A significant level of effort is demonstrated in performing DFT-DOS calculations, but the outcomes are unlikely to prove helpful in identifying new photoactive materials with improved PEC performance.
- The progress could be better if there is collaboration with an outside institution, such as the National Institute of Standards and Technology (NIST). Taking advantages of resources from NIST is always beneficial. Collaboration with national laboratories is also a good way to get confirmation on new materials development efforts.
- Based on the budget, which is quite modest, a significant amount of work has been accomplished as proposed, and the presenters demonstrated progress and results. The bandgap calculations via computer simulation have been completed, and the calculations agree with photoluminescence. The sol-gel synthetic route for $Cs_2Nb_4O_1$ production has been perfected, and the bandgap structure has been determined to be 3.5 eV by luminescence spectroscopy, as proposed. An extended computer doping study was set up; initial calculations and tests expect to be completed by May 2012 and are 90% complete. Synthesis of the new doped materials was started in December 2011, and dopants and levels will be determined from the computer study. This work is 50% complete, as appropriate. Overall, the results demonstrate the impact of various dopant amounts of sulfur-doped $Cs_2Nb_4O_1$ that has been synthesized by passing gaseous carbon disulfide over $Cs_2Nb_4O_1$ at elevated temperatures for a set amount of time, and their relative bandgap structures. Chemical analysis is being performed to determine dopant amounts as a function of experimental conditions.
- A lot of calculations have been performed on the effect of potential dopants, but actual synthesis of these materials seems to be just starting. The calculations examined the effect of substituting tantalum (Ta), vanadium, and sulfur on bandgap energy of cesium niobate (CNO). It appears that some work has been done with sulfur and doping with vanadium has yet to begin, and it is not clear whether the researchers will even try Ta, as the calculations did not depict a bandgap lowering that was as effective as they had anticipated. This reviewer wonders if the doping affects the color of the material. If CNO is white to start with and still white after doping, then little was accomplished with respect to solar absorption.

### Question 4: Collaboration and coordination with other institutions

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

- It would benefit the researchers greatly to collaborate with some other institutions that would help them characterize or show them how to characterize a photovoltaic/PEC material.
- This effort should be incorporated in the PEC Working Group to ensure timely peer review of the proposed work and the provision of advice and expert assistance in pursuit of the stated objective.
- There is a lack of collaboration with other institutions outside of the University of Nebraska.
- The project includes collaborations with the University of Nebraska at Omaha, the University of Nebraska-Lincoln, and National Taiwan Normal University. It is not clear why National Taiwan Normal University was chosen for the collaboration. It would be nice to see the project team collaborate with a DOE national laboratory,

such as Oak Ridge National Laboratory (ORNL), on the computational side. This might be a nice fit to use ORNL's extreme computational resources to extend the research; for example, using Jaguar.

### Question 5: Proposed future work

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

- It is hard to think of future work when the researchers have so much of their current work ahead of them.
- Future work on this project should be delayed until subject matter experts have conferred with the principal investigator regarding some concerns.
- The proposed future work should include methods on how to scale-up the technology for $H_2$ production as well as information regarding what the barriers could be.
- The future work will include calculation of the effects of doping $Cs_2Nb_4O_1$ with metals by evaluating the bandgaps and band edges of the various composites. The team will perfect the synthesis conditions for the sulfur-doped $Cs_2Nb_4O_1$ and characterize the properties of composites by experimental analysis. This is a very logical next step to complete the findings.

### Project strengths:

- This is an innovative idea to produce $H_2$.
- One strength is how the team is using the computer-aided bandgap analysis to drive the design of novel PEC catalysis materials for $H_2$ production. The computer assistance approach is outstanding.
- The project features a strong theoretical effort predicting the effect of the dopant on bandgap energy.
- Strengths of this project include the team's proficiency in running DFT-DOS and its wet chemistry dopant incorporation processes.

### Project weaknesses:

- It is not clear why National Taiwan Normal University is needed for the optical measurement support or why this may not be performed in the United States.
- Much of the experimental work is still ahead of the investigators. They may hit a home run, but so far it appears that they have not made a dent in the optical absorption of NDO.
- An area of weakness is the team's understanding of the limitations and uncertainties of applying DFT-DOS calculations to bandgap prediction at an interface.
- The project lacks strong outside collaboration. It would be helpful if the innovative process is verified or confirmed by other institutions outside of the University of Nebraska at Omaha.

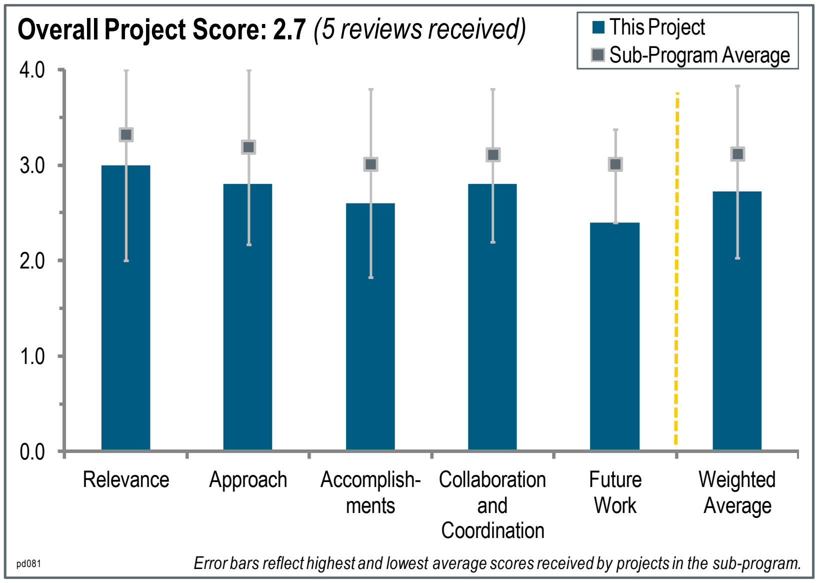
### Recommendations for additions/deletions to project scope:

- The researchers should perform a thorough characterization of the new materials they produce, or group them with partners to get it done.
- The project scope should include a scale-up process for the mass production of $H_2$. Collaboration with outside institutions, such as NIST or national laboratories, is recommended.
- The proposal may be strengthened by including a national laboratory in the work, for example ORNL and its advanced computational resources. Also, it is recommended that the team consider the cost impact of the technology in future discussions of the work. It is unclear whether the work is economically feasible. This reviewer believes it is, but it would be nice to have this market analysis discussed and presented by the proposing team.

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

Ivan Ermanoski; Sandia National Laboratories

**Brief Summary of Project:**
The overall project objective is to develop a high-temperature (HT) solar thermochemical (STCH) reactor for efficient hydrogen ($H_2$) production. The successful development of this reactor will provide a solar interface for most two-step, non-volatile metal oxide cycles. The objectives specific to 2011 and 2012 are to: (1) discover and characterize suitable materials for two-step, non-volatile metal oxide thermochemical cycles; (2) establish a screening protocol for candidate reactive materials and structures; (3) design particle receiver-reactor concepts and assess feasibility; and (4) construct and test reactor prototypes.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The project is relevant to DOE's goal of STCH $H_2$ production, and DOE's specific efforts to find the optimum HT material and solar reactor design.
- This is a simple, two-step thermochemical cycle for $H_2$ production that is applicable to the DOE Fuel Cell Technologies Program (FCT Program). This is such a long-term process that the near-term goals of the FCT Program will not be met. As with all solar-driven processes, there is the concern that where solar power is available, there is a limited supply of water. This is a challenge for all of the STCH projects.
- The project addresses the development of an HT solar thermochemical reactor for efficient $H_2$ production. The successful development of this reactor will provide a solar interface for most two-step, non-volatile metal oxide cycles. Several barriers are being addressed, including HT thermochemical technology, HT robust materials, and coupling solar and thermochemical cycles.
- Solar thermochemical cycles have the promise of being cost-effective, near-zero-carbon-emitting $H_2$ production options. The simple, two-step redox cycles included in this and other solar thermochemical cycle projects funded by the FCT Program are particularly attractive for this $H_2$ production approach. Much of this project is focused on a novel reactor design that could be utilized on the two-step redox cycles under examination in this project, or for other two-step redox cycles, such as the very promising hercynite cycle being studied in another DOE-funded project. The solar-thermochemical cycle focus of this project has been the ceria-based cycle, which has a relatively low redox capacity within each cycle and a relatively high reduction temperature, which make it less attractive than other potential solar-thermochemical redox cycles, such as the hercynite cycle. This project does intend to look at perovskites in the future plan.

## Question 2: Approach to performing the work

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

- The project appears to feature a well-balanced approach to the development of both difficult materials challenges and a mechanically sophisticated process system.

- The researchers are studying the mechanisms of the reactions that are necessary to understand in order to make a successful process. There are large concerns over $H_2$ and oxygen ($O_2$) mixing. Even with long tubes and slow permeabilities, these systems will be operating continuously for years. While the researchers have identified the important activities, it is not clear if the order of work is going in a reasonable manner.
- The following approaches were outlined by the presenter: (1) discover and characterize suitable materials for two-step, non-volatile metal oxide thermochemical cycles, (2) establish a screening protocol for candidate reactive materials and structures, (3) design particle receiver-reactor concepts and assess feasibility, (4) construct and test reactor prototypes.
- The approach for screening the high temperature (HT) metal oxide materials and the reactor design is sound. However, the current solar reactor design is unlikely to be practical for a number of reasons: (1) maintenance of a reactor with mechanical moving parts and daily thermal swings on top of a tall tower structure should be of particular concern; (2) the heat recuperation assumption is too optimistic with the reactor likely located 30–60 m above ground and in long pipes; and (3) there is no material that can withstand 1,500°C operating conditions for too long, especially with the likely daily temperature swing, $MO_x$ sintering issues will come up.
- This project is taking a good approach to meeting its overall objective of developing a cost-effective solar-thermochemical cycle method for $H_2$ production. It has invented a very novel screw conveyer reactor design that has a high potential to meet the needs of two-step, metal-particle-based redox thermochemical cycles, including efficient heat recuperation. The project is strongly focused on the reactor design, theoretical modeling of performance, and demonstration. This includes appropriate efforts on materials of construction that can withstand the HTs and reactivity of the metal oxides used in the thermochemical redox cycles. It is also using a novel laser-assisted stagnant flow reactor and sound kinetic analysis to determine the kinetics of ceria and a few other metal-based redox systems. The effort has included the evaluation of iron-based redox systems, which are generally known not to be good candidates for this approach to $H_2$ production. Part of the effort has focused on doping the ceria oxide cycle with other metals to improve its performance. This has not proven to be a useful approach. The ceria cycle has a relatively low redox capacity within each cycle and a relatively high reduction temperature, which make it less attractive than other potential solar-thermochemical redox cycles such as the hercynite cycle work in another DOE-funded project. This project does intend to look at perovskites in the future plan. The work done so far to demonstrate the feasibility of the novel reactor design is not nearly aggressive enough. So far, only ambient temperature testing for short durations (one hour) has been done. Much more work is planned though. Although the $H_2$- and $O_2$-generation steps occur at different places in this reactor design, there is still the possibility of them diffusing through the packed powder and mixing. This issue should be evaluated experimentally as soon as possible within the project. There does not appear to be cost-estimate work being done to ensure the cost effectiveness of the reactor and the overall approach of the project.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- A system concept improving the design to one moving part is a major design improvement.
- Progress for the past fiscal year was reported.
- There has been significant progress made on this project. The novel reactor design has been completed, built, and tested under ambient conditions for short durations (one hour), and it has successfully conveyed powder particles as designed. It has been theoretically modeled and shows promise of achieving its objectives, including sufficient heat recuperation. Alumina has been tested and appears adequate as a material for construction of the reactor. Ferrite and ceria redox systems have been characterized for their kinetics in a novel laser-assisted stagnant flow reactor. Ceria doping has been tried to improve the ceria cycle kinetics, but this proved unsuccessful.
- The project has achieved reasonable results despite the enormous challenges associated with all STCH processes. The researchers tried to address the HT materials and thermochemical technology aspects of the project barriers; however, little was presented on the stated solar and thermochemical coupling challenge. Although it was mentioned that the material was stable for up to 30 cycles, no data was provided. The particle size did not affect the reduction reaction. It would be helpful to know if similar studies were carried out for the oxidation reaction.
- The finding that the materials are not kinetically limited is not surprising at 1,500°C operation. There has been quite a bit of mechanistic studies on the different materials. The analysis work seems to be well done. Moving away from ceria to non-rare-earth materials is a good step, given the recent concern over rare-earth-material

availability. Moving to doped materials may result in decreased durability compared to the pure $CeO_2$. It seems the heat recuperation will be limited due to the fact that it is a solid-solid particle heat exchange. The tests on material compatibility for $CeO_2$ are a good start. The researchers are 75% done with the project and have not yet developed the protocol for material characterization. One would think that this was the first thing that would have been done. With 75% of the project complete, it seems that they have focused on reactor and particle conveyance development, but they have not even finished the most basic milestone of developing materials characterization protocols, nor have they identified materials. The criteria for success are not well identified. The focus of the work does not seem appropriate. They need to demonstrate that the chemistry can work, develop testing/characterization protocols with metrics for success, and develop the materials. These critical areas seem to have a lower priority than the hardware, but without the material, the hardware is not needed.

### Question 4: Collaboration and coordination with other institutions

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

- There seems to be good collaboration for this project.
- The coordination between the University of Colorado and Sandia National Laboratories appears to be very good.
- The collaboration is sufficient, although the emphasis of the project should be on creativity and productivity; not on collaborations. (The value of this entry as a means of assessing success is questionable. The Program may wish to rethink or rephrase this evaluation question.)
- There is good collaboration with the University of Colorado on kinetic studies, and with Bucknell University and Jenike and Johanson, Inc. on the reactor and reactor screw design.
- STCH projects require a higher degree of collaboration than other projects in the Program, such as materials, redox chemistry, mechanical and reactor engineering, process engineering, and heliostat design.

### Question 5: Proposed future work

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

- Demonstration of the reactor prototype platform will be a good addition.
- Their future work seems very ambitious. Because material development is still being done, it is not clear why the process is being scaled-up. It seems that it would be better to develop the material prior to scaling the process. The investigators need to develop their protocol for material characterization, and it should include clear metrics for success.
- There is a good plan for future work. Perovskites will be studied because they can potentially yield a redox cycle that is more suitable than ceria. A reactor prototype will be built that can be tested at relevant temperatures. Design and analysis of overall system performance will be done.
- On one hand, the researchers propose to go back to the basics, as in new perovskite-related material screening, acknowledging the ultimate challenges and unrealistic expectations of $CeO_2$-based materials. On the other hand, they are chugging along with developing a $CeO_2$ manufacturing process. This is a disconnect.

### Project strengths:

- This is a large, well-funded project with a strong team. The thermal chemical cycle is very simple, which improves the chances for success. The researchers have developed a way to operate 24/7, which overcomes the diurnal limitation.
- The reactor design work is an area of strength.
- The particle elevator is an improvement over the previous design.
- Solar-thermochemical cycles have the promise of being cost effective, near-zero-carbon-emitting $H_2$ production options. The simple, two-step redox cycles included in this and other Program-funded solar-thermochemical cycle projects are particularly attractive for this $H_2$ production approach. Much of this project is focused on a novel reactor design that could be utilized on the two-step redox cycles under evaluation in this project, or for other two-step redox cycles. This project is taking a good approach to meeting its overall objective of developing a cost-effective solar-thermochemical cycle method for $H_2$ production. It has invented a very novel screw conveyer reactor design that has a high potential to meet the needs of two-step metal-particle-based redox thermochemical cycles,

including efficient heat recuperation. The project is strongly focused on the reactor design, theoretical modeling of performance, and demonstration. This includes appropriate efforts on materials of construction that can withstand the HTs and reactivity of the metal oxides used in the thermochemical redox cycles. There has been significant progress made on this project. The novel reactor design has been completed, built, and tested under ambient conditions for short durations (one hour), and it has successfully conveyed powder particles as designed. It has been theoretically modeled and shows promise of achieving its objectives, including sufficient heat recuperation. Alumina has been tested and appears adequate as a material for construction of the reactor. Ferrite and ceria redox systems have been characterized for their kinetics in a novel laser-assisted stagnant flow reactor. Perovskites will be studied because they can potentially yield a redox cycle that is more suitable than ceria.

### Project weaknesses:

- There is a lack of sufficient collaboration.
- The project is 75% complete, and the researchers still have concerns about the materials. The progress seems to be slow. The project is 75% complete, but the protocols for material characterization are only 50% finished, while the particle conveyor concept is 100% complete. It does not seem that the work has progressed in a logical order. It seems, at a minimum, the protocol for material characterization should have been completed first. It is unclear why the researchers are spending effort on scaling the reactor up while they are still looking for the right material. They need to look at how this project is being managed.
- Any system with 1,100°-1,500°C temperatures and moving mechanical components is asking for trouble. No real support is given for a solar-to-$H_2$ efficiency of 25% (yearly average).
- The solar-thermochemical cycle focus of this project has been the ceria-based cycle, which has a relatively low redox capacity within each cycle and a relatively high reduction temperature, which make it less attractive than other potential solar-thermochemical redox cycles such as the hercynite cycle work in another DOE-funded project. The effort has included an evaluation of iron-based redox systems, which are generally known not to be good candidates for this approach to $H_2$ production. Part of the effort has focused on doping the ceria oxide cycle with other metals to improve its performance. This has not proven to be a useful approach. The work done so far to demonstrate the feasibility of the novel reactor design is not nearly aggressive enough. So far only ambient temperature testing for short durations (one hour) has been done. Much more work is planned though. Although the $H_2$- and $O_2$-generation steps occur at different places in this reactor design, there is still the possibility of them diffusing through the packed powder and mixing. This issue should be evaluated experimentally as soon as possible within the project. There does not appear to be cost-estimate work being done to ensure the cost effectiveness of the reactor and the overall approach of the project.

### Recommendations for additions/deletions to project scope:

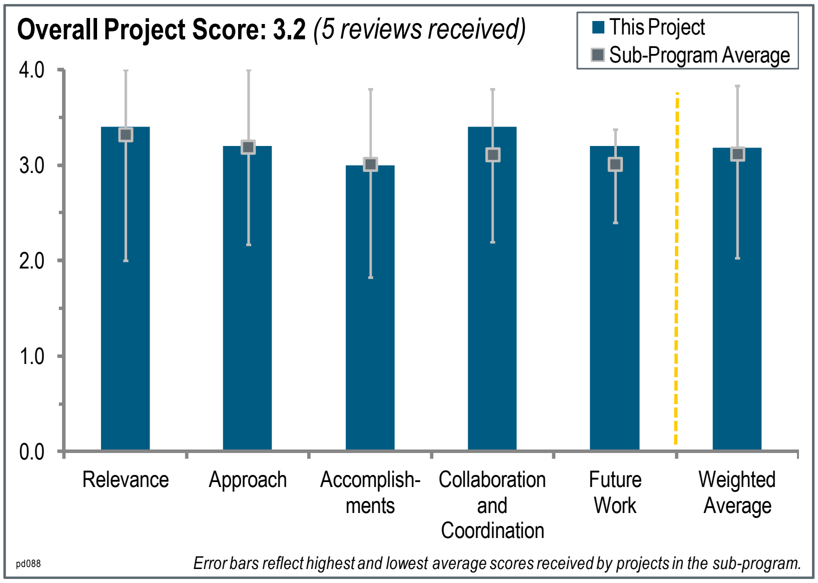
- It would be nice to see a breakdown of the efficiencies by the process. This way the reviewers can see where the heat loss is and clearly identify the motivation for where the researchers are working. The project does not seem to be focusing on the critical elements for it to be a success. The researchers are working on scaling-up the reactor when they do not even have a protocol for material characterization, and, apparently, the criteria for a successful material has not yet been shown.
- Solar-to-$H_2$ efficiency is reported to have a high heating value (HHV) ratio, but use of low heating value (LHV) is standard. The cycle life of the materials should be stated and tested. Cycle times have a significant impact on $H_2$ cost and should be overtly addressed in the project.
- The team should include evaluation and testing of the hercynite redox cycle in the novel screw reactor system. It should also do a thorough H2A cost analysis of a complete commercial-sized facility based on this novel reactor design.
- It may be beneficial for the Hydrogen Production and Delivery sub-program to revise the work scope and performance targets for all STCH projects, not just this particular one. STCH projects have been going on for years, and yet there is almost no convergence to a clear pathway to success. In practice, thermodynamically favorable, HT water splitting materials are still being screened. At this point, it does not make much sense to pilot a closed-loop solar reactor in an actual solar field with non-ideal materials. Researchers could be burdened and discouraged if they are asked to reach an almost impossible target.

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

Wei Zhang; Oak Ridge National Laboratory

### Brief Summary of Project:

The objectives of this project are to: (1) address the significant safety and cost challenges of the current industry-standard steel pressure vessel technology, and (2) develop and demonstrate the composite vessel design and fabrication technology for stationary storage systems of high-pressure hydrogen ($H_2$). The approach includes the use of commodity materials (e.g., structural steels and concretes), the mitigation of $H_2$ embrittlement to steels, an automated manufacturing process for a layered steel tank, and embedded sensors to ensure safe and reliable operation.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- Safe and affordable high-pressure storage at refueling stations is critical to achieving market penetration.
- The project team identified that storage is a barrier to $H_2$ infrastructure and vehicle development.
- From previous presentations, it is clear that compressors are the largest cost in a station. In order for this reviewer to properly evaluate the relevance of the project, the project team should better indicate the effect of vessel cost on $H_2$ delivery cost.
- This project is very relevant to the objectives of creating a low-cost, novel composite vessel technology to contain high-pressure $H_2$ at fueling stations.
- Pressurized $H_2$ bulk storage is a significant cost in gaseous $H_2$ delivery infrastructure. In order to meet the DOE Hydrogen and Fuel Cells Program's (the Program's) targets for the cost of $H_2$ delivered to a refueling station, it is necessary to reduce the cost of this storage. The approach taken of utilizing low-cost concrete, which enables a reduction in the use of more-costly steel, holds the promise to meaningfully reduce the cost of $H_2$ bulk storage.

### Question 2: Approach to performing the work

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

- The project features a very novel approach to achieving lower-cost storage by using layered stainless and alloy steel enveloped by concrete.
- The cost per unit of pound weight does not tell the whole story. It should list the cost per unit of strength in whatever form is relevant.
- The investigators propose an innovative method of using some low-cost materials, including pre-stressed concrete with a composite metallic pressure vessel construction fabrication method that has been used for industrial pressure vessels in a few select applications.
- The basic approach and analysis seem very good, and in some aspects it is outstanding; however, in other aspects it appears that the investigators made a decision early in the design process that they now accept without question, when perhaps they should revisit the earlier decision. Anytime an item is changed in a system design such as this, one needs to evaluate the impact of that change on every other part and design decision. The range

of liner materials under consideration and fabrication techniques should be expanded. Rather inexpensive oxide coatings have been found to be effective barriers to $H_2$ permeation for ultra-high vacuum systems, and this reviewer wonders if they could also be used for $H_2$ storage tank walls. If the concrete is in compression, this reviewer wants to know if the steel walls should also be in compression. It was unclear if the elimination of tensile stresses in the steel would eliminate the potential for $H_2$-assisted fracture, regardless of the material or alloy. It would also be helpful to know if a stress analysis of the system would help answer these questions.

- This project is investigating utilizing a concrete steel composite structure to reduce the cost of pressurized $H_2$ storage. The low-cost concrete reinforcement permits a reduction in the use of more-costly steel. This approach holds considerable promise. The current design utilizes an inner barrier layer of expensive stainless steel, three layers of carbon steel, and then reinforcement with pre-stressed concrete. Recent available data shows that carbon steel can be sufficient as an $H_2$ barrier, and its $H_2$ embrittlement can be sufficiently mediated with proper design and thickness. The project should consider eliminating the costly stainless steel barrier layer to further reduce the cost. It would also be interesting to look at the cost of utilizing a carbon fiber composite-based inner vessel lined with high-density polyethylene as a barrier layer with reinforcing concrete to reduce the amount of costly carbon fiber needed. The approach being taken is to design the reinforced concrete vessel using engineering calculations and existing pressure vessel codes, layout the manufacturing process in detail, estimate the cost of the vessel, and then optimize the design to lower the cost. All of this will be done before construction and testing. This is a very good and cost-effective approach. The vessel design includes the use of friction stir welding. This is a concern because this welding technique has not yet been commercialized, but it would be a very cost-effective welding approach, especially if it could be automated as planned. The project has excellent collaborators, including commercial engineering companies; experts on specialty concretes, high-strength steels, and friction stir welding; and the U.S. Department of Transportation (DOT).

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project team is making good progress and has produced a lot of new results.
- The project has produced a lot of calculations and estimations, but little demonstration of the concept.
- Good progress was made on the cost estimates for the pre-stressed concrete sleeve and the composite steel tank. It was not encouraging to see the projected cost go up with this more detailed analysis.
- There has been considerable progress made on this project. Two concrete-reinforced vessels and a standard all-steel vessel have been designed to meet the applicable pressure vessel codes. The manufacturing process has been laid out in detail and cost estimates have been completed that show promise for this approach to reduce the cost of $H_2$ storage.
- The team is making steady and significant progress toward the goals and objectives of the project. Researchers have identified and overcome significant barriers. A summary table of issues addressed, as well as materials and tests used, would help present this progress.

### Question 4: Collaboration and coordination with other institutions

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

- This project features good collaborations.
- The project team includes a wide range of industry and research representatives.
- The project has excellent collaborators, including commercial engineering companies; experts on specialty concretes, high-strength steels, and friction stir welding; and DOT.
- There is significant collaboration in the design of the concrete composite, cost estimating, welding, and regulatory testing.
- There was a fair amount of collaboration with the project partners, but this reviewer did not hear about their contributions.

### Question 5: Proposed future work

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

- The team needs to think about how to fabricate the final systems on-site because shipping large vessels seems problematic.
- The team needs to demonstrate the concept with real $H_2$ storage.
- The future plan is well thought through and complete. It includes optimizing the vessel design to minimize cost through engineering calculations and manufacturing cost estimates. Both materials and strain sensors that could be incorporated within the structure to detect any problems during use will be tested for durability in a pressurized $H_2$ environment. A vessel will then be constructed and fully tested for use against the appropriate codes and standards.

### Project strengths:

- This project represents a novel concept for low-cost storage.
- The strength of this project is the potential for cost reduction in $H_2$ pressure vessel production.
- This project features a good, hard-working team with good ideas and a nice design. The team has done an excellent job of assembling relevant collaborators and using collaborations. There is a good plan and progress is being made toward completion.
- Pressurized $H_2$ bulk storage is a significant cost in gaseous $H_2$ delivery infrastructure. In order to meet the Program's targets for the cost of $H_2$ delivered to a refueling station, it is necessary to reduce the cost of this storage. The approach taken in this project of utilizing low-cost concrete, which enables a reduction in the use of more-costly steel, holds the promise to meaningfully reduce the cost of $H_2$ bulk storage. The approach being taken is to design the reinforced concrete vessels using engineering calculations and existing pressure vessel codes, layout the manufacturing process in detail, estimate the cost of the vessel, and then optimize the design to lower the cost. All of this will be done before construction and testing. This is a very good and cost-effective approach. The project has excellent collaborators, including commercial engineering companies; experts on specialty concretes, high-strength steels, and friction stir welding; and DOT.

### Project weaknesses:

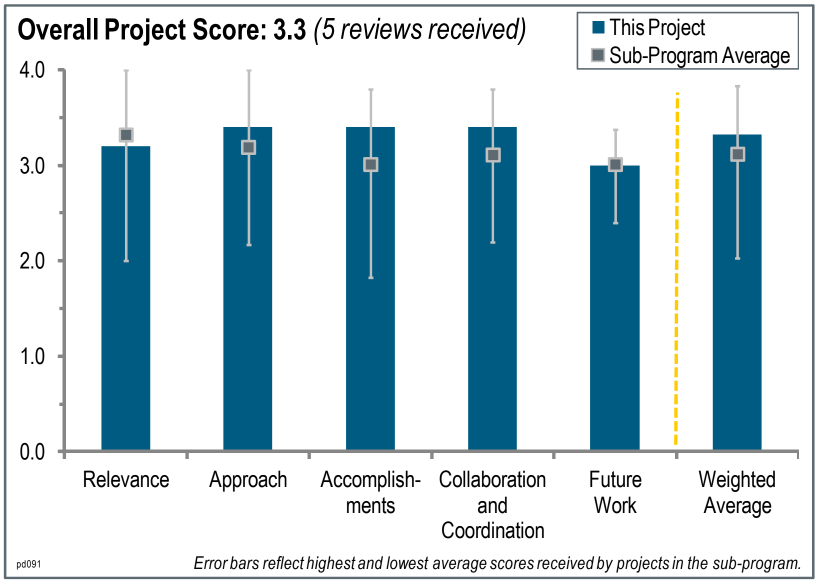
- The team should review the idea of using a high-cost stainless steel liner. It needs to demonstrate the concept.
- One weakness of this project is that $920/kg of $H_2$ seems high. This reviewer seems to remember Argonne National Laboratory publishing $20/kWh for automotive composite vessels that are higher pressure and lighter than these. This reviewer wants to know if infrastructure vessels are really expected to be more expensive than vehicle vessels, and if there is an advantage of these vessels that justifies the higher cost.
- One potential challenge appears to be that the technical requirements of the manufacturing process producing the pre-stressed concrete sleeve will be such that this manufacturing step will not be easily done in the field. Shipping the completed concrete-reinforced tanks from a central location may be challenging, particularly for the targeted 1,500-kg storage system proposed for fueling stations.
- Recent available data shows that carbon steel can be sufficient as an $H_2$ barrier and its $H_2$ embrittlement can be sufficiently mediated with proper design and thickness. The project should consider eliminating the costly stainless steel barrier layer to further reduce the cost. It would also be interesting to look at the cost of utilizing a carbon fiber composite-based inner vessel lined with high-density polyethylene as a barrier layer with reinforcing concrete to reduce the amount of costly carbon fiber needed.
- The team fails to take a truly global view toward materials selection and design for avoidance of $H_2$ embrittlement. For example, if the steel wall is in compression, because the concrete has to always be in compression, then the inner liner material is a redundant system for $H_2$ embrittlement avoidance. The inclusion of a redundant protection system is a great approach, but only if it is not expensive. Therefore, it is unclear why one would spend so much on a redundant system, especially when there may be much less expensive solutions (e.g., high density polyethylene, ceramic coatings, and inexpensive ceramic slurry coatings).

### Recommendations for additions/deletions to project scope:

- Recent available data shows that carbon steel can be sufficient as an $H_2$ barrier and its $H_2$ embrittlement can be sufficiently mediated with proper design and thickness. The project should consider eliminating the costly stainless steel barrier layer to further reduce the cost. It would also be interesting to look at the cost of utilizing a carbon fiber composite-based inner vessel lined with high-density polyethylene as a barrier layer with reinforcing concrete to reduce the amount of costly carbon fiber needed.
- The team should increase the scope to include the evaluation of the effects of different liner materials on $H_2$ permeation and the evaluation of whether or not liner materials are really needed (stress analysis).
- This reviewer has no recommendations at this time.

## Project # PD-091: Bio-Fueled Solid Oxide Fuel Cells

Gokhan Alptekin; TDA Research





### Brief Summary of Project:

The overall objective of this project is to provide ultraclean biogas to demonstrate the operation of a high-efficiency solid oxide fuel cell (SOFC) stack in a waste-to-energy application. More specifically, the objectives of the project are to: (1) develop and demonstrate a high-capacity sorbent to remove sulfur species from biogas, thereby providing an essentially sulfur-free biogas that meets the cleanliness requirements of SOFCs; (2) demonstrate operation of a 2 $kW_e$ biogas-fueled SOFC stack integrated with a biogas cleanup system in two different waste-to-energy applications; and (3) demonstrate the economic viability of TDA Research's sorbents to clean up biogas.


### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- The TDA Research gas cleanup system unit has already met the DOE goal to purify biogas for fuel cell applications. This helps to meet the objective of using domestic, clean energy.
- The effort is relevant because it will enable the use of fuel cells with biogas.
- Cost-effective biogas purification is of great value to the attractiveness of fuel cells because it allows for the use of renewable biogas.
- Sour gas cleanup is a well-established and highly mature technology that is practiced extensively on biogas and natural gas. It is not clear how this technology is superior to existing off-the-shelf cleanup solutions. See: http://www.quadrogen.com/wp-content/uploads/2011/10/Quadrogen-IBCS-for-Fuel-Cells.pdf and http://www.biothane.com/en/medias/articles/onion.htm.

### Question 2: Approach to performing the work

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

- TDA Research has a well-designed project that does an excellent job of addressing the barriers to desulfurization of organic sulfur-containing fuels. However, this reviewer is not sure how far along FuelCell Energy (FCE) is with producing its skid.
- The team features a good team of collaborators whose support upon successful completion of the project will be integral to accelerate commercialization.
- The skid-mounted system appears to be well designed and constructed. The landfillable sorbent is a good development. The sorbent has high sulfur capacity.
- The approach is consistent with the development of adsorbents. It was not clear if the down-selection process of the adsorbent material was based on the tests. The criteria used to assess the materials and how the results compare to the DOE targets were not addressed in the poster. The work only presented test results on $H_2S$, and not the more complex sulfides. Skid testing appears to be appropriate, yet from this poster it is difficult to assess

the viability of the design. Knowing what the design criteria were for the skid unit would be helpful. The breakdown of the work performed by TDA Research and FCE is unclear.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The TDA Research gas cleanup system unit is skid mounted and has already been successfully field tested.
- The purification team has conducted a thorough investigation of the challenges associated with biogas purification.
- The system has been developed on time and on budget. There has been minimal impact on the cost of electricity.
- It would have been helpful to have presented the work plan with the work schedule. It is difficult to assess the accomplishments from this work as presented, except for the development and build of the skid-mounted unit. Little detail on adsorbent efficiency, breakthrough, etc., was presented, especially with respect to the more-complex species and how it impacts the process. It was also not possible to understand the gas quality that the fuel cell will see. This reviewer wants to know why only $H_2S$ was discussed and what the adsorbents are. It is mentioned that siloxane cleanup is of interest, yet there is not any work presented on this species. It is reported that there are "expandable" and regenerative adsorbents, yet the presenter briefly explained that the skid is based on the expendable approach. It is unclear what tests were performed to decide this was the most viable approach. The piping and instrumentation design of the skid-mounted unit appears to be appropriate, yet without additional engineering criteria it is difficult to assess the viability of the unit, especially in light of the different species. This reviewer wants to know if the polishing bed can handle everything but $H_2S$. Slide 8 states that "we decided to use our own bulk desulfurizer…" This reviewer asks if whether this implies that from the work plan, a design and build test task (task 2) was not required.

### Question 4: Collaboration and coordination with other institutions

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

- TDA Research has assembled a very strong team for the design, manufacture, and testing of the fully integrated system.
- The collaborative efforts appear appropriate for this project.
- There is a strong collaboration team.
- Collaboration seems limited to TDA Research and its customers.

### Question 5: Proposed future work

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

- TDA Research is ready to move forward with the fully integrated system and test it at Cal-DeNier Dairy.
- The proposed work appears to be appropriate.
- The future work appears to be on target for a successful completion of the project.

### Project strengths:

- The project features a strong collaboration team.
- The project features a good design and a promising sorbent.
- There are several obvious strengths, including TDA Research's expertise in gas cleanup for natural gas, the fact that TDA Research has actually field tested its skid and is in the process of contracting with fuel cell manufacturers, and the successful field testing involving gas clean up for wastewater.
- TDA Research's experiences and expertise in sorbent development and process operations are significant and should facilitate the success of this effort. FCE is fully knowledgeable of integration challenges of this sort. FCE also fully understands (fuel) gas requirements for the SOFC technology.

### Project weaknesses:

- It is not clear that the project represents a major advance over the current commercial technology.
- One reviewer would have appreciated a chance to ask questions about the SOFC. Because biogas-fueled SOFC units are not that common, it would have been helpful to understand the perceived challenges associated with producing the SOFC unit as well as with full integration of both the biogas cleanup unit and the SOFC.
- It was not clear if there are weaknesses, yet it is easy to identify areas that might not have been thoroughly reported, presented, or addressed. The data reported at the dairy site was not comprehensive, and this reviewer wants to know if this possibly impacts the approach.

### Recommendations for additions/deletions to project scope:

- The team should perform a joint presentation with FCE next time.
- It may be worthwhile to explore the project's applicability to other technologies and for $H_2$ generation.
- This reviewer could not identify any recommendations.

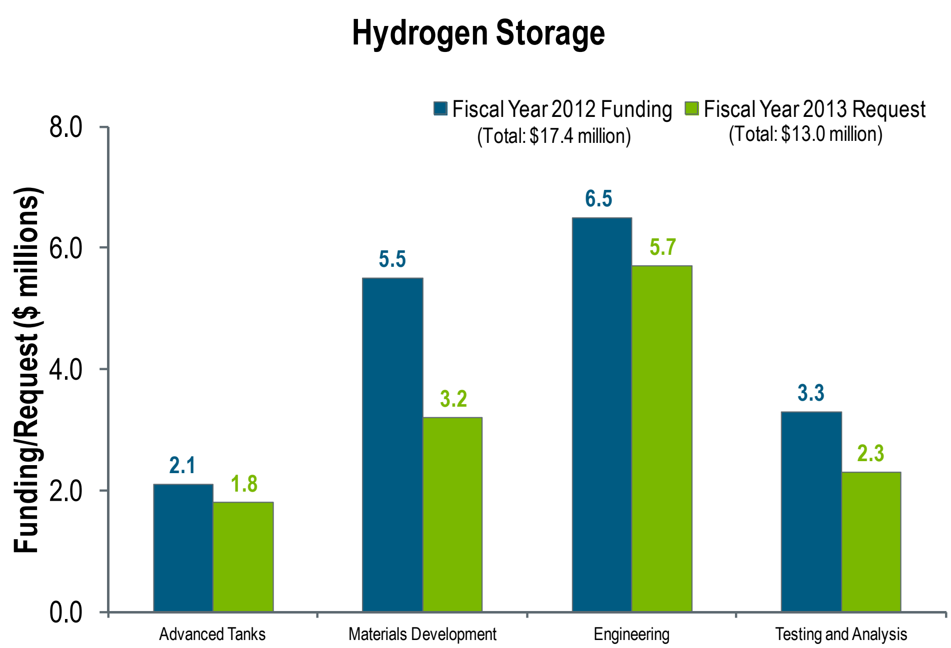
# 2012 — Hydrogen Storage
# Summary of Annual Merit Review of the Hydrogen Storage Sub-Program

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

The Hydrogen Storage sub-program portfolio was focused in fiscal year (FY) 2012 on system engineering for onboard transportation applications with continued effort in ongoing materials-based research and development (R&D) and physical storage options for near-term applications. Reviewers felt the sub-program was focused and very well managed with good progress shown in the Hydrogen Storage Engineering Center of Excellence (HSECoE). They also supported the sub-program's efforts in expanding into early market applications. Reviewers thought the sub-program was underfunded, making it difficult to support materials-based hydrogen storage to the level needed. Overall, reviewers commented that the sub-program is well managed and organized to focus efforts on achieving U.S. Department of Energy (DOE) goals, and it should continue to review materials and adjust the sub-program's priorities and funding to realize this aim.

## Hydrogen Storage Funding by Technology:

The chart below illustrates the appropriated funding planned in FY 2012 and the FY 2013 request for each major activity. In FY 2012, the sub-program received $17.4 million in funding, with a budget request of $13 million for FY 2013. The HSECoE continues to be a major activity for the sub-program. Work directed at lowering the cost of compressed gas storage for near-term commercialization is also a priority along with continued development of materials-based hydrogen storage. In some cases (such as materials development), the funding reduction reflects the completion of prior year projects, with little or no new projects planned in the area in the FY 2013 request.




## Majority of Reviewer Comments and Recommendations:

The Hydrogen Storage portfolio was represented by 29 oral and 11 poster presentations in FY 2012. A total of 25 projects (all oral presentations) were reviewed. In general, the reviewers' scores for the storage projects were good, with scores of 3.5, 3.0, and 2.0 for the highest, average, and lowest scores, respectively.

**Advanced Tanks:** Three projects on advanced tanks were reviewed, with an average score of 2.9. Overall, reviewers thought the work being done was very relevant and good progress was being made. Reviewers approved of the focus on lower-cost precursor materials and the melt-spun approach as key techniques to reduce carbon fiber costs. Reviewers thought the projects had strong collaborations but expressed some concerns with the speed of progress and suggested development of clear risk mitigation plans for each project.

**Materials Development:** Ten materials-based hydrogen storage projects were reviewed with a high score of 3.4, a low of 2.0, and an average of 2.8. In general, reviewers found the work on materials-based storage options, including investigations of metal hydrides, chemical hydrogen storage materials, sorbents, and liquid carriers, highly relevant to sub-program goals. Reviewers commented on the strong collaboration evident in many projects and noted the robust theoretical and computational efforts in explaining and guiding progress in materials development, particularly work on metal organic frameworks (MOFs). However, many reviewers felt experimental efforts could be more focused to provide stronger evidence in support of the theoretical work. Materials projects will continue in FY 2013, subject to appropriations, with an emphasis on a stronger link and feedback route between the experimental and theoretical efforts.

**Engineering:** Eleven projects were reviewed on hydrogen storage engineering, with a high score of 3.5, a low score of 2.9, and an average score of 3.3. Overall, reviewers believed the HSECoE made significant progress in FY 2012 with strong coordination and clear collaboration among the 10 partners. They also remarked on the difficulty in engineering complete systems without a material that currently meets all system requirements, but noted the importance of systems engineering occurring in parallel to materials development to help achieve the Hydrogen and Fuel Cells Program's goals. Substantial effort on chemical hydrogen and sorbent storage systems as well as physical storage tasks was regarded favorably by reviewers along with the development of key models. In general, for the individual partner reviews, the projects were thought to be well thought out with expert personnel to execute clear plans. Reviewers expressed concern about overlap between certain projects and a focus on ammonia borane at the expense of alane for chemical hydrogen storage systems. Reviewers believed work on elements not unique to specific materials was particularly beneficial, including engineering modeling of balance of plant issues, failure mode and effects analysis, and the systems analysis of hydrogen storage impacts on the global vehicular system. In general, it was thought the HSECoE and its partners were making good progress in evaluating materials-based storage systems and making decisions to meet DOE performance targets.

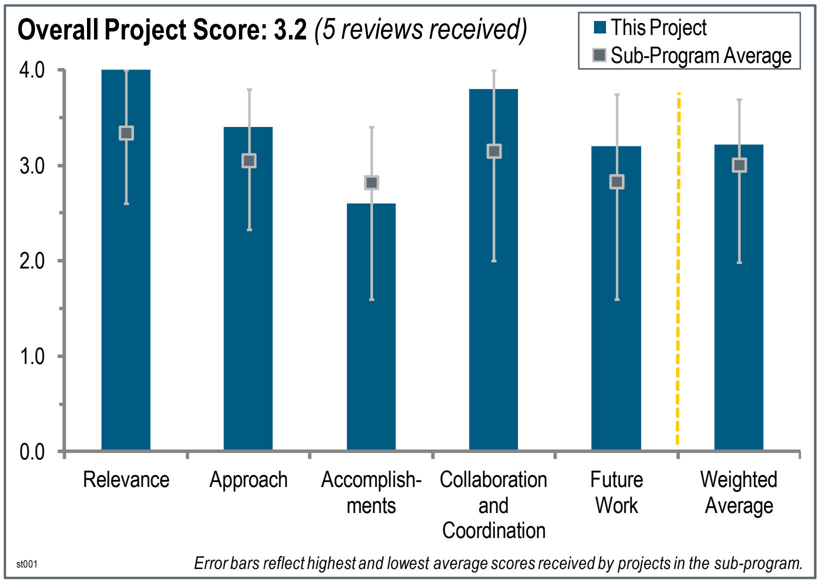
**Testing and Analysis:** One project related to testing and analysis was reviewed, with an overall score of 3.2. Reviewers felt this area was critical to the sub-program, as it provided important information and guidance for achieving DOE goals and successful program management. Reviewers believed this project made good progress in updating analysis of physical storage systems, para-ortho conversion of hydrogen in cryocompressed and MOF-5 adsorption systems, and completing onboard analyses for ammonia borane/ionic liquid and alane slurry chemical storage systems. However, they felt this effort would benefit from more industrial partners and that additional validation of model assumptions would be good. Overall, reviewers believed this work utilizes a strong team of analysts and consistent methodology to develop comprehensive analytical tools beneficial to the sub-program.

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

Rajesh Ahluwalia; Argonne National Laboratory


### Brief Summary of Project:

The objectives of this project are to: (1) conduct independent systems analyses for the U.S. Department of Energy (DOE) to gauge the performance of hydrogen ($H_2$) storage systems; (2) provide results to material developers for assessment against performance targets and goals and to help them focus on areas requiring improvements; (3) provide inputs for independent analysis of onboard system costs; (4) identify interface issues and opportunities, as well as data needs for technology development; and (5) perform reverse engineering to define material properties needed to meet the system-level targets.


### Question 1: Relevance to overall DOE objectives

This project was rated **4.0** for its relevance to DOE objectives.

- Accurate analysis is critical to guide the storage program and to ensure that storage options are reasonable at a systems level.
- This is a key project in terms of enabling meaningful cost estimates. This is a very important goal and is required for proper program management.
- Argonne National Laboratory (ANL) is providing in-depth and high-quality systems analyses that support the Hydrogen Storage sub-program with respect to the assessment of various storage approaches compared to performance targets for light-duty vehicles. ANL's results provide important insights on the attributes and limitations of current configurations toward meeting technical and cost goals. This information has been very useful for making go/no-go decisions on the continuation of several storage development projects as well as providing independent insight on the progress and potential of storage systems.
- This project provides comprehensive and quantitative systems analyses of $H_2$ storage approaches. The project is a solid complement to the work being conducted in the Hydrogen Storage Engineering Center of Excellence (HSECoE), and the technology assessments performed in this project provide DOE with an independent, objective evaluation of storage system options. The project fully supports the DOE research, development, and demonstration (RD&D) objectives.
- The $H_2$ storage system analysis is highly relevant to the DOE RD&D objectives because this type of effort provides a clear status assessment of the various storage systems to compare to the DOE targets. The project area that is not as relevant is the 70 MPa fast-filling analysis. This research is not aligned with the codes and standards effort that is occurring with the Society of Automotive Engineers (SAE) Standard for Fueling Protocols for Light Duty Gaseous Hydrogen Surface Vehicles, SAE J2601, and it is not consistent with the project scope, which should be focused on storage system analysis.

### Question 2: Approach to performing the work

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

- ANL has assembled a talented team with capabilities in several modeling areas to address relevant issues in the storage program.

- The principal investigator (PI) consistently has an effective approach to his system analysis. It could be improved based on additional steps to validate the models. In particular, the tank composite winding and fast filling should be confirmed based on data. Other system development models could benefit from a confirmation in the assumptions based on data or a reference to gain confidence in the transfer functions used within the models.
- The methods for carbon fiber (CF) tank work are good and appropriate. The tools are getting to be the right ones. It is not clear that the para-hydrogen to ortho-hydrogen work adds much to what Lawrence Livermore National Laboratory (LLNL) has already done. However, adding the para-to-ortho (PO) energy is a valuable addition to the existing model for cryo-compressed storage.
- The ANL approach generally considers essentially all of the relevant technical parameters needed to assess the ability of a given storage system to meet both the onboard and off-board performance targets. ANL collects and updates inputs from various sources to obtain reasonably complete descriptions of $H_2$ storage systems, and the laboratory's analysis methodology seems to be thorough and sound. The major limitation is the lack of sufficient details on specific properties of incompletely characterized systems (e.g., reliable reaction rates for $H_2$ reaction with the storage media in the appropriate operating temperatures, or important parameters such as thermal conductance of powders or compacted sorbents). ANL has exchanged information with several partners of the HSECoE. The consistent application of trade studies to determine the influence of various parameters is also valuable to identify which parameters have the most impact on achieving or limiting the performance targets.
- A solid technical approach focusing on the use of thermodynamic and kinetic models and trade-off analyses to evaluate different storage systems has been adopted. The major effort on this project in 2011 and 2012 addressed issues in two areas: (1) cost reduction and efficiency improvement in compressed gas storage systems utilizing CF-wound tanks, and (2) increased capacity in cryogenic systems employing metal-organic framework-5 (MOF-5) as a storage medium. Additional work included analyses devoted to optimization of conditions for $H_2$ discharge from a carbon, boron, and nitrogen (CBN) material, and a new process for improving the efficiency of ammonia borane (AB) regeneration. Although the approaches that were used to explore these different system options were well formulated, the priorities are somewhat concerning. First, there has been a tremendous amount of work conducted by tank manufacturers and other research centers on the reduction of CF usage without compromise of tank strength. Although the proprietary nature of some of that work is acknowledged, it is not totally apparent that the ANL project has provided any new or particularly useful information about CF tank improvements. Likewise, significant efforts are underway at the HSECoE and elsewhere on the use of cryo-confinement using MOF-5. There should be a more robust link between the ANL project and other DOE-sponsored efforts in this area. The efficient, off-board regeneration of AB and the elimination of contaminants (e.g., borazine, diborane, and ammonia) during onboard $H_2$ release from AB are critical issues that have not received enough attention. Although the analysis of a benzophenone-based process for AB regeneration was compelling, the overarching problem of hydrazine cost remains. Likewise, the issue of contaminant elimination in an onboard AB system is still problematic. Moreover, there is almost no work being conducted on the equally important problem of alane regeneration. Instead of focusing on problems that are already being thoroughly addressed (e.g., CF tanks and cryo-storage improvements), the impact of this project would be greater if the focus was on problems that are not already being explored extensively in other projects.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- ANL efforts during the past year were in updating assessments of physical storage systems—mainly compressed gas, assessments of P-O conversion of $H_2$ on cryo-compressed systems and on MOF-5 adsorption systems, and completing onboard analyses for AB/ionic liquid (IL) and alane slurry chemical storage systems. ANL's analyses indicate that serious limitations remain with all of the materials-based approaches for $H_2$ storage. ANL's assessments on the off-board performance of AB/IL point out that severe issues remain with the hydrazine regeneration of AB on the wheel-to-tank efficiencies below 20%. Once again the ANL team has investigated a broad range of systems in considerable depth, resulting in two valuable publications in the literature.
- Incorporation of P-O $H_2$ conversion is a good addition and a significant result. The AB regeneration work is a very important contribution. The project team should consider looking at economic analysis as well.
- In comparison to last year, the progress was not evident for the funding amount. Also, ANL was previously funded to assist in providing composite tank estimates for the cost estimating performed by TIAX. It appears that this work is being repeated for the new Strategic Analysis, Inc. (SA), (formerly Directed Technologies, Inc.)

work. In previous years, this project always provided a summary of the current $H_2$ storage technology that has been analyzed and updated. This year, the project review did not include any summary or reflection of any progress in the storage system attributes.
- The key function of compressed tank models came up unusually low in material used, which is a problem. The O-P conversion impacts are a useful addition to the model structure, though of course LLNL has already pointed out much of the results. However, it was useful to include this in the ANL model. Efficiency of AB processes is useful, though it may be that the project team feels that the very low levels of efficiency stated are acceptable, and this is absolutely not right.
- Good progress was made in all areas. However, a concern that was expressed in the 2011 review remains: only very limited information is provided concerning the remaining areas of risk as well as challenges that must be addressed for the systems that were investigated. Likewise, a detailed risk mitigation strategy is not evident. In addition, a discussion of related work that is being conducted in other laboratories would be useful in order to place the ANL work in the proper context. For example, the ANL results on CF tanks, MOF-5 cryo-systems, and AB regeneration should be compared and contrasted with those obtained in related efforts at other laboratories.

### Question 4: Collaboration and coordination with other institutions

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

- In general, fruitful collaborations with other organizations are evident. The overall impact of the project has been enhanced by those interactions; however, a closer collaboration with the HSECoE, especially on the cryo-compressed $H_2$ systems, is needed. The project is well managed, and the systems analyses are being conducted by a first-rate development and engineering team.
- ANL has identified a good set of partners to collaborate in its work. There is good collaboration with SA, PIs, and others.
- The collaboration is with the right people and is especially effective. This is not a list of people the project team talked to, but rather groups it has materially helped.
- ANL worked with TIAX in predicting both onboard and off-board costs for several storage systems and with SA on compressed gas systems. There were close interactions and an exchange of technical information with a number of other organizations within the HSECoE, which has been a benefit to the Hydrogen Storage sub-program. ANL also worked with the University of Oregon on regeneration reactions for the chemical storage system CBN.
- The collaboration with the industry is good, and ANL is active in various forums to provide information. It would still be useful to be engaged with the HSECoE project. In particular, the exchange of balance-of-plant assumptions would be highly recommended, as would a further comparison of approaches.

### Question 5: Proposed future work

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

- The proposed future work seems consistent with what the project team is charged with doing. The calibration of the tank model is a very, very good plan to calibrate models to known systems.
- It would be nice if ANL could look at alane regeneration.
- The future work outline appears fine except for the effort on the 70 MPa filling analysis. Also, additional proposals for tank optimization (i.e., end caps) need to be coordinated with a tank manufacturer and/or manufacturing trials.
- The future work is a straightforward extension of the 2011 effort. However, there is an overemphasis on physical storage, especially CF tank improvements. Those improvements are being addressed in detail elsewhere. The remaining resources should be directed to problems associated with AB and alane regeneration, contaminant reduction during $H_2$ release from AB, and improved CBN regeneration chemistries.
- There should be further comprehensive analyses of the compressed and cryo-compressed storage vessels that include variations in design configurations and optimization that address manufacturing constraints for safety and structural materials (e.g., CFs, aluminum versus stainless steel, optimization of designs, and fiber wrapping). ANL should continue, as planned, to assess the properties of promising chemical storage materials such as CBN

and related compounds. Other options also should be considered as information becomes available from new studies just started within the Hydrogen Storage sub-program.

## Project strengths:

- ANL has developed very comprehensive analytical tools for detailed engineering assessment of both the onboard and off-board aspects of $H_2$ storage. ANL's results appear to be very reliable and robust from comparisons that have been based on current knowledge and experience of others with available prototype and demonstration storage systems. The engineering staff at ANL has provided clear presentations of its methods and results. Analyses appear to be based on the best available data from various sources.
- The project has several strengths: a strong team of analysts, good collaboration with academic and industrial partners, and consistent methodologies for analyses.
- One strength is the history of models that allows a consistent comparison across technology.
- This ANL project has consistently been an excellent resource for $H_2$ storage system analysis. The analytical methodology is typically a strength of this project.
- This project is providing DOE with useful information concerning the design and implementation of an optimum onboard storage/delivery system. The analyses and engineering efforts are being conducted by a strong team with considerable expertise in thermodynamic and kinetic modeling for systems applications.

## Project weaknesses:

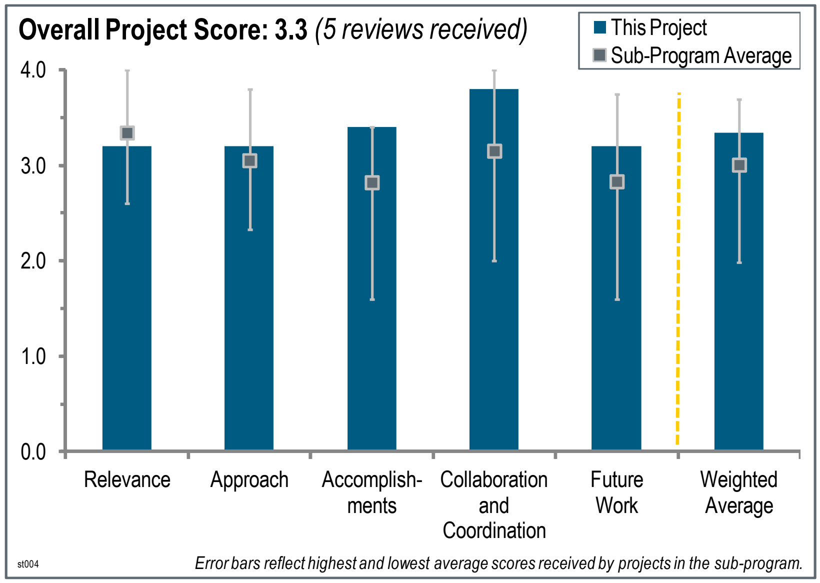
- DOE should strive to ensure that the analysis effort has the resources it needs to carry out relevant analyses.
- This project would really benefit from multiple industrial partners or coaches at least; DOE and its technology team are not enough. There is not enough contact with them to drive good realism into the code.
- A straightforward discussion of the present work in the context of previous studies is lacking. Likewise, a candid risk analysis and robust mitigation strategy for each of the candidate systems is missing.
- As was also noted in last year's review, the primary challenge for these analyses by ANL is the limited availability of reliable and complete reaction parameters (i.e., kinetics data) for the various $H_2$ storage media over sufficiently broad temperature ranges to generate robust predictions of performance in specific designs. Without the capability of generating the necessary input parameters, ANL appears to sometimes extrapolate properties outside of reasonable limits and may not be capable of fully establishing the correct behavior.
- As indicated, the project needs to include some level of validation or confirmation of the model assumptions. The composite tank analysis and the proposals for the end cap design need to be reviewed with a manufacturer to confirm the results. The end cap concept needs to have an understanding of how to join the end cap to the tank. The reason for the fast-fill analysis is unclear because 70 MPa Type IV tanks have also shown the need for pre-cooling.

## Recommendations for additions/deletions to project scope:

- The team should validate the composite tank analysis, seek proactive engagement with the HSECoE, and provide further validated information about the rates involved with the P-O conversion.
- The project team should delete the effort with improving the liner conductivity for the fast-fill analysis and replace it with analysis to study the increase in temperature limits from 85°C to a higher temperature. The physical storage systems should be de-emphasized in favor of more in-depth and extensive analyses of chemical storage systems. The problems and challenges associated with the latter (especially AB, alane, and CBN regeneration) are significant, and given the importance that is being placed on chemical $H_2$ storage materials in the DOE portfolio of engineering activities, a focused and innovative effort that addresses those problems is needed.
- ANL should continue to focus on comprehensive assessments of the physical storage systems in configurations that can be used in near-term vehicles and early market applications. Unless an AB regeneration scheme can be identified with greater than 40% efficiencies, there does not seem to be a need for further onboard and off-board assessments of the AB/IL storage system. However, more work on properties and behavior of chemical storage based on CBN materials and similar candidates is acceptable. ANL should look into the regeneration of $LiAlH_4$ via the dimethyl ether process developed by the team of Jensen/McGrady from the Metal Hydride Center of Excellence. Finally, there should be continued collaboration between ANL and the HSECoE to maximize information exchange.

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

Don Anton; Savannah River National Laboratory




## Brief Summary of Project:

The objective of this project is to develop materials-based hydrogen ($H_2$) storage systems for onboard $H_2$ storage for light-duty vehicles. This includes a three-phased approach of: (1) establishing system requirements and novel concepts (answering "where are we?" and "where can we get to?"); (2) novel concept modeling design and evaluation (answering "how do we get there [close the gaps]?" and "how much further can we go?"); and (3) subscale prototype construction, testing, and evaluation (putting it all together and confirming claims).

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- Given the fact that no material exists that comes close to meeting all requirements (including cost), engineering efforts may be premature.
- The project supports the DOE onboard $H_2$ storage objectives for three categories of technologies out of a larger set of choices. For the three methods the Hydrogen Storage Engineering Center of Excellence (HSECoE, the Center) is tasked with, it has done a very good job of improving and organizing the understanding of metal hydrides, chemical $H_2$ storage materials, and adsorbent systems.
- The Center is addressing a well-considered set of "barriers" to the $H_2$ storage system aspect of the DOE Hydrogen and Fuel Cells Program (the Program). The targets are based on realistic assessments of storage system requirements. The notion about the need to do materials development and system engineering in parallel is absolutely correct at the present stage of the Program. The spider chart approach provides a clearly illustrative means of tracking progress toward meeting targets in a timely manner.
- The HSECoE project is focused on evaluating different candidate storage systems and developing engineering solutions that utilize the material systems whose storage characteristics are most closely aligned with DOE targets. Unfortunately, no single material system that meets all of the DOE targets has been identified. Consequently, the overall technical effort has (by necessity) comprised materials development activities as well as the more relevant systems engineering problems. Overall, the HSECoE work is a vital part of the $H_2$ research, development, and demonstration program. However, the absence of an ideal storage material has generally limited its impact.
- If there were a material(s) that had the requisite properties to enable a viable storage system to be designed, the HSECoE relevance would be excellent. If surrogate materials are not sufficiently advanced, a viable prototype storage system may not be possible. In that case, the Center's relevance is in question and the resources devoted to the Center would have been better spent in materials discovery. However, it is too soon to know if the Center will proceed into Phase III.

### Question 2: Approach to performing the work

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

- The Center is doing a good job of understanding the onboard system. Little is being done on off-board systems. This appears to be a program issue not under control of the Center. The Center is doing a great job of using project management tools to manage its program and ensure that tasks are on target and on time.
- The overall approach would indeed be difficult to improve upon. The HSECoE is presently in the middle of Phase II and the approach appears to be working well. A major emphasis in terms of storage medium development is focused on adsorbents and liquid-phase chemical storage materials. Work on other materials options was discontinued for well-determined reasons. Go/no-go logics are being applied in a timely and definitive manner. A sensible plan is emerging in the approach wherein the Center will go forward with the best overall storage medium, where "best" is determined by how well the storage medium can collectively meet targets regardless of whether or not all of the targets can be met. It is probably going to turn out that by the end of Phase III, the final "best" $H_2$ storage system will still fall short in terms of meeting "system" gravimetric and/or volumetric storage targets, but that does not mean the system could not be applied in numerous present-day transportation infrastructures.
- The HSECoE has adopted an two-pronged approach focused on: (1) the evaluation of storage materials candidates and selection of the best material systems for incorporation into an engineering test system; and (2) design, implementation, and validation of innovative system architectures that employ those materials. The current materials limitations notwithstanding, this is a sound and compelling strategy, and the HSECoE has done a good job of identifying key technical barriers and developing engineering strategies that address those challenges.
- While an ideal material does not exist, the Center's approach is to use a combination of modeling and simulation and characterization of surrogate materials to address the engineering issues and challenges that must be overcome to design a prototype storage system. This combination of modeling and subcomponent and material characterization is sound. However, what is not clear is how close to the DOE targets a surrogate material needs to be to enable a viable storage system to be designed and built. The decision process to proceed into Phase III and build a system needs clarification and some additional DOE input. The management structure is sound and the matrix approach appears to be working well. Communication among the team members appears to be very open and cooperative.
- Given the limited materials and physical processes available, this project has done a good job charting a course to identify $H_2$ storage possibilities that also have the constraint of meeting the needs of light-duty vehicles and reasonable fuel infrastructure. This has been a long program with a reasonably good engineering approach. However, at about $6 million/year, it would be important to recognize when a no-go or down-select is appropriate, and act at that time, even if it is earlier than the planned go/no-go date. In that context, it would be interesting to know if work was performed on metal hydrides and dry, solid-phase chemical $H_2$ storage materials even though the no-go was known.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The year-over-year progress toward DOE performance goals for systems and processes for adsorbents and chemical $H_2$ storage materials has trended well. The engineering accomplishments were the primary reasons for this.
- The Center has done a good job on its $NaAlH_4$ system to understand performance optimization and trade-offs. The photos of the alanate pellet are frightening because they show significant degradation of the pellet. Adsorbent work to understand thermal and permeability issues is a significant accomplishment.
- Significant progress has been made toward meeting objectives and overcoming one or more barriers. The exploration and assessment of metal hydride storage materials was comprehensive and to the point. The rationale for ending work on metal hydrides was clearly presented at the DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR). The stepwise projection of plausible improvements that will allow 2017 gravimetric $H_2$ storage targets to be met for chemical $H_2$ storage materials is well thought out, but it is also very ambitious and seems (more than anything else) to depend on being able to increase the ammonia borane (AB) loading. The

baseline values for system gravimetric and volumetric $H_2$ storage for the adsorbent approach still fall significantly short of meeting 2017 DOE targets. Also, plausible progress steps that are proposed to bring both of these parameters into line with the targets are somewhat conflicted. The failure modes and effects analysis activity fits well into the work of the HSECoE at this time. It is somewhat concerning that the outcome of these studies may turn out to adversely impact the Center's ability to meet/sustain established performance targets.

- A careful and well-formulated effort has been conducted to down-select two classes of storage systems; namely liquid-phase chemical $H_2$ storage materials and adsorbent-based systems ("spider charts" provide a useful snapshot of material status relative to DOE goals). This has allowed the HSECoE to more effectively direct the technical effort and resources in future work. In addition, identification of important technical barriers has been provided. That said, there are three general concerns: (1) Based largely on the excellent work conducted in the Chemical Hydrogen Storage Center of Excellence, AB has been selected as a candidate for further development in the HSECoE. Although "$H_2$ purity" is mentioned as a technical challenge, the use of multiple scrubbers to remove those compounds seems unwieldy and cumbersome, and implementation in a complete system seems problematic. A more innovative solution is needed. (2) $AlH_3$ remains a solid contender for development in an engineered system. However, the Center is so strongly focused on the AB system that alane is becoming marginalized. Moreover, it seems unlikely that the engineering solutions that apply to the exothermic AB system will be appropriate to an endothermic release material such as $AlH_3$. (3) Given the rapid pace of adsorbent material development, other candidates (besides metal-organic framework-5 [MOF-5]) should be carefully considered and evaluated for the cryo-adsorbent system application—hopefully that will be done in the system architecture work next year.
- The accomplishments in 2011 have been very good. Down-selection was completed for the reversible metal hydrides and characterization data for AB, and the activated carbon sorbent AX 21 was obtained, which allowed critical issues to be identified and subsystem designs to be developed. Potential improvements for each system have been identified and their impact on system characteristics has been projected. If all of the improvements are realized, the AB system appears able to meet the onboard 2017 system targets. Some compromises in gravimetric density of adsorbents need to be made to meet the volumetric density and cost. In addition, failure modes and effects analyses for both systems were completed. Preliminary cost estimates for each system have been developed, but it is not possible to assess whether the costs will meet the DOE targets because they have not been finalized by DOE and the U.S. DRIVE Partnership partners.

### Question 4: Collaboration and coordination with other institutions

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

- The Center appears to continue to do a very good job in coordinating the program and the member institutions.
- The Center has assembled a good team to carry out its work and appears to be taking a very proactive approach in promoting active engagement and collaboration among its members.
- Coordination across the Center is first rate. Individuals and organizations that are assigned various aspects of the effort report their progress to their respective system architect, who is responsible for all of the development activities along a given pathway. Information flow from the previous three materials centers is facilitated by individuals transitioning from those centers to the HSECoE team.
- The collaborations with partnering institutions are very well coordinated by the HSECoE; the evidence for this appeared seamlessly throughout the presentation by Don Anton (as it should be in an effectively orchestrated multi-partner project). Indeed, Savannah River National Laboratory (SRNL) has accomplished this level of partner interconnection through careful selection of collaborating institutions and personnel, regular progress meetings, and common databases.
- A solid management plan is in place and good communication channels seem to be in place among the many partners in the Center. However, given such a large and complex entity, a fully engaged coordinating council or executive committee is of vital importance to assist the director in assessing progress, coordinating activities, and suggesting possible "mid-course corrections" and redirections of technical work that will undoubtedly occur. The role of a coordinating council is missing from the "management" section of the presentation.

### Question 5: Proposed future work

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

- The future plans are focused on well-considered key "steps" that have been identified (and are being intensely investigated) for boosting all underperforming system parameters to the 2017 DOE system target values.
- The future work on the chemical $H_2$ storage material and cryo-adsorbent systems is given in sufficient detail to provide confidence that a well-focused technical effort will occur in those areas. Reasonable milestones are in place, and a technical plan consistent with meeting those milestones has been formulated. A more extensive effort on developing an alane-based engineering subsystem would be desirable.
- Given the low efficiency and high cost of AB regeneration, the Center should not work on chemical $H_2$ storage material issues unique to AB, such as ammonia removal. Its focus should be on issues common to any off-board regenerable materials.
- The future work plans are good and address the unresolved component design and performance issues and challenges leading up to the Phase III decision point. The Center has made projections for future improvements in materials and component characteristics that indicate a pathway to meeting the DOE targets for the adsorbent and chemical $H_2$ storage material systems. It would be helpful to understand the likelihood of each of these steps achieving the desired improvements, for example, the likelihood that the thermal conductivity of MOF-5 will be increased by an order of magnitude.
- Regarding the proposed future work on chemical $H_2$ storage materials and adsorbent work on slide 38, it is unclear if there is enough time in the 11 months left before the go/no-go decision. The presenter did mention the possibility of a no-cost extension, and at 55% complete on 3/31/12, it could easily be necessary. There may be challenges in trying to increase the AB concentration in the slurry from 65% to 85%. There are a number of time-consuming variables to consider in understanding the implications of liquid and powder properties on slurry rheology.

### Project strengths:

- This project features excellent management, effective collaborations, a well-orchestrated approach, a properly focused emphasis on the research and development tasks, and an overall plan that promises to be a success story at some level.
- The Center has assembled a great team. Center management has been very proactive in managing the team and keeping it focused on tasks and overall goals.
- The project has an excellent team and management structure that takes full advantage of the experience gained in the earlier materials centers. It also features excellent collaborators. The project made good response to comments from last year's review comments.
- The main strength is the aggregate set of capabilities of all of the partners. The general approach in meeting the objectives has probably helped keep focus on a project with a wide range of diverse tasks and technologies to meet a common goal.
- The HSECoE comprises a well-qualified team with strong backgrounds and expertise in material assessment, engineering modeling, and prototype development. A solid technical approach has been adopted, and there has been good progress on meeting the overall goals of the project. Specifically, the Center should be recognized for conducting a careful and objective evaluation of multiple material candidates and making the go/no-go decisions that were needed to provide a better focus for development of system solutions in the future.

### Project weaknesses:

- There really are not any weaknesses, as far as the SRNL component of the HSECoE is concerned.
- The overarching problem that faces the activities in the Center is that no single material that meets the DOE targets has been identified. This has forced the Center to concentrate on a variety of less-than-ideal systems. Hopefully, the information gained from that work will translate effectively to a more capable material system that may emerge in the future.
- The Center focuses on the storage system and performance onboard. Very little is being done on off-board regeneration. The entire system needs to be optimized rather than just the onboard system. As stated earlier, this is an issue for the Program—it is not under the Center's control.

- One weakness is the lack of a suitable material that would enable a truly viable materials-based storage system. This is not necessarily the Center's fault, but it could lead to a diminution of the Center's accomplishments.

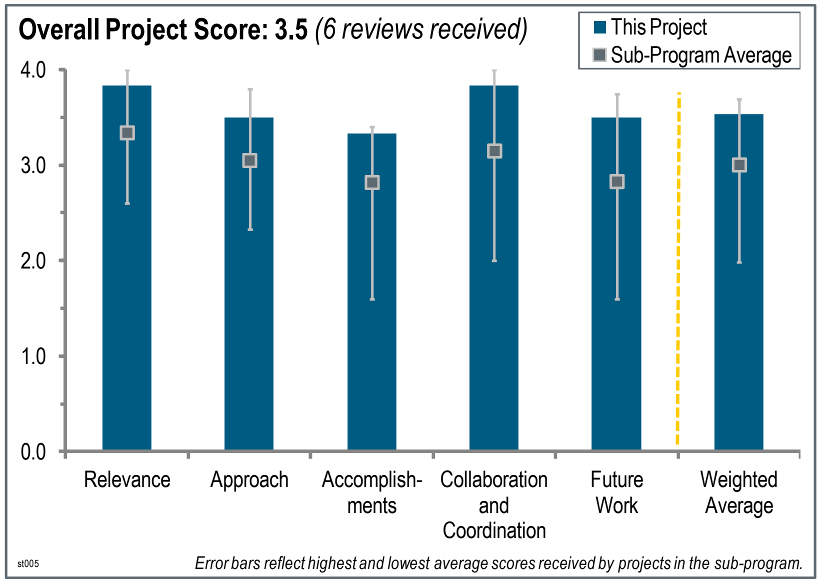
### Recommendations for additions/deletions to project scope:

- There should be more collaboration with analysis efforts to include forecourt issues.
- In next year's AMR presentation, the project team should consider an overlay approach to the spider charts that should project the year-to-year progress made by the HSECoE. This reviewer has been assured by Don Anton that the modeling activities being performed across the HSECoE are all appropriately integrated and use the same framework, basis, etc. At the 2013 AMR it would be a good idea to illustrate to the audience how this integration is accomplished.
- A more robust effort on the design and development of an alane-based subsystem is recommended. The present work focuses strongly on AB. It is unlikely that the engineering trade-offs identified for that material will apply equally well to alane. The Center management and coordinating council should take precautions to avoid unnecessary overlap of activities in this large and complex center. Special attention should be paid to limiting the duplication of system modeling efforts in the National Renewable Energy Laboratory (project ST-008) and Ford/BASF-DE/UM (project ST-010) projects. Finally, it is known that work on developing a strategy and engineering solution to off-board regeneration is outside the scope of the Center activities. However, the cost and efficiency of the regeneration process(es) remain daunting and critical challenges. The DOE Office of Energy Efficiency and Renewable Energy should consider a parallel activity to more fully explore system solutions to the spent fuel regeneration problem.
- Consider a "reality check" modeling step that uses the bill of materials' output from the HSECoE system engineering and couples that to the system's capital and fuel cost model. Such a step may illuminate bad cost trends early and save some time. The AB slurry is still subject to AB's thermal stability at temperatures in the 50°C to 80°C range. For a fuel transported and stored in an infrastructure or on tanker trucks that can be in locations where the air is 50°C–55°C, it would be worthwhile to look into this. It would be easy to do a time-temperature-decomposition study in an environmental chamber, or in Palm Springs in the summer.

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

Jamie Holladay; Pacific Northwest National Laboratory

**Brief Summary of Project:**
The overall objectives of this project are to: (1) design chemical hydrogen ($H_2$) storage system and balance-of-plant (BOP) components; (2) develop system models to predict mass, volume, and performance; (3) reduce system volume and mass while optimizing storage capability, fueling, and $H_2$ supply performance; (4) mitigate materials incompatibility issues associated with $H_2$ embrittlement, corrosion, and permeability; (5) demonstrate the performance of economical, compact, lightweight vessels for hybridized storage; (6) guide design and technology down-selection via cost modeling and manufacturing analysis; and (7) perform value engineering of BOP to minimize cost, volume, and mass.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- Both the tank analyses and the chemical $H_2$ storage work are highly relevant.
- There are few current technical options for vehicular $H_2$ storage, and the options in this project represent a major hope for providing an $H_2$ storage solution for this key application—light-duty transportation.
- This project is relevant to DOE's objectives for the development of viable onboard physical and chemical $H_2$ storage systems. Pacific Northwest National Laboratory (PNNL) provides analysis and data in support of the Hydrogen Storage Engineering Center of Excellence's (HSECoE's, the Center's) go/no-go decision on chemical $H_2$ storage options.
- The HSECoE's value is good, but it would be outstanding if researchers were closer to materials that had the potential to meet DOE targets. Cost and production efficiency of ammonia borane (AB) will likely preclude its use as a storage material. Vessel optimization is appropriate because it spans all technologies. Engineering modeling of BOP issues is important.
- This project is a centerpiece of the overall HSECoE. In this project, the key deficiencies in chemical $H_2$ storage material properties are addressed, with an emphasis on mitigating those deficiencies up to the point of meeting or surpassing 2017 DOE $H_2$ storage system performance targets. This is done through a process of materials property enhancement and the development of novel engineering solutions. As such, this project contributes key materials performance and system architecture information needed for full system analysis. In addition, the project develops and transmits modeling/simulation tools, prototype systems, engineering methodologies, and similar tools to the greater $H_2$ storage community.
- It is very important to demonstrate commercially viable $H_2$ storage systems other than cryo tanks. It is a mistake that there is not ongoing material development working in parallel with the HSECoE. The impact of system operation and packaging on the chemistry of the fuels is not possible to predict a priori, and engineers do not have sufficient molecular level expertise to "engineer" solutions to these "material deficiencies." A cynic might suggest that perhaps the engineers could consider engineering to accommodate a material's set of properties instead, but it is more appropriate to just be practical and recognize that the DOE goals are very challenging and cannot be expected to be achieved without a multifunctional team that includes a widely diverse set of technical

backgrounds and synthetic and physical chemists along with the engineers. The PNNL team appears to have the balance necessary to achieve DOE's goals.

### Question 2: Approach to performing the work

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

- Finite element analysis on wall thickness is appropriate and well-designed and well-executed. Materials screening for Type IV tanks is appropriate and done well.
- PNNL provides strong support of HSECoE activities in chemical $H_2$ storage, pressure vessel, and BOP. The project utilizes both modeling and experiment efforts to address DOE targets in weight, volume, cost, durability, and efficiency. There is a lack of information on BOP components (heat exchanger, pumps, valves) that could work with PNNL's AB slurry fuel system.
- The comparison of exothermic AB and endothermic alane slurry approaches is very good. The slurry AB approach is interesting, but identification of the best liquid for the slurry approach is the key. Also, slurry instability (settling out of the reacted AB) is an important issue. It is unclear if the slurry approach has been compared to the AB ionic liquid approach.
- This project features a very good approach, mixing the pressure vessel and choices for endothermic and exothermic material options with options for slurry and liquid. There is a statement on slide 4 about guiding design and technology down-selection using cost modeling and manufacturing analysis—this also needs performance as a criteria. It may be implied in the slide that discusses combining predictive models with cost models, but that should be in a broader context for this overall project.
- The study of pressure vessels as an enabling technology is a key aspect of this project. The manufacturing and performance issues being addressed are central to the goal of meeting DOE 2017 $H_2$ storage system cost and performance targets. The approach to chemical $H_2$ storage system development is focused on reducing system volume and mass to improve the overall performance of the system in terms of deliverable $H_2$ per system volume, system weight, and system cost. Experimental and modeling studies are appropriately integrated to optimize system performance parameters, mainly in the context of meeting DOE 2017 $H_2$ storage system targets. A number of other important $H_2$ storage system issues are also addressed in ways that are generally applicable to all storage material concepts. Information pertinent to go/no-go decision points is being developed in a concerted manner that is well aligned with HSECoE objectives and timelines.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The incorporation of cost into tank models is a significant step. Vehicle level modeling to predict materials performance is well done.
- It would appear that the slurry alane results are unfavorable compared to the slurry AB results, with regard to the gravimetric storage system characteristics. The project team did nice work on the optimization analyses of the tanks.
- Good progress was made in characterizing AB slurry properties. The settling of spent slurry (within hours) could be problematic for refueling. Cryogenic testing of polymer liner material provides critical data to assess the applicability of Type IV tanks for cryo-adsorbent or cryo-compressed $H_2$ storage options. It is not clear what fatigue limits are referred to in PNNL's analysis of Type III tanks. It is unclear if they are derived from stress versus the number of cycles to failure curves established by ASME, and whether the finite element analysis accounted for the changing material strain/stress behavior after hundreds or thousands of cycles.
- Significant progress toward meeting HSECoE program objectives and elucidating pathways to eliminating performance barriers has been made in the past year. In the pressure vessel area, there are still uncertainties and limitations that need to be resolved, particularly with respect to demonstrating Type IV vessel performance and meeting cost targets. In the chemical $H_2$ storage material development area, gravimetric storage density and plant efficiency targets remain to be met. Accomplishments from the liquid slurry work are both impressive and encouraging.
- The study of polymer liner material, dehydration kinetics, and the initial phase of AB slurry properties have been valuable accomplishments; combined with the other accomplishments, they are still small in comparison to the

significance of the accomplishments hoped for from the future work. Overall, most of the leverages of this project seem to rest on the future work.

### Question 4: Collaboration and coordination with other institutions

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

- This is a well-balanced team of scientists and engineers.
- This project features good work across Center partners and vendors.
- The project seems to be effectively coordinated and used partners' capabilities and contributions.
- PNNL has close interaction and collaboration with members of the HSECoE. More active information exchange with members of the Storage Systems Analysis Working Group is encouraged.
- The collaborative activities that connect to this project are all clearly aligned with specific needs and objectives of the HSECoE in areas that support the tasks and responsibilities assigned to PNNL. Each collaboration addresses a clearly defined need or issue that fits into the pathway for completion of key performance and cost validation milestones.
- It is unclear what the collaboration is with Los Alamos National Laboratory (LANL) on the AB ionic liquid approach.

### Question 5: Proposed future work

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

- The future work looks reasonable.
- Future work is a continuation of ongoing effort in chemical $H_2$, pressure vessel, and BOP. There are many practical limitations for a slurry $H_2$ storage system, both onboard and off-board; therefore, PNNL should consider collaborating with LANL in developing a viable AB liquid fuel formulation.
- The only recommendation concerns the work with alane. The HSECoE's effort to standardize on the same alane, specifically, the ATK macrocrystalline alane that was manufactured in the former Soviet Union, is appreciated. However, the HSECoE needs to ask itself if this alane is truly representative of the alane that will be manufactured on a large scale, should this material pass a go/no-go. The HSECoE also needs to consider if the ATK macrocrystalline alane stands a chance of passing a go/no-go if this form of stabilized alane does not form slurries as well as alane manufactured differently on a large scale, such as microcrystalline with a large surface area and potentially different surface stabilization chemistry.
- The future plans clearly build on past progress and are indeed sharply focused on performance and cost barriers. Sensitivity analyses, component performance validation, and advancements in the level of design detail on a component-by-component basis characterize the proposed work for the coming year. There are still some serious issues with the alane approach that need to be resolved. Perhaps an all-out effort on the AB slurry approach would be the best way to focus activities and resources for the coming year.
- Work should focus on generic rather than materials-specific issues. Because current materials are surrogates, problems specific to AB or alane should be ignored and resources should focus on problems that are common to more potential storage materials should be investigated. Cost analysis, pressure vessel, and BOP issues are good issues for this project.
- The ability to optimize the vessel based on cost with the predictive/cost model should prove to be very helpful. The work cited in the Future Work slide covers the main, necessary areas. The only concern is in the study of the property and behavior effects of the slurries on performance. Validation of slurry system component options is very important. There is concern about the implications of slurry properties on the valves, pumps, and heat exchangers. Optimum slurry properties may conflict with possible operational envelopes for fluid components. Steady-state properties as well as cumulative properties such as particle agglomeration at some fluid system discontinuity or other element need to be understood and considered.

### Project strengths:

- The project features excellent experience with solid AB.
- This project has a well organized plan, and a capable and coordinated team.

- The PNNL team has significant expertise to tackle several different areas in $H_2$ storage, both in analysis and experiment.
- Overall, the relevance, approach, and task planning are outstanding. The quality of the project management and the orchestration of the collaboration activities are also major strengths.

### Project weaknesses:

- Slurry fuel formulation may not be practical in many aspects of operation and regeneration.
- There are no significant weaknesses. Accomplishments over the past year were impressive in many respects, but there are still some gaps in the performance area that need to be closed.
- It is not certain that the AB slurry approach is clearly superior to the AB ionic liquid approach.
- The slurry study should lean more toward the science of slurries and less on the art. The current direction seems to focus on bulk slurry properties, with insufficient details on the microscale properties. The project team also needs to study rheology behavior such as viscosity, settling, agglomeration, and flocculation on the particle scale to fully understand issues and mitigation methods. In addition, adding the engineering knowledge related to how slurry properties are affected by the actions of shear flow, valves, pumps, and other components will help in completing the engineering model. Often these elements can deagglomerate, stir, or re-average the slurry to the benefit of the system.

### Recommendations for additions/deletions to project scope:

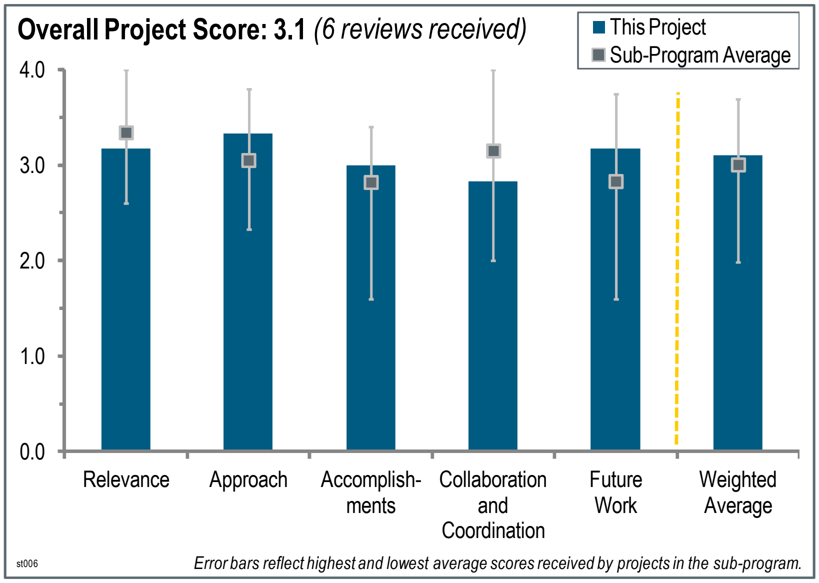
- The researchers should increase slurry study.
- PNNL should consider collaborating with LANL in developing a viable AB liquid fuel formulation.
- Some thought should be given to focusing resources on the best performing chemical $H_2$ storage material candidate in the coming year to allow more opportunity for achieving validation of all performance targets.
- The presentation indicates that the silicon oil for the AB slurry approach is not the ideal fluid. If not, it would be good to know why not, and what type of fluid is the ideal fluid. The researchers might benefit from an examination of the literature on the colloidal chemistry of non-aqueous media.

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

Bart van Hassel; United Technologies Research Center

### Brief Summary of Project:

The prime objective of this project is the design of materials-based vehicular hydrogen ($H_2$) storage systems that will allow for a driving range of greater than 300 miles. The project makes use of in-house expertise in various engineering disciplines and prior experience with metal hydride system prototyping to advance materials-based $H_2$ storage systems for automotive applications.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- This project is well aimed at DOE Hydrogen and Fuel Cells Program (the Program) targets and barriers. It is an important support component of the Hydrogen Storage Engineering Center of Excellence (HSECoE).
- With no viable materials for storage, the value of engineering analysis on proxy materials is uncertain.
- The development of integrated storage system modeling assists in down-selecting materials systems with the potential to achieve DOE targets.
- This project is investigating the engineering aspects of materials-based automotive $H_2$ storage systems. Conducting such studies is important to see if a certain $H_2$ storage material that is promising at the material level will have the requisite performance and weight/volume characteristics needed for use in fuel cell vehicles. One example of the usefulness of such studies is the determination that known onboard reversible metal hydride systems will not meet the automotive requirements. As a result, metal hydride systems have been dropped from further consideration in this project (see slides 5 to 9).
- The United Technologies Research Center (UTRC) provides oversight and also contributes in a major way to an integrated measurement and modeling capability that is profoundly important to the Program. All aspects of the project are fully supportive of DOE research, development, and demonstration objectives. The simulation framework developed and applied in this project allows for quantitative comparison of $H_2$ storage system options on a common basis and contributes underpinning to go/no-go decisions within the HSECoE.

### Question 2: Approach to performing the work

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

- The approach is excellent. It focuses on several important problems that must be solved to make an onboard $H_2$ storage system practical and commercially viable. The approaches are both analytical and experimental, as the individual cases may dictate. Initially all three storage media were considered: reversible hydrides, chemical $H_2$ storage materials, and cryo-adsorbents. It is important that risk analysis is included.
- The gas-liquid separation (GLS) work is relevant and should be applicable to all chemical storage systems. Ammonia removal is good work that is applicable to systems where acid or base impurities are present. Because the more difficult impurities are unique to ammonia borane (AB), it is probably acceptable to ignore them.
- As a baseline design, the GLS component in the GLS Test Facility looks like it should have considered the potential for slurry particulate problems. It is actually a gas-slurry separator. It would be good to learn what other GLS designs or GLS components were considered in this design.

- The approach was to develop system models for the various types of $H_2$ storage materials and then compare the performance and other characteristics to the requirements identified as DOE 2017 targets. Such systems were defined to a sufficient level of detail to enable assessment of system performance versus requirements on a common basis for each option. Vehicle, fuel cell, and component models were based on data and process information from one or more project team members and other sources.
- UTRC plays a centralized role in the Integrated Power Plant Storage System Modeling (IPPSSM) team within the HSECoE. UTRC also has responsibility for key aspects of chemical $H_2$ storage material process operability, $H_2$ quality maintenance, thermal conductivity enhancement, and failure modes/effects analysis. These are all pivotal engineering pursuits that should serve the greater HSECoE in many illuminating and beneficial ways with respect to refining and optimizing overall $H_2$ storage system performance.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Estimation of the tank weight and volume for hydrides based on systems modeling was a key result.
- The improved efficiency of the $NH_3$ regenerable separator is noteworthy.
- The project has produced reasonable results in compaction. The determination of particulates and control is a significant accomplishment.
- Two qualitatively different metal hydride systems were designed and analyzed, one using only the fuel cell waste heat for $H_2$ discharge, and the other with an $H_2$ combustor for the needed thermal energy (slide 6). Based on the available hydride materials (slide 9), it was determined that none of these materials could meet the automotive requirements. For the liquid chemical $H_2$ storage material option, the major effort was in the design and testing of the GLS, including a failure mode analysis for this component. For $H_2$ from AB systems, metal-chloride-based systems were found to achieve approximately 10 wt.% ammonia sorption capacity at 25°C. For cryo-adsorption systems using super-activated carbon, mechanical, thermal, and plasma compaction/sintering was examined as a faster alternative to using a binder.
- UTRC has made significant progress toward further elucidating, and to some extent overcoming, one or more barriers to $H_2$ storage system performance in the past year. IPPSSM contributed to the "diversion" of the metal hydride $H_2$ storage approach based on the identification of sizable gaps in the material properties needed to meet DOE's 2017 $H_2$ storage targets. There had been some encouraging progress reported for $H_2$ purification, activated carbon compaction, and metal-organic framework-5 (MOF-5) thermal conductivity enhancement; ground work has been laid for GLS validation tests. A substantial portion of the important, planned laboratory experimentation for the current fiscal year (FY) remains to be done in the latter quarters of FY 2012.
- Consistent with the level of funding for this project, a large number of good and useful results have been generated. The project's modeling results on reversible hydrides have helped to determine that this family of materials is not likely to meet the DOE 2017 system targets for onboard storage, and have thus helped make the way for this family of media being removed from the HSECoE. This is a very useful result. For other good results (e.g., liquid-gas separation, $NH_3$ removal, particulate removal, MOF composite compaction, and conductivity enhancement), it would be good to see more preliminary thoughts as to practicalities for a real system—in particular, cost and complexity.

### Question 4: Collaboration and coordination with other institutions

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

- Although several collaborators are listed, the nature of collaboration is not obvious; in other words, it seems to be within information sharing.
- This project is based at the HSECoE. The multiple team members in the HSECoE include industry, national laboratories, and a university, and these team members bring multiple viewpoints to bear on work. The regularly scheduled face-to-face meetings (see slide 18) help to enhance effective interactions among the team members.
- Close, well-integrated collaboration seems to prevail throughout the HSECoE, and this is likely as true for UTRC as it is for other HSECoE partners. However, unlike most of the other HSECoE partner presentations, this aspect of the UTRC effort was not clearly elaborated on in the UTRC presentation.

- The HSECoE partnerships were briefly listed, but they were not discussed in much specific detail. With a few exceptions, the slides list mainly UTRC. Given the joint publications listed in the reviewers-only slides, there must be more detailed collaborations that can be provided.

### Question 5: Proposed future work

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

- The proposed future work is reasonable.
- Given the high cost and low efficiency of AB regeneration, the HSECoE should consider dropping the AB work.
- Future work is limited to only one slide (slide 18) that seems to basically reflect a budget-revised operating plan. It would seem that a full analysis of the results obtained thus far should imply some other changes in direction to maximize the value of the project.
- The plan for work in the coming year (as displayed in slide 18) is well conceived, and all the tasks are sharply focused on mitigating $H_2$ storage system performance barriers. It is recognized that UTRC did have to restructure its work plans after the “diversion” of the metal hydride storage approach.
- The concept of expanding the system models developed for hydrides into cryogenic and chemical $H_2$ storage materials seems to be missing. It would be helpful to focus on this and the failure mode and effects analysis (FMEA). With regard to $H_2$ quality research, it is recommended to communicate with companies and institutions that specialize in this technology in order to accelerate the progress and establish feasibility of having the impurities at levels within polymer electrolyte membrane fuel cell tolerance.

### Project strengths:

- The system modeling and experience with metal hydrides tanks are strengths of this project.
- This project features good engineering analysis by a strong team.
- Excellent analytical and engineering skills are incorporated in this project.
- The different $H_2$ storage options are being analyzed in a similar manner to allow for performance comparisons on a common basis. The various team members have different types of expertise, which helps to identify issues in the design or analysis. The FMEA is worth doing for critical components, even at this early stage of system development.
- UTRC has the expertise, experience (especially in $H_2$ storage development), and facilities to perform effectively as a key partner in the HSECoE. The UTRC role in establishing the IPPSSM framework is critical to the success of this important system analysis/validation activity.

### Project weaknesses:

- There are several unrelated tasks.
- There is not enough iteration between recent results and changes in future directions. It is unclear if the collaborations are optimum.
- This project has no obvious weaknesses. A chart that clearly reveals how the UTRC effort interfaces with the efforts of other HSECoE partners should be included in next year’s DOE Hydrogen and Fuel Cells Program Annual Merit Review presentation.
- The component and system designs are based on materials whose performance is not quite up to the requisite values yet. The actual materials, when developed, may have properties different from what has been tested. For example, the compaction techniques and thermal conductivity enhancements investigated for super activated carbon may not apply to MOF-based or other sorbent that might be better for the automotive application. Unfortunately, the surrogate materials are the only ones available to work with at this time. Beyond the ammonia mitigation, there was no discussion of the engineering issues related to AB systems.

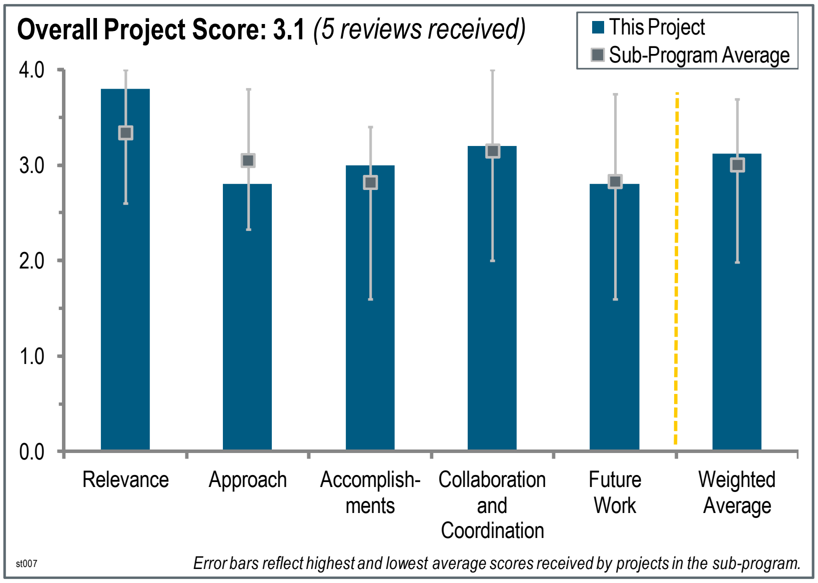
### Recommendations for additions/deletions to project scope:

- The scope of activities in this project seems appropriate. Critical barrier issues are being addressed in every task.

- The project team should communicate and/or outsource the $H_2$ quality work to companies and institutions that specialize in separation technologies. The team should also use current experience with metal hydride system developments and apply it to cryogenic and chemical storage.
- It may be worthwhile to monitor particulate formation with time. Oxide catalysts often produce significant fine particulates in early stages of operation and then show almost zero fine particulate formation for the remainder of their useful lives.
- The only recommendation is that the team should carefully make use of current results to change directions as necessary to provide maximum value to the HSECoE.
- Based on the FY 2012 and FY 2013 plans shown on slide 18, it is not clear if the investigators intend to address the thermal issues related to $H_2$ discharge in AB systems—the design, configuration, and process control in this reactor are likely to pose significant engineering challenges. The two metal hydride systems shown schematically on slide 6 are configured without an $H_2$ buffer tank. For AB or other materials-based systems, $H_2$ will be needed for start-up, and the needed buffer tank should be included in the system configuration (and in weight/volume assessment).

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

Troy Semelsberger; Los Alamos National Laboratory




## Brief Summary of Project:

This project focused on system-design concepts and integration of fluid-phase chemical hydrogen ($H_2$) storage. The research addressed barriers of efficiency; gravimetric capacity; volumetric capacity; durability/operability; $H_2$ discharging rates, including start time to full flow and transient response; $H_2$ purity; and environmental, health, and safety barriers. Progress was monitored on chemical $H_2$ storage technology for necessary features to be advanced and to ensure needed communication across groups and areas.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- This project involves both chemistry and system engineering that is critical for demonstrating a chemical $H_2$ storage material solution for vehicular applications.
- Down-selection of $H_2$ storage materials has been done for tank fabrication. Two candidates were selected, but solid-state chemical $H_2$ storage materials and reversible metal hydrides were eliminated. It must be a significantly difficult task, but the team has done a wonderful job.
- This project is a part of the DOE Hydrogen Storage Engineering Center of Excellence (HSECoE, the Center), and thus it is very relevant to the goals and objectives of the DOE Hydrogen Storage sub-program. This project plays a major role in the chemical storage system work within the Center.
- The Los Alamos National Laboratory (LANL) has completed three years on this project as a partner in the HSECoE. The primary objective of the LANL work is to address critical materials and engineering issues in the development of chemical $H_2$ storage systems that can meet all of the DOE targets for fuel-cell-powered passenger vehicles. The specific roles that LANL covered over the past year include serving as the system architect and lead designer for fluid-phase chemical $H_2$ storage systems, assessing for $H_2$ release and degradation of ammonia borane (AB)-ionic liquids (IL) mixtures, designing subscale reactors, and developing purification components to increase $H_2$ purity as delivered to the fuel cell.
- Overall, this project seems well aligned with the DOE objectives because it addresses many of the key barriers. The work on AB slurries is comprehensive, addressing a number of key issues from capacity, durability, rates ($H_2$ release and viscosity), purity (through scrubbers), and safety. The only criticism is that this project is somewhat narrowly focused, because it only involves AB slurries. This is really more a criticism of the HSECoE, not this specific project.

## Question 2: Approach to performing the work

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

- There must be some variation in the target, but the team has down-selected the materials exactly according to the DOE targets.

- The approach used in this project included a down-selection process resulting in the discontinuation of several research areas to allow focus on reaction characteristics, chemical compatibilities, and subscale reactor design.
- Building on its expertise as a co-leader of the Chemical Hydrogen Storage Center of Excellence, LANL has led the work within the HSECoE to develop AB storage systems. After extensive assessments and review, liquid-based solution or slurry is being continued by the HSECoE. LANL actively participated via internal modeling and contributing to analyses by other HSECoE partners. Good attention was paid toward developing systems that could meet DOE targets.
- It would have been clearer to present the reaction characteristics results in a table showing quantitative results in the different solvents. The rationale behind the solvents tested was not presented. There was no way of knowing how many different solvents were tested and if any trends were noted or established. Solvent effects have a huge impact on chemistry, and a discussion would be preferred. It also would be good to see the measured weight percent $H_2$ from each run, along with the impurities. It is incongruous to say “no physical degradation of bladder material when exposed to various AB compositions.” Also, the presenter should be specific instead of using useless terms such as “various AB compositions” and then later say that the project team should quantify chemical and physical changes to bladder material. There should be a more technically sound method for determining slurry stability than letting a vial sit for two months and eyeballing the amount of settling. It is unclear how the reactor design shown on slide 16 addresses issues 1–3, and whether it was necessary to do a failure modes and effects analysis (FMEA) to know that reactor fouling is a potential failure mode. It is also unclear why tetraglyme is seen as a solvent in the reactor design but not mentioned in the slides on reaction characteristics. It would be good to know which adsorbents were tested for borazine and why, and whether it was demonstrated that the adsorbed material was not pyrophoric or dangerous when exposed to air and moisture, as will need to be done on a commercial scale if these systems go forward. It is not enough to prevent it from killing the stack; it has to be rendered non-toxic and safe to the consumer in the event of exposure. The literature shows that silica and alumina will adsorb borazine. It would be good to know how these materials compare with zeolites and the others mentioned on slide 21, and whether they were tested. It is unclear what the evidence is that chemical modification of an adsorbent will help meet the borazine scrubber HSECoE mass targets. Overall, the presentation only did a fair job of showing technical data supporting the claims in the summary and on what basis the down-selections, discontinuations, and go/no-go decisions were made.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The down-selected adsorber and liquid-state chemical $H_2$ storage materials have some difficulties to fabricate an onboard storage tank. The adsorber requires a liquid nitrogen temperature, and the liquid-state chemical $H_2$ storage material (AB) requires a process for the regeneration of spent fuel.
- Progress has been significant during this period. Programmatic “Specific, Measurable, Achievable, Relevant, and Timely” (SMART) milestones have been satisfied, and contributions to the total Center team effort have met expectations.
- It is unclear if everyone knows what SMART milestones are. It would be nice to show a table quantifying progress toward milestones similar to what is seen in other presentations.
- A number of accomplishments were made this past year, including the determination of mass loadings and viscosities, the characterization of gaseous-side products, and AB-slurry and slurry-tank chemical compatibilities. The project has overcome one critical barrier by identifying a slurry liquid that remains liquid after dehydrogenation. The progress seems to be well focused and aligned with the overall objectives of the project.
- LANL has conducted extensive experimental screening tests on AB-IL candidates that meet or nearly meet the 2017 DOE storage targets. LANL assessed numerous materials and has discontinued or made no-go decisions on the marginal ones. LANL also continued efforts to identify reaction conditions and compositions that could reduce the formation of ammonia and the very detrimental boron impurities. The laboratory identified reasonable solutions and research pathways to address degradation and performance issues well within the spirit and scope of the HSECoE Phase II work plan. The system architect successfully led an FMEA and developed plans to alter formation of detrimental products during the exothermic decomposition reactions of the AB-IL systems. Limited attention was paid to the endothermic chemical $H_2$ storage materials, such as alane.

### Question 4: Collaboration and coordination with other institutions

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

- This project features extensive collaborations with other Center members and, in addition, other external groups.
- Hopefully it was just an oversight that the presenter did not do an adequate job showing what was done at LANL and what was done by the collaborators.
- This project has a number of internal and external collaborators, and the work seems to be well coordinated with other efforts.
- Collaboration with other national laboratories, universities, and industries is significantly important for down-selection as well as the future work. To start collaboration with an expert of social modeling is also suitable for the DOE Fuel Cell Technologies Program.
- LANL has worked very well with various HSECoE partners and other organizations, leading to advances in predicting, down-selecting, and improving performance of chemical $H_2$ storage media. The tasks appear to have been well coordinated and of great mutual benefit.

### Question 5: Proposed future work

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

- The principal investigator (PI) would benefit from some mentoring so the plans are approached in a technically sound, focused manner, and/or so the PI learns how to present the results more clearly.
- Proposed future activities are a logical continuation of the current tasks that have been vetted by Center members and management.
- Because slurry contains liquid, both volume and weight $H_2$ densities are unfavorable for onboard $H_2$ storage. In addition, diffusion of $H_2$ in the liquid is significantly slow. This must be considered in the future work.
- The plans build on progress and continue to address key issues. It is nice to see that reactor fouling and impurities will be investigated for both alane and AB slurries. Purification will likely be critical for AB slurries—significant improvements are necessary.
- LANL has given a comprehensive plan to perform validation testing of conceptual reactor designs and several other important components such as gas/liquid phase separators and $H_2$ purifiers for the liquid-based AB media. It is good that much attention is being focused on understanding the formation of harmful boron impurities during storage and decomposition reactions. There are no plans for considering other kinds of chemical $H_2$ storage materials such as alane slurries, though. It would be an oversight for the Center not to make any effort.

### Project strengths:

- This is a very important aspect of the HSECoE.
- This project made a number of nice accomplishments this year. The project is well integrated with the HSECoE and is showing good progress.
- The team has down-selected materials for the next step. It is significantly important but must be very difficult.
- One strength is the use of an FMEA to identify reactor fouling as a potential failure mode.
- LANL has brought very capable technical personnel into the HSECoE team who provided sound theoretical modeling and materials characterization of chemical $H_2$ storage materials, especially AB. A good balance was made between modeling and experimental assessments. Very comprehensive assessments were conducted for liquid AB materials/reactors during this Phase II stage of the HSECoE project. The knowledge and experience of the former Chemical Storage Center of Excellence was an excellent capability in all of the HSECoE tasks.

### Project weaknesses:

- Because slurry contains liquid, both volume and weight $H_2$ densities are unfavorable for onboard $H_2$ storage. In addition, diffusion of $H_2$ in the liquid is significantly slow. At present, these issues are not considered.
- There are no issues with the breadth of effort and innovations for the several components for chemical $H_2$ storage systems. However, nearly all of this effort has been on AB, with virtually no attention being given so far to other exothermic or endothermic (e.g., alane) $H_2$ storage materials.

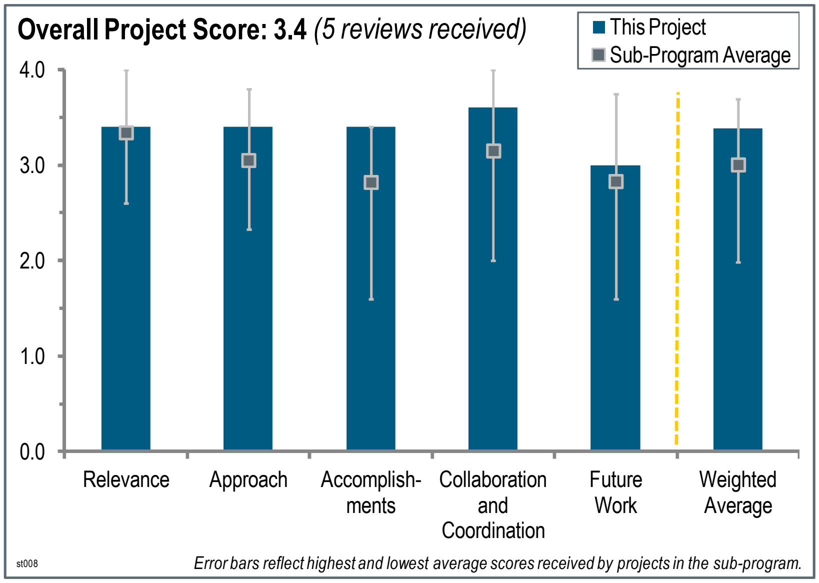
- It seems somewhat premature to spend so much time and energy designing tanks with this level of sophistication. Many assumptions are being made that lead the engineers down a path that in all likelihood is not ideal or universal. This type of project should only be done with strong feedback from the materials research community (which was the plan before the Materials Centers of Excellence were cut). It is very hard to imagine that optimal tank design for AB is similar to that for alane. The other important issue that was not discussed is regeneration. Obviously, the regeneration work lies outside the scope of this project, but it seems hard to justify so much attention on AB without first solving this key problem.

### Recommendations for additions/deletions to project scope:

- There are no recommended deletions. To add minor research on other types of $H_2$ storage materials is recommended to avoid some risks in the future.
- LANL should proceed with the verification testing activities as described in its future plans. Continuing efforts should be made to devise and demonstrate more efficient and regenerable $H_2$ purifiers with an emphasis on the diborane and borazine species. However, it would be best to develop AB materials that do not form these impurities during aging or $H_2$ release. Finally, it would be good to see a single slide given in the main DOE Hydrogen and Fuel Cells Program Annual Merit Review presentation that is a direct comparison of the attributes and limitations of the "best/most promising" exothermic AB-IL materials with the endothermic $AlH_3$ slurry prepared by the chemical systems architect, rather than several slides buried in the reviewers-only section.
- Purification remains an important issue for AB—borazine scrubbers need to be improved by a factor of 2–4. Ammonia scrubbers were not mentioned much, but they will probably need to be improved as well. At this early stage in the development of chemical $H_2$ storage materials, it will probably be useful to continue to explore a variety of tank configurations for a few different materials: $LiAlH_4$, alane, and AB. Each has its unique advantages and disadvantages and will therefore have different optimal tank designs. One could design a single tank that works for all three, but it will likely be suboptimal in all cases.

# Project # ST-008: System Design, Analysis, Modeling, and Media Engineering Properties for Hydrogen Energy Storage

Matthew Thornton; National Renewable Energy Laboratory




## Brief Summary of Project:

Vehicle performance objectives for this project are to: (1) develop and apply a model for evaluating hydrogen ($H_2$) storage requirements, performance, and cost trade-offs at the vehicle system level; and (2) provide high-level evaluation (on a common basis) of the performance of materials-based systems. The energy analysis objective for this project is to perform $H_2$ storage system energy analysis to evaluate well-to-power-plant efficiency, energy requirements, $H_2$ cost, and greenhouse gases emissions.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- This work provides good information for the Hydrogen Storage Engineering Center of Excellence (HSECoE, the Center).
- Comparisons of the various $H_2$ storage systems in relation to overall vehicle considerations are very important.
- Vehicle performance modeling and energy analysis efforts serve as important links between storage requirements and higher-level systems and objectives. Characterization of media properties provides a clear common basis for subsequent work by collaborators.
- This project provides a pivotal performance assessment function that allows the HSECoE to evaluate progress toward meeting the overarching goals of the DOE Hydrogen and Fuel Cells Program (the Program). All aspects of the project align with DOE research, development, and demonstration objectives. This work is absolutely essential to the task of validating the progress made by the HSECoE.
- The National Renewable Energy Laboratory (NREL) project focuses on the impact of storage/delivery system design, including media engineering properties, on vehicle performance. Although there seems to be considerable overlap with other efforts in the HSECoE (especially the vehicle and onboard storage parameter modeling and storage system and manufacturing cost projection work in ST-010), this project provides the HSECoE with tools to evaluate storage system designs on a common vehicle platform. In that sense, it supports the overall goals of the HSECoE.

## Question 2: Approach to performing the work

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

- The vehicle model approach looks excellent. Basically, the energy analysis employs the Hydrogen Analysis (H2A) configuration. It is unclear if there is anything better than H2A, or if H2A is good enough for what is needed.
- The vehicle modeling approach is good—it is incorporating different storage systems readily, and validation efforts have been performed. The approaches for the remainder of the efforts appear adequate.
- The approach involves the development and application of a vehicle-level performance model that allows the HSECoE to evaluate alternative $H_2$ storage materials/concepts in a realistic, self-consistent manner using a

common platform with uniform assumptions. The scope of the modeling framework is impressive, as is the way the analyses link up with other relevant vehicle performance models (i.e., Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation [GREET] model; H2A model; and others). In addition to the trade-off studies and energy analyses that come from application of the model, the project also addresses selected engineering issues that are relevant to adsorbent-type $H_2$ storage media.
- The scope of the overall approach is too broad to provide information that will effectively guide the HSECoE effort. Very high-level system modeling is used to formulate and evaluate $H_2$ storage requirements and to analyze well-to-wheels energy performance. In addition, there is a seemingly unrelated task that focuses on "media engineering properties." By extending the project across such a large analysis space, the investigators risk an overall dilution of effort and a loss of focus that does not provide much benefit to the Center. There is overlap between the system modeling approach used in this project and the approach adopted in ST-010. Likewise, there are numerous material assessment efforts being conducted in the Center that overlap strongly with the approach used here. It would have been helpful if the work on this project had been distinguished from the other efforts.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has vehicle modeling analysis for compressed $H_2$ tanks and some results for the metal hydride, chemical $H_2$ storage materials, and adsorbent materials systems, as well as some activated carbon results.
- The accomplishments in this project for the past year are excellent. The model development is as comprehensive as it can be after taking into account the present state of $H_2$ storage system development and operational experience within the HSECoE. Slides 15 through 20 of the presentation display types of numerical information that give highly useful insights and guidance about where $H_2$ storage system development stands and what needs to be addressed in future work. The results say that a 200+ mile range passenger vehicle is within reach.
- The development of the vehicle model Hydrogen Storage Simulator (HSSIM) that can be used to evaluate storage systems on a common vehicle platform has provided useful and important new information. In addition to the existing slides that present the results of the HSSIM modeling, a slide that summarizes the conclusions and provides recommendations for future Center work would have been useful. The results on $H_2$ delivery from an alane-based subsystem are also interesting and potentially useful for guiding further work in the Center.
- The fixed volume vehicular modeling is an improvement over the constant available $H_2$ approach. Some vehicular performance measures have not only targets, but also minimum thresholds; the prime example of this is vehicle range. Below a certain level, the system would not be considered regardless of performance or cost. Storage system cost should be presented. A better description of accomplishments in the energy modeling area is needed. More emphasis on key takeaways in the material characterization accomplishments is needed. It is not clear how the accomplishments presented are important or where they could lead.

### Question 4: Collaboration and coordination with other institutions

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

- This project features excellent collaborations with other organizations.
- The project has important collaborators from industry and academia, with important contributions being made by the collaborators.
- The collaboration structure is indeed outstanding. The connections with HSECoE partners and with other institutions that provide companion analytical models and supporting data are seamless.
- Slide 23 does a good job showing the expertise that each partner brings to the project. What is not clear is how they manage the collaboration; for example, it is unclear what sort of team meetings are held and how communication is managed, among other issues. So while the team probably deserves a 4 on this, the score has to be downgraded to a 3 because of the uncertainty.
- Collaborations with multiple HSECoE partners are evident, and those interactions strongly support the technical effort in this project. It would have been helpful if the specific contributions from the individual partners had been described. That would have allowed the role played by NREL to be clearly identified and areas of overlap within the HSECoE to be clarified.

### Question 5: Proposed future work

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

- The future work is relatively straightforward and described adequately.
- The future work plans basically continue what the project team has been doing.
- The proposed future work is spot-on in every respect. Next year's DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) is worth looking forward to. The simulation results should provide a bevy of seminal new insights about where things stand for fuel-cell-powered cars.
- The future work is stated in such a general way that it is difficult to fully assess what will actually be accomplished. A straightforward and clearly stated set of milestones would be helpful. Likewise, a brief presentation of technical barriers and plans to overcome them would greatly aid the ability to assess the future work plans.

### Project strengths:

- One strength is the analyses of $H_2$ storage system impacts on the global vehicular system.
- NREL has a solid vehicle performance modeling capability. The development of the HSSIM is a strong aspect of the project.
- Vehicle modeling aspects provide an important bridge between storage system characteristics and overall requirements and target evaluation.
- This project is an invaluable asset to the HSECoE and to the Program, and it is being done with exceptional thoughtfulness and skill. There is seamless integration of this project with the other contributing projects within the HSECoE. The presentation of this project at the 2012 AMR was very well done.

### Project weaknesses:

- The modeling approaches use what has been developed in the past. There is nothing new.
- More detail regarding the activities in the material characterization and the use of the results by others is needed.
- An average of the 2011 and 2012 budgets for this project (which should tell approximately how much funding was applied to the project since the 2011 AMR) is $215,000. That is not much at all for a project focused on what this one is trying to accomplish. In addition, NREL also performs some experimental activities within the HSECoE, presumably out of this same budget.
- The (limited) materials assessment work seems disconnected from the modeling/analysis work on the project. The overlap between the vehicle modeling and materials evaluation work in this project and others in the Center needs to be clarified.

### Recommendations for additions/deletions to project scope:

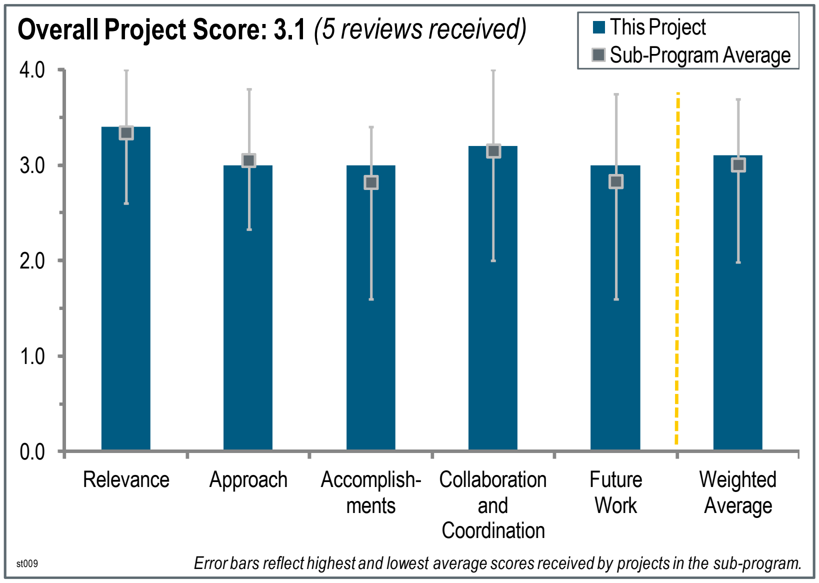
- It is unclear what fraction of the funding for this project goes into the material engineering aspect. Perhaps the NREL effort should be fully focused on modeling and simulation. This is not to say that the materials work is unimportant; it is more to emphasize how important the analysis work is. In the future, the project team should please define all acronyms on first use. There are many acronyms in the slides that were not defined. This was actually the case for most of the HSECoE presentations.
- A thoughtful discussion should occur between the principal investigator and the coordinators/director of the HSECoE to ensure that unnecessary duplication and overlap of effort with other projects in both the modeling and materials evaluation areas is minimized. The materials effort is largely distinct and disconnected from the modeling/analysis work. In addition, the materials effort lacks the kind of rigor and scope that is needed to be beneficial to the Center. A detailed review of the materials effort on this project should be conducted in the HSECoE, and mid-course corrections should be made as appropriate.
- This reviewer had no recommendations.

# Project # ST-009: Thermal Management of On-Board Cryogenic Hydrogen Storage Systems

Darsh Kumar; General Motors

## Brief Summary of Project:

The objective of this project is to address the barriers of system weight and volume, energy efficiency, charging and discharging rates, and thermal management of hydrogen ($H_2$) storage systems by studying: (1) discharge thermal management for adsorbent systems; and (2) how differently sized and shaped cylindrical pellets affect $H_2$ adsorption. The project also includes the measurement of engineering properties and the design and fabrication of a three-liter cryogenic adsorption test vessel.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- This project focuses on modeling thermal management in adsorbents to gain insight into the balance of thermal diffusion and $H_2$ diffusion.
- The projects addresses tank heat management, which is necessary to help determine materials feasibility.
- This project is helping to develop a usable cryo-adsorbent $H_2$ system—one of the few options for storing $H_2$ onboard a light-duty vehicle.
- This project is in the Hydrogen Storage Engineering Center of Excellence (HSECoE); therefore, it is relevant to the basic goals and objectives of the Hydrogen Storage sub-program. In particular, this project is the lead for thermal management of cryogenic storage systems, which is a critical component of the total absorbent storage system.
- This project addresses a critical aspect of the $H_2$ storage system. The entire work scope fully supports DOE's research, development, and demonstration objectives and is essential to achieving the goals of the HSECoE. Heat transfer and thermal management will have to be optimized to the fullest extent possible because they profoundly influence time scales and efficiencies and can have an impact on just about all of the barriers being addressed by the HSECoE.

## Question 2: Approach to performing the work

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

- The modeling approach is reasonable and allows for presenting solutions to tackle heat management issues.
- This is a good plan and approach for achieving the goals and objectives of this project. The focus is on system simulation models and materials properties to support system design.
- The approach involves using system simulation modeling to aid in the design of heaters for adsorption systems for two scenarios and two cases. There was some mention of using failure mode and effects analysis (FMEA), but the connections between the analysis and the modeling were unclear.
- The overall approach is well orchestrated and concisely focused on the key thermal management issues that have been identified within the HSECoE for adsorption-type $H_2$ storage materials. The integration of transport models with simulation results and with results from direct experimentation is done in a well-considered manner.

- The approach of identifying material properties and then using them in a model simulation, followed by experimental validation, is the right sequence, but time-wise it seems weighted too far on the material properties and modeling efforts and not on physical testing. If the testing does not simply validate the model, and the data shows results different from the model, then the approach of only validating the model becomes an iterative process.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- One accomplishment was using modeling to compare hot gas recirculation to electrical heating with coils. Modeling gave insight into optimizing heat transfer and mass transfer in sorbent pellets.
- Good progress has been made toward achieving system design and definition goals. Design parameters have been identified that allow optimization of system performance. A test vessel has been designed and built, including instrumentation required to evaluate system performance.
- Significant progress has been made toward elucidating the factors that most heavily influence thermal management in adsorption beds and toward providing solutions to some of the apparent barriers. Examples include the helical coil heater approach, the influence of pellet size and shape, and the consequences with respect to refueling time. A cursory look at the "Technical Back-up Slides" showed that there may be some serious issues for metal-organic framework-5 (MOF-5) with respect to pellet durability and thermal conductivity.
- The helical coil heater approach for the cryo-tank is interesting; however, it ultimately concludes that an optimized heater is necessary to determine tank $H_2$ volume and mass density loss, and hence establish feasibility.
- In general, the accomplishments so far represent important progress toward DOE's goals, if physical feasibility is proven experimentally. One concern is related to the potential thermal modeling of the pellet and how meaningful it will be in predicting the thermal behavior of a packed bed convolved with a heat exchanger and variability in gas flow patterns, among other things.

### Question 4: Collaboration and coordination with other institutions

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

- There are outstanding collaborations with other HSECoE partners.
- There appears to be good coordination among partners and they seem to have collaborated well, but it was not completely clear from the presentation.
- It would have been helpful to hear more details about the similarities and differences of the work being done by Savannah River National Laboratory and General Motors (GM). One assumes that the project team will need significant discussions with Oregon State University (OSU) on the Modular Absorption Tank Insert device.
- Clearly, the GM staff working on this project are appropriately connected with partners in the HSECoE that are working on closely related modeling and data development activities directed at adsorbent-type $H_2$ storage materials properties, system design issues, and FMEAs.

### Question 5: Proposed future work

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

- The plans for future work are a logical continuation of current activities. Testing of the cryo-adsorption vessel is critical to the project.
- The future plans do address critical barriers and should result in important information and designs, but at the pace of the project so far, the future plans look like they would exceed the time available. The cryo-adsorption vessel testing will be more than a simple model validation and will require time to iterate.
- For the work focused on heating to desorb the $H_2$, it will be interesting to see the results on cooling and how this couples with various heating scenarios. Also, experimental results from the three-liter system will be great to benchmark models.
- The future plans for this project logically build on progress in the past year and are sharply focused on critical $H_2$ storage system performance barriers. Based on the results presented by this project, one gets the feeling that as

the studies of thermal management and its effect on component efficiencies progress, the task of meeting DOE $H_2$ storage system targets could become even more daunting than currently perceived.
- It is recommended that GM consider the cost and challenges of pellet manufacturing as designs are being proposed, and that the team optimize tank design with modeling prior to test-bed-based testing. For example, tank heat distribution and its effect on sorbents is a difficult issue that needs to be examined closely.

### Project strengths:

- This project features a strong and well-qualified team. Extensive collaborations have supported and benefitted the project's accomplishments.
- The strengths of this project include the team's strong capabilities in modeling and knowledge of system requirements.
- The approach to thermal management issues within the HSECoE seems to be well organized. The design activities, modeling efforts, and experimentation appear to be well integrated and focused. GM is certainly holding up its end in this task.
- Despite the other concerns in this review, this project has a good approach and elements of the project have achieved important results.

### Project weaknesses:

- The collaboration with other HSECoE members working on heat management—for example, OSU—is not visible.
- The potential use of liquid nitrogen for the adsorption heat exchangers should be avoided, if at all possible. Methods for more completely discharging $H_2$ should be explored. Leaving behind approximately 6% of the stored $H_2$ is a serious inefficiency.
- This particular project does not exhibit any tangible weaknesses. The thermal management picture for MOF-5 may show some serious deficiencies with respect to target-level performance.
- One weakness is the absence of experimental validation of the modeling effort. There is a good chance that when the three-liter vessel is tested with a heat exchanger and a pellet bed reflecting the analytical model designs, there will be differences from the model. This would suggest the need for at least an iteration on the model and a new physical version of the three-liter vessel or heat exchanger and the pellet stack or pellet configuration. It is unlikely there is enough time for that.

### Recommendations for additions/deletions to project scope:

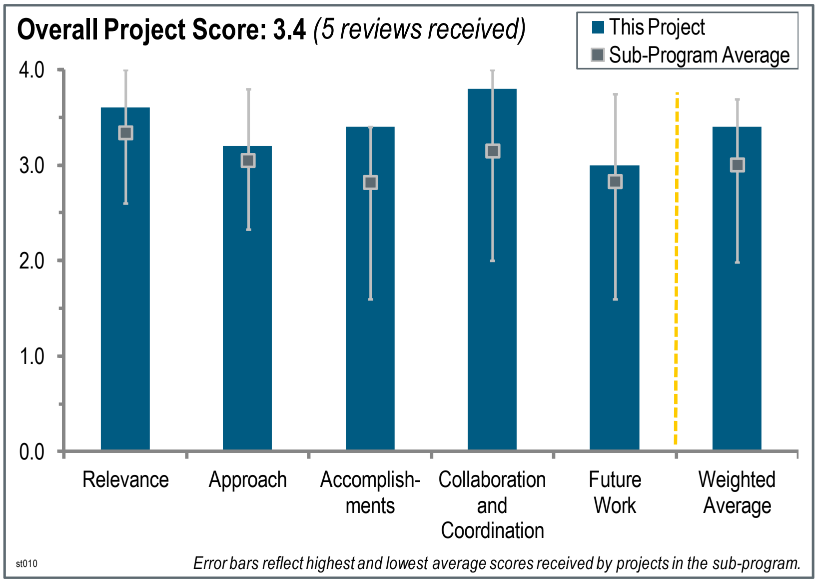
- The scope, overall approach, and plans for the coming year are well thought out. There is no need for change.
- It is recommended to focus on optimization of the proposed heater design using modeling prior to test bed measurement. Another recommendation is to have closer collaboration with other HSECoE team members working on heat management, such as OSU.

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

Mike Veenstra; Ford Motor Company

**Brief Summary of Project:**

The objectives of this project are to: (1) develop a dynamic vehicle parameter model that interfaces with diverse storage system concepts, (2) develop robust cost projections for storage system concepts, and (3) devise and develop system-focused strategies for processing and packing framework-based sorbent hydrogen ($H_2$) storage media. The models developed for tasks 1 and 2 support the determination of overall vehicle cost and performance as well as enable storage concepts to be exercised at a real-world level. Task 3 supports the creation and validation of sorbent bed models and aids in trade-off analyses.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- Sorbents may be the most promising materials-based $H_2$ storage strategy. These efforts will clarify issues around the use of sorbents.
- This project is a part of the Hydrogen Storage Engineering Center of Excellence (HSECoE, the Center) and, as such, it is relevant to the goals and objectives of the Hydrogen Storage sub-program. Ford Motor Company is a technology lead in several important technical and organizational areas, including their role as sorbent system architect.
- The project plays a number of key roles in the HSECoE, and thereby strongly supports the DOE Hydrogen and Fuel Cells Program's (the Program's) objectives. Storage targets are implicitly considered.
- This project provides a vital connection between the HSECoE, DOE, and a large automobile manufacturer. Vehicle parameter modeling, manufacturing cost modeling, failure mode and effects analysis (FMEA), and adsorbent system architecture coordination activities serve as useful adjuncts to the main technical task on characterization and optimization of framework-based $H_2$ storage media. This project is an important element in the overall HSECoE engineering activity and is closely aligned with the objectives of the Program.
- Ford's role in the HSECoE is to develop a dynamic vehicle model that interfaces with different storage concepts, develop cost projections for these concepts, and devise strategies for packing framework-based sorbents into system containers. These roles are very relevant to the HSECoE effort and to the DOE goal of developing materials-based storage systems. It remains to be seen if metal-organic framework-5 (MOF-5) can approach the DOE targets close enough to reach a "go" decision for Phase III.

## Question 2: Approach to performing the work

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

- The adopted approach involves several crucial activities such as identifying performance gaps, developing processing-structure-property relationships, and material characterization.
- Ford has identified critical issues around sorbents and is systematically addressing them. Data is being supplied to modelers as needed.

- The approach to developing processing guidelines and structure-property relationships is well organized and systematic. Thermo-physical data and isotherm data is being developed. Compaction issues and thermal conductivity relationships are being investigated to provide information for the go/no-go decision. Volumetric capacity and thermal conductivity, which have been identified as critical issues, are the focus of the efforts on adsorbents. The integrated system model approach is vital for a complete evaluation against the DOE targets.
- The approach consists of several discrete efforts; each is led by an individual and dovetails into the needs of the HSECoE. There is a good industrial component to the work, which is obviously needed if the overall concept of onboard solid-state storage is to succeed. The effort focuses largely on the MOFs, which are important to the cryo-adsorbent focus of the HSECoE. The FMEA will also be very valuable to the overall HSECoE effort.
- A systematic and well-reasoned approach has been adopted in all three tasks of the project. MOF-5 was selected as a prototype system for framework material characterization and optimization, and a solid experimental approach was used to characterize the engineering properties of the material under different process conditions. However, it is not entirely clear whether the results obtained from the extensive experimental and modeling studies conducted on the MOF-5 system will translate into the predicted performance of improved framework materials that may emerge in the future. There appears to be overlap and duplication between the modeling and cost analysis efforts of this project and the National Renewable Energy Laboratory project (ST-008). It would have been helpful if the technical efforts had been compared and contrasted in the review presentation.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Ford has made good progress with the MOF-5 material characterization, which is vital to system design and performance modeling. The system design FMEA has identified important potential problem areas during the design process.
- The permeation data is a critical addition to this work and shows promising results. Failure analysis is a very significant addition to the project and has the potential to guide and refine project direction and resource allocation.
- The accomplishments for 2011 are excellent. An engineering property database for MOF-5 was completed, as well as a partial database for activated carbon that is limited by densification challenges. Ford also took on the responsibility of system architect for adsorbent systems in 2011. Ford developed "Specific, Measurable, Achievable, Relevant, and Timely" (SMART) milestones and a Gantt chart to coordinate the work on adsorbent systems across the Center. The architect identified and prioritized research gaps. Ford also verified the integrated system model results for a complete system evaluation against DOE performance targets.
- Good progress has been made in all tasks of the project. The experimental, modeling, and cost analysis results are being transferred effectively to other partners in the project. The expansion of the project to include the coordination of the system architecture work is an important and useful addition. However, it is unclear how that overlaps with the system architecture work in the Jet Propulsion Laboratory project. One concern is that multiple modeling and experimental efforts are being conducted on the MOF-5 system within the HSECoE. This has led to some confusion for the reviewers about the roles and responsibilities of each individual group in the total effort. For purposes of this review, it would have been helpful if the technical work on MOF-5 materials in the Ford Motor Company/BASF-SE/University of Michigan (UM) project could have been put in the context of all of the other related efforts of the Center.
- The project has been active and has produced numerous results. However, the slides were rather compressed, so the individual efforts have probably generated more results than can be shown in this short presentation. The preliminary cost analyses are very useful. The shortcoming of this presentation is the general failure to clearly state how the results have influenced the HSECoE and relate to achieving the DOE targets. For example, it is unclear whether distinct problems have been uncovered that might affect the upcoming go/no-go decision for Phase III.

### Question 4: Collaboration and coordination with other institutions

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

- The collaboration with complementary research groups is excellent. The collaborators are well qualified and provide valuable contributions to this effort.
- Generally, the collaborations seem to be within the three project partners and interaction with the HSECoE. A few more collaborations might help.
- There are extensive interactions and collaborations with the HSECoE team. Many of the partners in this project made major contributions to the accomplishments and success of the project. In addition, BASF and UM were subcontractors to Ford on this project and supported the progress and accomplishments.
- The close collaboration and synergy among numerous partners in the project are evident. All partners are making significant contributions to the technical effort. Regular exchange of technical and program information is occurring, and good coordination with the overall HSECoE effort is apparent.
- The Center's approach of experimentalists and modelers working closely together is good.

### Question 5: Proposed future work

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

- The future plans have Ford continuing the important characterizations of the MOF-5 adsorbent and the associated storage system and vehicle modeling. Important in-situ neutron imaging will also be done for model validation.
- Neutron sorption measurements should be used to validate diffusion/sorption models.
- Future work plans aim to fill in the technical gaps to inform the go/no-go decision process for Phase III. They are quite ambitious, but the budget has been increased significantly for 2012. Cost projections are intended but cannot be evaluated against the DOE cost target until the revised target has been established.
- The proposed future work continues the work started without much change in direction, if any. One would expect that some of the findings to date might suggest at least limited changes in directions.
- The future work is clearly stated and focused on the important technical barriers in the MOF-5 adsorbent system and on the continuation of the storage parameter and manufacturing cost modeling work initiated earlier in the project. Although the engineering evaluation of the MOF-5 system is important and fully consistent with the HSECoE goals, it remains unclear whether the results of that work will provide a useful predictive capability that can be applied to improved adsorbent systems that may emerge in future materials work.

### Project strengths:

- This project has a strong, committed team with extensive experience and capabilities.
- A strong development and engineering team is conducting the technical work on this project. Excellent progress is being made on all tasks and a solid plan for future work that focuses on critical technical barriers is in place.
- The Ford team has excellent qualifications and the collaborators have significant expertise and experience to bring to the effort.
- This project features good industrial/commercial input. Looking at practical (application) problems is another area of strength.

### Project weaknesses:

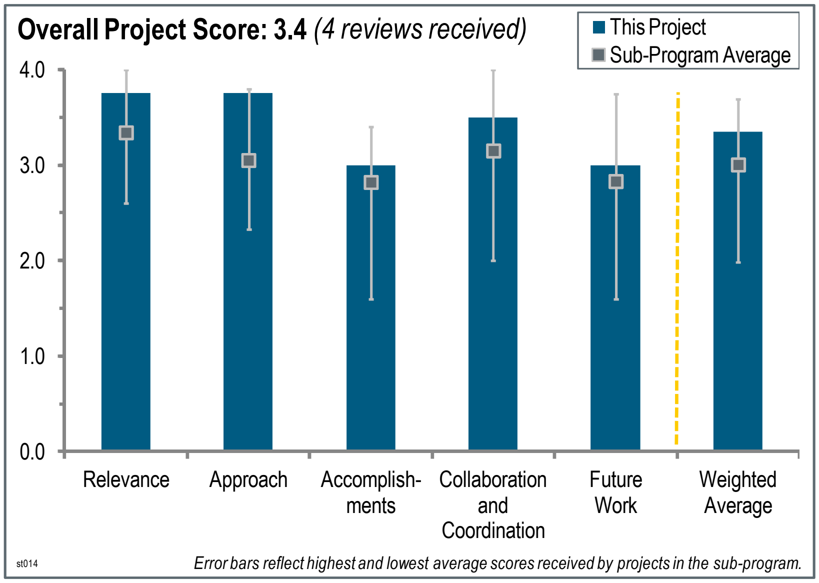
- This presentation did not clearly define the "effective" $H_2$ storage capacity (in terms of weight percent) or range of capacities, or indicate how the proposed system will meet the DOE storage targets.
- A large number of modeling and experimental efforts are being conducted on the MOF-5 system within the HSECoE. This has led to some confusion concerning the roles and responsibilities of each individual group, and it seems that some efforts are duplicated. A clear and succinct delineation of the role played by this project within the context of all the other work in the HSECoE is needed.
- This project does not seem to express practical conclusions very clearly; for example, relative to the overall practicality of using MOFs for vehicular $H_2$ storage.

### Recommendations for additions/deletions to project scope:

- The project team should focus more on the question, what the results achieved thus far mean, and whether any directions should be changed.
- It would be good to know whether the Center has calculated mechanical forces on pellets/particles due to thermal expansion/contraction of the tank on cooling and warm-up. These forces, along with structural properties of pellets, should give a good estimate of fracture behavior in use.
- Ford should be encouraged to evaluate the prospects of whether a sorbent-based system can be a viable alternative for automotive use and to develop a rationale for continuing research into sorbent materials and systems.
- MOF-5 is not likely to meet the 2017 DOE targets, and it is unlikely that material will be employed in the final engineering embodiment developed in the Center. This project must not simply focus on MOF-5 as the final solution; it should develop the flexibility to incorporate new materials as needed.

# Project # ST-014: Hydrogen Sorbent Measurement Qualification and Characterization

Phil Parilla; National Renewable Energy Laboratory




## Brief Summary of Project:

The overall objective of this project is to help ensure that capacity measurements for hydrogen ($H_2$) storage materials are based on valid and accurate results, thus allowing promising materials to be properly identified. To advance the accomplishment of this objective, this project specifically aims to: (1) assist materials research groups to characterize and qualify their samples for $H_2$ storage properties; and (2) analyze, identify, and recommend corrective actions for major sources of measurement error in volumetric systems.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The accurate measurement of all potential $H_2$ storage materials plays a critical role in the success of the DOE Hydrogen and Fuel Cells Program (the Program).
- The validation of the material properties proposed is critical to avoiding misleading results.
- This laboratory has spent the past six years refining the techniques necessary for the evaluation of materials behavior/performance in a global research environment that has demonstrated less-than-consistent results. The National Renewable Energy Laboratory (NREL) is now one of the few laboratories that can produce credible results over the wide pressure/temperature ranges of interest to the Program. These capabilities would have been of great value at the start of the DOE Hydrogen Storage sub-program's three material Centers of Excellence six years ago.
- This project represents a much-needed effort to improve gas adsorption measurements in the DOE Office of Energy Efficiency and Renewable Energy (EERE) portfolio. This should improve material identification and provide reliable isotherms for engineering systems. The dissemination of best practices is appreciated. The project provides a robust validation mechanism independent of the material synthesis laboratories.

## Question 2: Approach to performing the work

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

- This project features an excellent approach.
- The volumetric and spectroscopic techniques used are the most appropriate for determining materials performance.
- It is hard to criticize the achievements of this project. The project team is taking a logical and necessary approach to the work.
- The approach of identifying the possible error and guiding the material researchers to characterize/qualify their samples is very logical.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Several major sources of errors have been identified and resolved. The "Best Practices" recommendations will help the researchers to produce a more accurate material-level evaluation.
- It is recommended that this team also be involved in validating the measurements of compacted cryo-absorbent media.
- The efforts of the laboratory have probably been redirected several times over the course of the Program. It has unfortunately taken a long time for the NREL efforts outlined in this presentation to come to fruition. NREL's effort should be rated closer to 3.5.
- The procedural work is clearly world class and a valuable asset for DOE, and it has been developing throughout the lifetime of the Hydrogen Sorption Center of Excellence. The round-robin effort seem a little late, given that the door has almost closed on the materials development portfolios, but there are indications that there is strong agreement across laboratories, perhaps due to the earlier work. This work should clearly be published, but the best practices document may suffice. Validating adsorption properties has been useful, but clearly even this is being abused in some forms. For instance, in the talk by Northwestern University, where the NREL nitrogen isotherm data was arbitrarily scaled by a factor of approximately 1.2 and then applied to $H_2$ isotherm data, even though the shapes of the $H_2$ isotherms are not even similar between the Northwestern University and NREL measurements. This type of practice needs to be guarded against. Hopefully the information Northwestern University gained from this interaction is put to good use.

### Question 4: Collaboration and coordination with other institutions

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

- This project's worldwide collaboration is visible.
- The round-robin testing involved several partners, and the measurements are well coordinated between all the partners.
- The collaboration with Northwestern University appears to have worked particularly well in highlighting the analysis errors of that institution. It would have been beneficial for specific data or data analysis to have been presented that outlined in better detail the nature of the collaboration with certain principal investigators (PIs).
- Samples in round-robin testing and external samples measured indicate a solid collaborative foundation and demonstrate a continued need for the measurement/validation services.

### Question 5: Proposed future work

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

- The development of new measurement capabilities is necessary and critical to the Program.
- Given what appears to be the direction being taken by the Program, the proposed future work seems appropriate.
- It seems that this project acts in more of a service role, which should be available to EERE-funded projects. In that respect, the details of the future work are sketchy, but understandably so. Perhaps EERE should indicate that NREL is available to its funded projects.
- It is recommended that this project also be involved in validating the measurement of compacted cryo-absorbent media within the Hydrogen Storage Engineering Center of Excellence (HSECoE). Another recommendation would be to lead the validation of results reported within the HSECoE as opposed to just selected samples. A clear, specific plan is missing. It would be helpful to show specifically what materials and testing are planned.

### Project strengths:

- This project features capable and experienced scientists with good facilities.
- NREL now appears to be the only laboratory capable of delivering indubitable results, and it is more capable of collaborative work in a professional environment.

- Strengths of this project include the detail of the measurement techniques and the ability to identify errors. Other strengths are the HSECoE collaborative efforts and the measurement validations.
- Strengths of this project include the coordinated effort between multiple institutions and the well-organized "Best Practice" procedures.

### Project weaknesses:

- If this project loses funding, the expertise and instrumental capability at NREL will atrophy. Also, there are no clear efforts by NREL to leverage its capabilities to the new projects and to the needs of the HSECoE.
- The development of any new measurement capability will take a lot of resources in general. It will be good for the PI to prioritize the new capability list and rationalize the effort.
- There were no weaknesses.

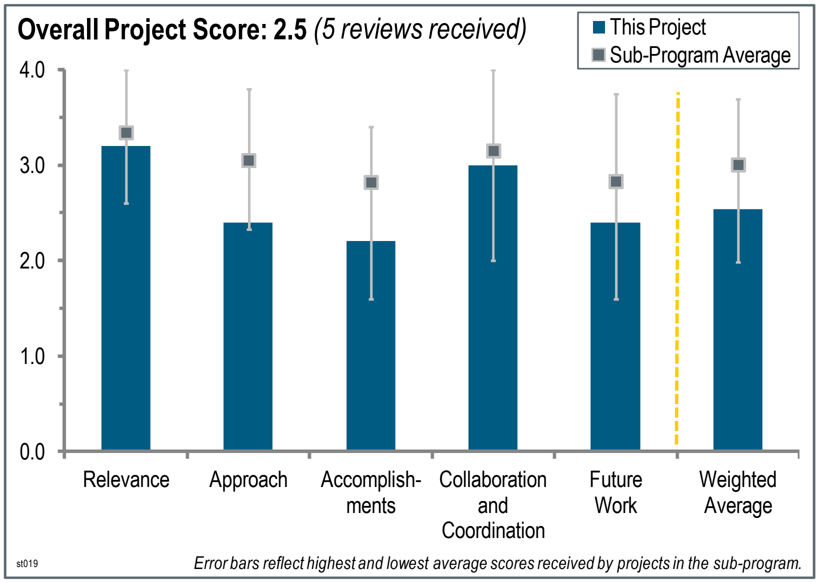
### Recommendations for additions/deletions to project scope:

- These facilities should be available to the EERE $H_2$ storage projects.
- The PI may consider determining the acceptable error bar range in this kind of measurement and make recommendations to materials research groups.
- It is recommended that NREL be involved in validating the measurement of compacted cryo-absorbent media within the HSECoE. Another recommendation would be to lead the validation of results reported within the HSECoE as opposed to just selected samples.

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

Peter Pfeifer; University of Missouri

**Brief Summary of Project:**
The objective of this project is to address the lack of understanding of hydrogen ($H_2$) physisorption and chemisorption by: (1) fabricating high-surface-area multiple-surface-functionalized nanoporous carbon (from corncob and other precursors) for reversible $H_2$ storage (physisorption) with superior storage capacity, (2) characterizing materials and demonstrating their storage performance, and (3) optimizing pore architecture and composition. These tasks also address the barriers of system weight and volume, system cost, charging and discharging rates, and thermal management.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- The project is relevant to DOE objectives, in particular reducing the raw materials cost of $H_2$ storage materials.
- The project aims to attack the key barriers and, if successful, might move the leading edge of adsorption capabilities in some areas. The ability to lower costs is a very important aspect.
- Increasing the binding energy of adsorbed $H_2$ is critical for this technology from the point of view of materials development. Optimizing the packing of adsorbent material is also important for adsorption storage systems.
- This project is looking at improving the adsorption properties of high-surface-area, porous carbons that are derived from natural products. The project team's primary objective remains to incorporate substantial concentrations of boron into the carbon microstructures in order to increase bonding energies with $H_2$, while still retaining storage capacities at ambient temperatures and moderate pressures (i.e., up to approximately 100 bar) to levels found when $H_2$ is adsorbed at cryogenic temperatures. If such storage materials can be developed with sufficiently low costs, more of the DOE storage performance targets might be achieved using adsorption storage vessels. Significant issues still remain with volumetric densities from these boron-carbon materials, even if gravimetric capacities were increased. Higher heats of reactions also negatively impact engineering these vessels for thermal management during both $H_2$ adsorption and release. The issue of reducing the costs of making and processing these materials over conventional activated carbons was not addressed in any detail.

### Question 2: Approach to performing the work

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

- While some of the work to understand the role of the dopants seems derivative, the compounds used are new and seem to be providing results. The use of tanks to test the material is excellent. The confirmation of previous speculation with spectroscopic data is good, though there is more to do here.
- The work this year was significantly focused on fundamental measurements and modeling, which may not be justified without a plan to significantly improve the base materials. Some improvements in boron doping have been achieved, but this does not appear to be the main focus of the work as it probably should be. While not

funded by the DOE Hydrogen and Fuel Cells Program (the Program), the engineering test bed does not offer much value to the improvement of materials and may be a distraction.
- The ultimate goal of the boron functionalization work is unclear. Boron substituted within a graphene-like structure seems to be the goal, but the ability to do this appears to rely on a high defect density. Given the overall reduction in surface area of materials that have been exposed to $B_{10}H_{14}$, it would be interesting to know if one can assume that the defect density is low. In addition, the goal should be to generate as many lattice defects as possible in the base material. This approach also seems to be inconsistent with the language of slide 5, where doping the surface rather than promoting the lattice substitution is the goal.
- Using extensive, in-house expertise with boranes (e.g., $B_{10}H_{14}$) to provide novel means for boron additions to the promising carbon host, the University of Missouri team has been able to incorporate boron atoms via thermal processing to minimize the loss of surface areas. A variety of characterization techniques are used to evaluate the impacts of synthesis and processing on the nature and distribution of the pores and the adsorption properties (e.g., capacities and heats of adsorption). During the early phase of the project, computer modeling of pore structures was used to predict the optimal configurations for maximum $H_2$ adsorption. The more recent focus appears to be on material preparation and characterizations, which is appropriate.
- The doping approach is thorough and methodical, and it goes deep into the understanding of the fundamentals. However, this is done at the expense of good $H_2$ storage characterization and finding solid proof of increasing binding energy. The project team has only presented a couple of near-room-temperature isotherms to deduce the isosteric heat of adsorption, which is generally high at low coverage, such as the case here. A more convincing demonstration would have been to find the isosteric heat of adsorption at larger coverage by going down to lower temperatures, as the principal investigator has done in the past. It would also be useful to learn whether the doped material is reversible or not.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.2** for its accomplishments and progress.

- Progress is relatively slow; the characterization efforts are an overkill for such little gain. There seems to be a little improvement in functionalizing the adsorbent, but the storage capacity is still very low.
- A low-temperature isotherm shows $H_2$ uptakes that are similar to most other high-surface-area activated carbons. The still relatively low isosteric heat of 10 kJ/mole will presumably still fail to satisfy the Program goals at room temperature. Regarding the information on slide 9, if a doubling of binding energy increases adsorption in the Henry's Law region, it is not clear why such a large enhancement is also seen at 200 bar.
- Using $B_{10}H_{14}$, carbon samples were doped with approximately 5–10 wt.% boron and were characterized by various methods, including assessing the adsorption behavior of $H_2$. While some enhancement of $H_2$ capacity and binding energies were noted near ambient temperature, these improvements still fall far short of the objectives and DOE targets. There did not seem to have been many samples made and characterized during the past year. The methodology used to analyze adsorption test data was not well described, and it was confusing to interpret actual performance observed by this team. It seems to be more or less a shotgun approach as related to the selection of characterization techniques and samples, without a coherent explanation of the project team's observations and the true potential and limitations of these materials.
- Many of the technical accomplishments appear to have been the prior year's work. The work on improving the derivation of the enthalpy of adsorption is very useful, but it is surprising to see that it was only applied to one sample. Developing and testing a variety of methods and parameters for improving boron loading should have been a priority, with enthalpy analysis being used as a tool to evaluate performance.
- Doping the base material without loss of surface area is a good accomplishment, though it would have been beneficial to see much of this confirmed by others. Testing in a multi-liter vessel shows not only the ease of making the material, but also provides interesting results for tank designers. Unfortunately the $H_2$ storage capacities are low, but that is important for the researchers to learn and work toward better results. The main concern is the actual mass percent improvement of the system based on this developed material, relative to a tank with no media at the same pressure and temperature, may be negative or very small. This is an issue all storage projects need to face. The University of Missouri has taken on this issue, so the team should be applauded for facing the problem, but it needs to test for significant $H_2$ mass increase due to adsorbent at room temperature. It was good to see the spectroscopic data supporting previous results.

### Question 4: Collaboration and coordination with other institutions

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

- The group has both national and international collaborations on sample characterization.
- The project features a good set of international collaborations.
- The project has many collaborators, but their contributions are unclear. It is likely that many are just discussions at conferences.
- There was good cooperation with the various University of Missouri departments on producing and characterizing materials. It was less obvious how the theoretical modeling results contributed to the overall project, especially on approaches to dope with boron. Several outside collaborations to obtain specialized characterizations were evident. Hopefully this will be continued, with the addition of other insightful methods such as Raman and solid-state nuclear magnetic resonance of the boron-doped carbons as made and in the presence of adsorbed $H_2$.

### Question 5: Proposed future work

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

- The proposed future work is not very ambitious, but it is acceptable.
- The most important result expected from this work is to clearly demonstrate the advantages and disadvantages of boron-doped materials. The pelletizing work seems to be complete.
- Given the results presented here, the heats of adsorption and the surface areas still fall short of the Program goals for $H_2$ uptake at temperatures of 80 K or 303 K. A number of comments from last year's program review could apply to this year's work. In spite of some apparent improvement in $H_2$ uptake, these materials seem to have an upper limit of $H_2$ uptake that still falls short of the Program goals.
- All of the proposed tasks on slide 23 are very reasonable and highly desirable to provide much-needed insight and validation on whether boron-doping can substantially improve the adsorption properties of microporous carbons. However, the prognosis of these materials reaching performance levels needed to achieve DOE targets is poor.
- The focus on new boron-doping techniques is the most promising of the proposed tasks. These materials will not meet DOE goals without large improvements in the base materials. Given the limited resources, that work should not be diluted by fundamental studies or scale-up (monolith) studies without some significant improvement in material properties.

### Project strengths:

- The University of Missouri has personnel with great knowledge and expertise with boron chemistry, as well as equipment and experience working on the adsorption properties of porous carbons and other materials.
- Strengths of this project include the low cost of the base stock material and the ability to make and test large batches.
- This project has a methodical and thorough approach and gives a more complete picture of the advantages and disadvantages of boron doping.
- A significant increase in the $H_2$ adsorption energy with boron doping has been made. The boron-doping process has been improved and doping levels have been well characterized. This is critical for room-temperature physisorption of reasonable amounts of $H_2$.

### Project weaknesses:

- The material developed by this project is far from being practical at room temperature.
- While boron-doping has been improved, not a lot of progress appears to have been made on developing host materials or increasing the capacity to meet DOE goals since the prior year.
- There has been considerable disorder in the approaches used to select and investigate the carbon-boron candidates. Synthesis methods and characterization techniques seem to have been selected mostly for availability at the university or research group, with little discrimination on whether they are the most appropriate to address

the stated objectives and needs. In addition, nonconventional assessments of thermodynamics are given with minimal explanation or justification of their applicability.
- It is not clear whether meaningful added storage is achieved even with the best materials. There is some additional storage, but one must consider only the excess amount of $H_2$ stored compared to what is in the gas phase. The net $H_2$ storage with these materials relative to a system with no adsorbent at all is likely to be low.

### Recommendations for additions/deletions to project scope:

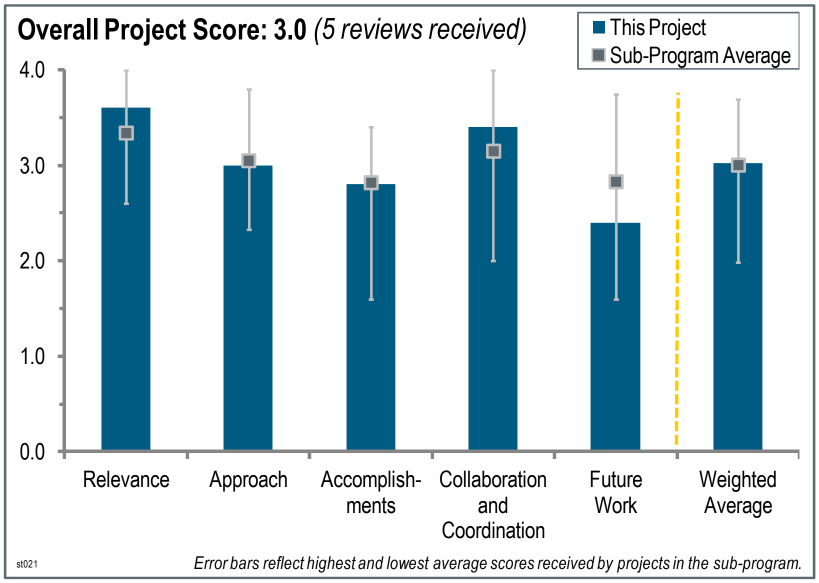
- The researchers on this team should vigorously perform the tasks outlined on their Future Plans slide 23. Their goal should be to validate their claims of better performance from boron-doped carbons and provide experimental insights into changes in structure and chemical bonding within the host sorbent and the nature of the $H_2$ adsorption process. In addition, thorough characterization of all relevant properties should be made, and the results should be replicated.
- The overall storage properties of the best materials from this project are only a little better than the activated carbon materials MSC-30 or AX-21. Apples-to-apples comparisons should be made. For example, testing should be performed for MSC-30 monoliths compared to boron-doped 3 K monoliths prepared under the same conditions. It seems that efforts would be better spent on improving the current materials than working on engineering scale-up.

## Project # ST-021: Weak Chemisorption Validation

Thomas Gennett; National Renewable Energy Laboratory

### Brief Summary of Project:

The overall goal of this project is to evaluate the spillover process as a means to achieve U.S. Department of Energy (DOE) 2017 hydrogen ($H_2$) storage goals. To advance the accomplishment of this goal, this project specifically focuses on four objectives: (1) validating measurement methods, (2) identifying and synthesizing several candidate sorbents for spillover, (3) determining $H_2$ sorption capacity enhancement from spillover, and (4) observing and characterizing spillover $H_2$-substrate interactions with spectroscopic techniques.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.6** for its relevance to DOE objectives.

- The room-temperature $H_2$ adsorption is a very attractive technology option for onboard $H_2$ storage, and accurate measurement is the key to the success of this project.
- Given the diversity of broad disagreement in the scientific community about spillover, this project is vital to bring about consensus.
- This is a critical project for the DOE Hydrogen and Fuel Cells Program because it will establish if spillover is viable in a way that will be hard to debate. With that knowledge, smart decisions can be made on funding spillover work in the future.
- Developing reliable, validated measurements of $H_2$ stored via the controversial, often irreproducible spillover mechanism is important to DOE to evaluate the future promise, or lack thereof, of the spillover mechanism for $H_2$ storage.

### Question 2: Approach to performing the work

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

- Selecting samples with established and standard preparation procedures was a good starting point. At a minimum, it is recommended to expand this work to the samples that are reported with much higher capacities.
- Although ruthenium (Ru), palladium (Pd), and platinum are not practical for real applications, using these catalysts is a good choice for fundamental understanding. It would be nice if the principal investigator (PI) could also indicate how much enhancement one must achieve to make spillover viable for $H_2$ storage systems based on today's best available materials. It is unclear if it is 15%, 50%, or 100% enhancement.
- Air-stable, synthetically facile materials with a well-established detection limit of 15% are excellent starting conditions. Although the process is both described as chemisorption and hydrogenation (slide 13), something to consider is whether the project team can exclude hydrogenation through calorimetry tests.
- The use of doubters and adherents in all experimental phases is key to having a robust result suitable to inform future research and development program decisions at DOE. The precalibration between laboratories and the use of spectrographic characterization methods to study the effect observed is excellent. This is a model project, in these aspects. The use of a wide set of spectrographic data to try to understand spillover as an $H_2$ storage process

is also excellent. The project team's only fault is that to get stable materials with fast kinetics, it gave up the possibility of seeing a large enhancement.
- The approach to validating the spillover of $H_2$ onto substrates was appropriate, with a combination of reproducible sorption measurements and characterization techniques. A criticism of this general approach is that other than the sorption measurements, the spectroscopic measurements tend to be more qualitative, and they are only made semi-quantitative with a lot of hard work.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The samples are synthesized with well-controlled and repeatable conditions. All of the measurements are well planned and conducted.
- The identification of water (from catalyst reduction) in the measurements was very important. It is recommended to reconsider the reliance on Diffuse Reflectance Infrared Fourier Transform Spectroscopy (DRIFTS) measurement to establish reversible spillover, given the high noise level.
- Regarding the material presented on the high-temperature activation slide, it would be nice to know whether further heating of Ru-hydrogen bonds converts the metal back to zero valency and releases $H_2$, or if it merely transfers this hydride to the proximal substrate, altering the system (possibly observed by cycling this system). It was unclear if the PI measured the $H_2$ released during the 250°C degassing. If the carbon-hydrogen bonds proximal to the Ru catalytic site appear activated for reversible release of $H_2$, why their absorbance is not shifted should be explained. Carbon-hydrogen bonds that are agostically interacting with metal centers have clear spectroscopic signatures. This is difficult, but for all DRIFTS measurements it would be nice to see positive controls with dosing similar to the proposed unknown measurements, thus instilling confidence in the small "peaks" that are highlighted. In reference to the $RuBC_x$ solid-state Nuclear Magnetic Resonance, if one doses the carbon support with less boron, it would be interesting to see if an intermediate chemical shift (between 5 and 7 ppm) and less broadening is observed.
- The results are good, but perhaps a bit slower than expected. The identification of a potential spectroscopic trace of spillover is good, but until it is understood, there will still be room for doubt as to what is being seen in the spectrographic data. That notwithstanding, the widely agreed establishment of spillover as a real but small (in these systems) phenomenon is a big accomplishment. While the project team chose the samples for the valid reasons of fast kinetics and good stability, none of these samples was likely to offer high-capacity change (4% plus), so the small enhancement is in the order that might be expected. Thus, the goal of estimating if spillover can meet DOE goals is somewhat in doubt; it is too open-ended of a question to prove the goals cannot be met using the spillover mechanism. There is no physical example of the goals being met, and the theory is not dependable based on the wide range of theory results in the literature. It was good to see the rigor in data reduction, such as in the data on the Pd-templated material.
- The progress toward making reliable, reproducible sorption measurements of $H_2$ spillover on "model" materials has been good. The quality of the measurements gives confidence that the amount of spiltover $H_2$ is being measured with accuracy and precision. This quantity is unfortunately much lower that the claims of others that inspired this work. Therefore, this work at the National Renewable Energy Laboratory is very valuable in correcting the overly ambitious claims made previously by other organizations. The work falls short in providing adequate, confident correlations of the spectroscopic data given the small amount of spiltover $H_2$.

### Question 4: Collaboration and coordination with other institutions

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

- The overall activities involve several partners, and the efforts are well coordinated.
- The fact that this project is inherently a high-collaboration program is why it is a good project.
- Other institutions (e.g., Penn State University) within the Hydrogen Storage sub-program are also conducting related and similar work. It is recommended to consolidate these projects or have clear distinctions between them.

- This project features strong collaborations, especially in the sorption measurements and in improving the sorption measurements that others in the field are performing. The project is a terrific service to the sorption community. The project is well done.

### Question 5: Proposed future work

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

- The spillover effect has been studied for awhile. A conclusion should be achieved soon in terms of whether this is a realistic pathway for onboard $H_2$ storage applications. The continued fundamental understanding could be accomplished within the scope of the DOE Basic Energy Sciences Program.
- Granted, the project is nearly over, but it is unclear how “enhancement” measurements will help DOE judge whether spillover materials can meet automotive application targets. It is unclear whether absolute capacities are required, and whether they can be bounded.
- This project looks like good work, but it would have been good to have seen a dependable estimate of what might be possible with spillover.
- It would be recommended to expand this work to include samples reported with the highest weight percent $H_2$ uptake within the Hydrogen Storage sub-program to prevent continued controversy.
- Based on the current results, which are similar or identical to other reliable researchers, now is the time to develop the best case scenario for spillover of $H_2$ onto substrates for DOE and other researchers interested in this area. Particular focus should be placed on developing chemically/catalytically realistic, thermodynamically and kinetically reasonable pathway(s)/mechanism(s) to provide a “best guess” of the maximum amount of $H_2$ adsorbed via spillover at room temperature and technologically useful pressures. The existing future work statement seems to be an exercise without any well-defined endpoints, go/no-go decisions, etc. The stated future plan includes a task to go back into the “materials design phase”; the project team should first have to justify what chemically reasonable pathways can get these materials into the ballpark of achieving the DOE $H_2$ storage targets before proposing any additional future work. This work should not stray from the goal of providing the storage community with information regarding the ultimate potential for $H_2$ storage these spillover mechanisms enable; if, as it seems now, there is no path forward, then this line of research should be terminated.

### Project strengths:

- This project features a good, coordinated effort between multiple organizations along with well-planned experiments and measurements to evaluate the spillover effect.
- The choice of materials, protocols, and the team assembly are all good.
- Strengths of this project include the mix of adherents and doubters, the spectrographic data and $H_2$ capacity measurements, and the calibration of capacity results from all laboratories beforehand.
- This project’s capable scientists are an area of strength.
- This project features competently performed sorption measurements that are reproducible, accurate, and precise.

### Project weaknesses:

- It is not clear when a final conclusion will be drawn for the potential of spillover to meet the $H_2$ storage target.
- An area of weakness is how the project team just talks about “enhancement” values without discussing approximately where these values need to be for spillover materials to be useful.
- It is not clear that the theory behind $H_2$ spillover will be as uncontestable as the experiments.
- Analysis of the samples with a high reported weight percent is missing. The concern is that no conclusion may be reached on the optimum amount of $H_2$ that could be stored via spillover.
- A weakness of this project is its ill-defined future research and development plans. There is also a lack of go/no-go criteria, and it is unclear what the targets are. Achieving an additional 15% enhancement of a very small capacity number is a questionable metric.

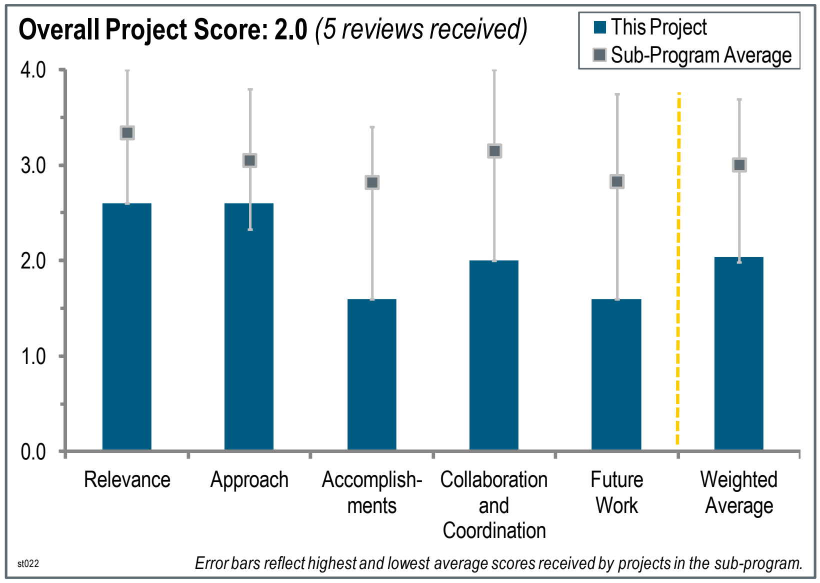
## Recommendations for additions/deletions to project scope:

- It will be good for the PI to compile a table with the direct comparison of micropore/total pore volume changes among all of the samples with and without metal and with and without metal reduction treatment. This will help to draw an insightful technical conclusion for the cause of improved $H_2$ uptake of spillover samples.
- Other institutions within the Hydrogen Storage sub-program are also conducting related work. It is recommended to consolidate these projects or have clear distinctions between them.
- The project team should quickly develop a set of go/no-go criteria for this and other $H_2$ spillover projects. This could also encompass the “metal modified” metal-organic framework projects that have many of the same issues.

# Project # ST-022: A Joint Theory and Experimental Project in the Synthesis and Testing of Porous COFs for On-Board Vehicular Hydrogen Storage

Omar Yaghi; University of California, Los Angeles

**Brief Summary of Project:**
The overall objective of this project is to develop new materials to meet U.S. Department of Energy (DOE) system hydrogen ($H_2$) storage targets. This objective involves: (1) making a theoretical prediction of $H_2$ storage capacities to guide chemistry, (2) synthesizing lightweight crystalline porous solids for the metalation, and (3) measuring $H_2$ uptake and adsorption enthalpy. Covalent organic frameworks (COFs) for lightweight crystalline porous solids will demonstrate control of structure, topology, and interpenetration; consist of lightweight materials; be designed for functionality; and be suitable for metal impregnation.




## Question 1: Relevance to overall DOE objectives

This project was rated **2.6** for its relevance to DOE objectives.

- This is an important project that examines possible new avenues to design room-temperature adsorbents for $H_2$ storage.
- The scope of this project is aligned with the DOE Hydrogen and Fuel Cells Program (the Program). Sorbent-based $H_2$ storage technology is one of the most attractive options for advanced storage systems. COF has certain advantages compared to carbon and metal-organic frameworks.
- Increasing the binding energy of adsorbed $H_2$ is critical for this technology in terms of the DOE goals.
- The relevance of the project as presented is important; for example, to improve binding energies and synthetic scale-up. However, the data presented did not seem relevant to establishing a pathway to achieve DOE project goals.
- This collaboration appears to be an effort at predicting structures of potential interest for storage applications. While some effort appears to have gone into computational design aspects of COF structures, it appears that little work has been accomplished over the past year in structures that have been synthesized. Rather, the empirical effort seems to be that of producing as many structures as possible while providing little physical insight from the less-than-optimized materials that have been made. While at the start of the presentation reference was made in a number of viewgraphs that illustrated data from several years, no apparent progress has been made since then in showing better material properties. While this is not a problem per se, it is not clear whether any attempt has been made at trying to optimize any of the new materials that have been synthesized over the past year.

## Question 2: Approach to performing the work

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

- The theory-guided experimental approach should provide a quick evaluation of the potential of this class of sorbent.
- The approach is original but risky; some of the experimental results on synthesized and seemingly difficult-to-activate materials are far below theoretical predictions.

- It is unclear whether there is an attempt to address barriers in this work. The overabundance of previously reported work that was presented as part of the overview serves to highlight the lack of progress made over the past year.
- The theoretical approach is relevant and individually marks as a 3. However, the poor contributions and approach from the experimental side are disappointing.
- A description of the theory and methodology to compare simulation results with experiments should be provided. The agreement between theory and experiment is usually very good. It is unclear if the experimental data is rescaled by the specific surface, and whether the specific surface of the materials has been evaluated numerically either through an insertion method or a simulated BET measurement. It is also unclear how theory and experiments compare from the point of view of the specific surfaces and pore volumes, and how those are measured. In some cases it seems that the theoretical and experimental excess isotherms are closer than the absolute adsorption isotherms (for example, all of the materials compared in slide 5 of the PowerPoint presentation), and it is unclear whether there is an explanation for this.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **1.6** for its accomplishments and progress.

- The project has made very little progress toward metalation since last year. From the results, it can be clearly seen that the developed material will be far short of meeting the DOE goals.
- It is unclear if it will be possible to synthesize the promising material presented by the theory group, and if this material is practical. It is also unclear if it can be improved so that the systems' criteria are met at room temperature.
- Slide 21 shows a summary of the work as it pertains to the Program for the past year. Apparently the empirical effort consists of having produced one material, COF-320, for which a relatively low surface area of 1,620 $m^2$/gm and a 0.6 wt.% uptake at 1 bar and 77 K were measured. While slide 19 shows that this material can be synthesized to >10 gm levels, there is nothing to indicate in the data produced so far that such quantities at such surface areas are of topical interest to the goals of the Program.
- There is a lack of follow-up experimental work after a good structure is predicted. For example, COF-105 and COF-108 have never been experimentally made in reasonable quantity for the evaluation of their real $H_2$ storage properties. It is also not clear why COF-3xx samples were studied in depth. With such a small amount of $H_2$ uptake, it is unclear if there is a chance for this class of materials to ever meet the $H_2$ storage targets.
- There was only one slide presenting new data. Most of the work appeared to be focused on the production of gram quantities of linker molecules, showing neither pathways nor results of improved batch synthesis. This seems like a gross deviation from the tasks and milestones as outlined. It was disappointing that data from 2006 and 2008 were the major focus of the presentation and that all other results (slides 21 and 22) were in progress or not attempted yet. Based on the results presented, funding of the project should be discontinued; it appears that there is no concerted effort to finish tasks.

### Question 4: Collaboration and coordination with other institutions

This project was rated **2.0** for its collaboration and coordination.

- The collaboration between the theory group and the experimental group should be described in more detail.
- It appears that collaboration between the two main partners of the project is minimal.
- This collaboration does not appear to work too effectively, given the productivity of the empirical effort. Based on the presenter's inability to answer several of the questions posed, there appears to be a lack of engagement with the work that has been accomplished.
- The interaction between this project and other DOE Hydrogen Sorption Center of Excellence and Hydrogen Storage Engineering Center of Excellence partners is not clearly demonstrated. The experimental measurement should be validated at the National Renewable Energy Laboratory or another DOE partner facility.

### Question 5: Proposed future work

This project was rated **1.6** for its proposed future work.

- There was no apparent path forward presented. It does not seem that this project is a priority of the principal investigators.
- It is not clear how the planned future work will ever address the current gap between where the project stands in COFs and the $H_2$ storage target.
- Results show enhanced storage capacity at room temperature; however, it is far below that of currently available technologies.
- Given the lack of accomplishment over the past year in having performed one measurement on one non-optimized material, the characterization work described as "work to be performed" should have been accomplished already.
- The authors have a path forward with a new material that could show real progress toward achieving some of the DOE goals. The best materials predicted by the theory group still would fall short of the systems DOE targets. It is unclear if there is a strategy to go beyond this material.

### Project strengths:

- The project features good material synthesis capabilities.
- The effort put in by the theoreticians is a strength of this project.
- The project has improved understanding of the mechanisms. The theory team has proposed a new material.
- The theory-guided experimental approach could provide a quick evaluation of the COFs' potential and compare their advantages and disadvantages to other sorbent materials.

### Project weaknesses:

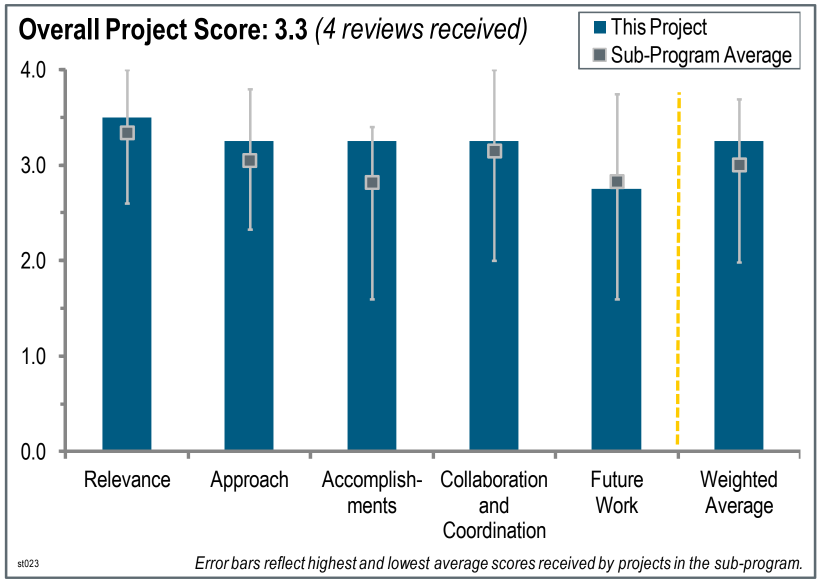
- This project's empirical effort in addressing the goals of the Program has not been productive.
- This project is not making progress in terms of practical onboard $H_2$ storage applications.
- There does not seem to be any defined path forward to accomplish the milestones.
- No materials presented in the review so far have shown significant progress toward reaching storage densities in the ballpark of the DOE goals at room temperature.
- Although a lot of good theory prediction is coming out of this work, there is a lack of experimental follow-up on the good structures that have the potential to meet the target. It is also not clear why a lot of effort was focused on the materials with low $H_2$ which will never meet the target.

### Recommendations for additions/deletions to project scope:

- It would be good to have a table of the properties of the materials examined by the group so far to better evaluate progress; for example, specific surface, pore volume, excess density adsorbed at 77 K and 298 K, and isosteric heat of adsorption.
- If the project team does not reach its milestones in September, the project should be discontinued.

# Project # ST-023: New Carbon-Based Porous Materials with Increased Heats of Adsorption for Hydrogen Storage

Randy Snurr; Northwestern University





## Brief Summary of Project:

The overall objective of this project is to develop new materials to meet U.S. Department of Energy (DOE) volumetric and gravimetric targets for hydrogen ($H_2$) storage. The goal is to use metal-organic frameworks (MOFs) and polymer-organic frameworks (POFs) to create room-temperature $H_2$ storage sorbents that have both high heats of adsorption and large surface areas. This year's objectives specifically focus on: (1) obtaining validation for previous results; (2) developing high-surface-area materials for cryogenic storage and high-surface-area materials containing functional groups that can bind $H_2$ at room temperature; and (3) using modeling to screen cations and cation environments for their ability to bind $H_2$ and the resulting storage capacities, and to assess the relationship between high surface area, pore size, and strong binding sites with respect to performance.


## Question 1: Relevance to overall DOE objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This project is highly relevant to the DOE Hydrogen and Fuel Cells Program (the Program) in terms of the use of computational screening to help in the discovery of higher-capacity materials and providing new methods for increasing the adsorption enthalpies of physisorption materials.
- The principal investigators (PIs) in this collaborative effort have a clear understanding of the scientific issues and appear to work well together in addressing the thermodynamic and density issues associated with solving the storage problem. Both the theoretical and empirical efforts are focused efforts and include a "global" computational screening approach to addressing both the prospective gravimetric and volumetric requirements of the Program.
- Understanding the basic interaction of $H_2$ with complicated chemical functional groups and exposed metal centers in MOFs is a developing science, and this work contributes significantly to this effort. Ultimately, it needs to be known what materials are potential candidates for high-capacity, room-temperature storage, and the combined MOF/POF and calculation approach here is reasonable, even if the work is only partially successful at this point (e.g., high surface areas in MOFs, modification of the isosteric heat of adsorption [$Q_{st}$], and screening calculation development).
- The project, if successful, will have an impact on DOE storage objectives. However, it did not deserve an "outstanding" rating because while the science/scope is excellent, one cannot be certain that the framework materials will be applicable to transportation applications due to issues with purity and stability. However, they are a significant improvement over several of the hydride materials.

### Question 2: Approach to performing the work

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

- The combination of approaches between calculation and synthesis is potentially the best approach to reaching DOE goals for this class of materials.
- The approach of combining the modeling and experimental work to develop new materials is a strength of this project.
- Through the combination and actual interaction of the synthetic scientists and the theorists, the project team has outlined an exceptional pathway to achieve its goals in this project. While not truly an inverse design approach, it is unique in that it addresses and limits the no-go synthetic possibilities.
- As with all efforts that have an experimental component, full materials optimization has not yet been achieved, at least in the case of the compelling manganese (Mn) POP materials. Approaches that minimize the extent of trapped solvent in structures of this type are critical in judging the potential for use of a material of this type for engineering applications. Supercritical $CO_2$, which is the approach that has been adopted post synthesis, has helped to address some of this problem. Perhaps an approach similar to Jeff Long's optimization of MOF-5 relying on synthesis in an inert atmosphere might also be worth exploring.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Progress has slowed since the prior year in terms of improved results, but not in terms of effort toward discovery, testing, and development of new materials. The project has shown a high level of effort in making progress toward DOE goals.
- From the standpoint of addressing the issue of heats of adsorption, the work on the low-surface-area Mn POP that shows a fairly constant enthalpy ($\Delta H$) is of particular interest because it appears to be the only candidate system that displays such behavior, albeit over a somewhat short range of uptake. While it may fall somewhat out of the purview of the goals of this project, providing some level of physical insight into how such constant $\Delta H$ has been observed would be of overall scientific and technological interest.
- While the researchers have made excellent progress on their project, especially on the theoretical initiative, there is an issue with their "normalization" of the National Renewable Energy Laboratory (NREL) generated data to reach their milestone. The normalization factor applied is not valid. Also, it leads one to believe that their volumetric measurements are more in question, through their own admission. While good progress has been made on the purification and removal of solvents from the framework materials, there are limitations in that they cannot purify a material on-site at NREL to the same level as in their own laboratory. Granted, they are suffering from the history of erroneous results in the storage community at large, but their "correction" factor only exacerbates the problem.
- This past year does not seem to have yielded many experiments or results. Last year, there were indications that the researchers have characterization beyond the laboratory (neutron scattering), but no information was presented in this report. Speculation about the interactions of $H_2$ with theoretical or model structures is fine, but the real experiments and difficulties associated with activation and recycling are absent. The real gap is (not only in this project) between theory and experiment; multiple $H_2$ binding in first-row transition metals and light metals has not been shown in any of these systems (either structurally or spectroscopically), and indications are that the $H_2$ takes up significant volume next to its adsorption site, even with relatively high binding strengths (neutron diffraction). Most of the new experimental work seems to be through collaboration with NREL, and the data show significant discrepancies with Northwestern University's (NWU's) own measurements. Potential explanations were given, but applying a universal scaling factor to account for the difference between facilities is not reasonable and validates a lower value than indicated last year (albeit still a large value). Increasing $Q_{st}$ seems to have had some mildly interesting results, but the data are only shown to 0.4 wt.%. It is certainly worthwhile to extend this measurement range to correlate $Q_{st}$ and how it drops with coverage to metal content and species. The final uptakes on metal decorated systems will always run afoul of reducing specific surface areas with increasing metal content. The limits of this need to be understood for these systems.

### Question 4: Collaboration and coordination with other institutions

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

- The PIs in this effort appear to have a clear feedback approach that works within the NWU collaboration, and the help provided by NREL in the evaluation of their materials has had a positive impact on improving the analysis and synthesis of materials for this project.
- The technical validation through the collaboration with NREL is commendable. Other projects should do the same. Given that this project is nearing the end, the synthesis and modeling tools developed should either be continued or shared with the international community.
- NREL and the work with Parilla is the only evidence that there is interaction with other $H_2$ storage groups, and the corresponding results of this interaction were presented. This actually should benefit NWU significantly in improving its isotherm measurement capabilities for the future. It would be good to see some results from the project team's other collaborating interactions, but unfortunately there was little to judge in this presentation.

### Question 5: Proposed future work

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

- Given the timeline, this is a reasonable effort, but it is unlikely to achieve DOE goals in the remaining period.
- The proposed future work is acceptable as long as the researchers get their sorption measurements issues resolved.
- There is a long list of future work with very little time remaining to accomplish the work. It is recommended that the remaining work focus on one or two goals only; specifically, those that will provide the ability to continue work in this area, such as validating the high-throughput computational screening and high-pressure measurements on one or two of the current best candidates for increased room-temperature capacities.
- While the PI has presented what seems to be a logical set of approaches for proposed work, he does recognize the limits of his first bullet point in the limitations of the use of metal additions in the attempt to increase the heat of adsorption, which will counterbalance the goal of high gravimetric density. Unfortunately, this overall avenue of pursuit in the interest of finding an MOF or POF (or generic coordination polymer) for $H_2$ storage applications may be too difficult a system to pursue, given issues such as precursor expense, solvent extraction difficulties, and limited charge transfer effects from the metal to linker (that might affect a reasonably constant $\Delta H$). The low score in this case is not a reflection of the work as proposed by the investigators, but an assessment of the limitations of continuing to pursue this overall area of work for $H_2$ storage applications.

### Project strengths:

- This project features unique synthetic approaches and an excellent group.
- This project features a strong collaborative effort with PIs who clearly understand the issues of the problem and have chosen a narrow, promising set of systems to investigate and optimize for the project.
- Strengths of this project include the combination of approaches, the expertise of PIs, its productivity, and the papers published.
- The collaboration with NREL to validate NU-100 $H_2$ storage properties was an important step and successful. Extensive synthesis and experimental characterization on the introduction of metals into the MOFs and POPs was another strength of this project. The development of high-throughput computational screening has the potential to dramatically reduce discovery and development time and efforts, and if proven successful through experimental validation, it will be of great benefit to the Program.

### Project weaknesses:

- The volumetric measurements are an area of weakness.
- Extensive modeling and synthesis work has been accomplished, but it has resulted in only relatively minor improvements over prior materials.
- In the end, given what is now known about coordination polymers and their likely utility for storage applications, this overall avenue of research is proving to show its limits for technological adoption. While the PIs have

demonstrated a dedicated level of engagement in their work, the overall demands that combine ΔH, gravimetric, and volumetric targets will probably prove too challenging for materials of this type.
- Weaknesses include linking theory to experiment, and a lack of detail on the impacts of metal incorporation. There are unsatisfactory in-house isotherm capabilities and/or sample reproducibility between facilities, and weak external collaborations.

### Recommendations for additions/deletions to project scope:

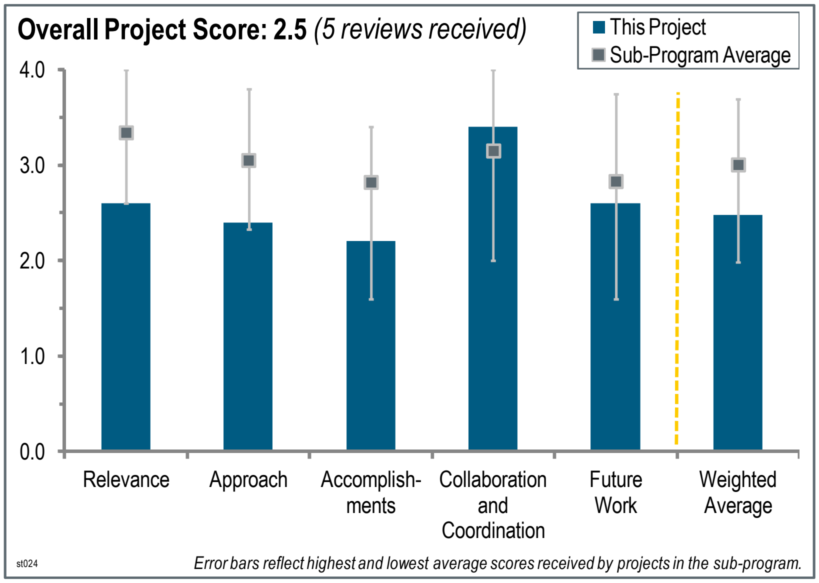
- The PIs have laid out the best course of action for their work and should continue to pursue their work as outlined in their presentation.
- The high-throughput computational screening is potentially a very powerful approach. However, it is very important that it is validated through experimental work before the end of the project. One suggestion would be to randomly select a few (feasible) candidate materials from the computational analysis (some with high predicted capacities and some with low capacities) and test these to see how well the computational models predict their $H_2$ storage properties. The calculated capacity plots do not identify specific materials; therefore, it is difficult to evaluate if there is a general trend that high gravimetric capacities correspond to low volumetric capacities. At a minimum, this potential correlation should be evaluated on the present data set.

## Project # ST-024: Hydrogen Trapping through Designer Hydrogen Spillover Molecules with Reversible Temperature and Pressure-Induced Switching

Angela Lueking; Pennsylvania State University

### Brief Summary of Project:

The overarching objective of this project is to synthesize designer microporous metal-organic frameworks (MOFs) mixed with catalysts to enable hydrogen ($H_2$) spillover for $H_2$ storage at 300 K–400 K and under moderate pressures. This project addresses the barriers of gravimetric capacity, minimum and maximum delivery temperature, maximum delivery pressure from a tank, and volumetric capacity.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.6** for its relevance to DOE objectives.

- Room-temperature $H_2$ adsorption is a very attractive path, and spillover is critical to this path.
- The evaluation of whether spillover has any potential to be a viable method for room-temperature $H_2$ storage is relevant to DOE objectives.
- The project is focused on assisting DOE with unraveling the controversy and confusion surrounding the topic of spillover as applied to $H_2$ storage. The relevance is good; the principal investigator (PI) has performed a relevant experimental project using $H_2$ uptake and spectroscopic studies to provide reproducible data on the model spillover materials.
- The DOE Hydrogen and Fuel Cells Program (the Program) is concerned with the wide variability of spillover measurements, so it is clearly important to help out with the reproducibility, methods, etc. It is unclear why these efforts should supplant the original goals of this work, as there is already a group charged with this mission (National Renewable Energy Laboratory [NREL]). In addition, although it is important to find measurements that prove/disprove this concept, it is unclear what the goal is regarding enhancement. It is unclear what value or range of valves (enhancement) constitutes success, or at least indicates that this concept might (eventually) yield materials to meet DOE targets.
- The spillover effect is in question as a meaningful method for $H_2$ sorption. It is important to understand, but at the present it does not seem likely to be a viable mechanism. In addition, there is a larger and more focused project that will likely go well beyond this one, making this project less important. Working at 20 bar may provide data that is valid at those conditions, but that is not where the DOE Office of Energy Efficiency and Renewable Energy (EERE) is aiming. High pressure for room-temperature adsorption is needed to reach DOE goals, so in that aspect the work is not fully aligned. This really would fit better in the DOE Basic Energy Sciences (BES) Program or the National Science Foundation.

### Question 2: Approach to performing the work

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

- Accurate measurement is critical to the success of this project. The PI deserves all of the credit for modifying the original approach in order to address the reproducibility issues.
- The approach is sound. Getting reproducible results is important, even though it has proved difficult. It is meritorious that the group changed direction when it was the right thing to do. On the other hand, doing lots of

work at only 20 bar and room temperature is not investigating the really important adsorption area and risks missing the real impact for the EERE goals.
- There are several approach slides suggesting a fluxional scope. In the original approach, it is desired to maximize metal dispersion (it is unclear how that will be measured) and optimize $H_2$ receptors. It is unclear what is meant by "optimizing $H_2$ receptors." Then, there is a search for correlations between spillover and functional groups. It is unclear how the proposed experiments will put bounds on the potential of spillover to meet automotive applications.
- Prior work focused on improving the quality of the measurements and validation. The focus of the 2012 work is less clear. It appears that more questions have been generated than answers regarding key material performance behavior.
- The PI's approach is to validate (or not) historic observations of the controversial and often irreproducible phenomenon of storing a small excess amount of hydrogen on surfaces via the well-known (to catalyst scientists) spillover of hydrogen onto substrates. The PI was careful to explain that the research was directed at validating (or not) spillover on known materials types and developing some potential mechanistic information, not developing new materials for storage via spillover. The PI used a variety of appropriate spectroscopic techniques correlated with $H_2$ sorption studies at room temperature and at relevant pressures that were validated by NREL. This approach is good, and as the PI recognizes, it is fraught with difficulties because searching for a potential minority species responsible for the very small (0.1%) amount of spiltover hydrogen stored on substrates at room temperature is a difficult task.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.2** for its accomplishments and progress.

- The project team has made good progress in collecting spillover evidence. However, it is not clear how the base materials were chosen for the study because the MOFs CuBTC and IRMOF-8 have low capacity.
- Results from collaborators' work were shown, but it was not clear whether much progress has been made by the PI. No high-pressure results were presented. Very few new material results were presented. Many questions about the $H_2$ storage performance of current materials that should be easy to address have not been answered by the current experimental work.
- Progress has been slow, which may reflect the fragility of the underling concept. It is unfortunate that the $H_2$ signal is so poor in the Temperature Programmed Desorption, because it makes the $H_2$ uptake and the nature of it unclear. The effect of oxygen is interesting, but it is hard to tell if it is meaningful in storage because the majority of the gain is below 1 bar. Indeed, it may be hurting the usable amount because the slope with pressure seems lower.
- On slide 15, the data indicate approximately 0.5 wt.% $H_2$ storage capacity, yet the notes indicate an irreversible reaction. It is unclear how this is evidence of spillover, and the implied reversible concept. Perhaps this is not hydrogenation. Concerning slide 17, it is unclear why the C=O infrared absorptions would occur in nearly the same place when a metal is still coordinated to the putative COOH functionality. It is not clear if the PI (or the literature) has model complexes that show similar hydrogen uptake that support the project's assertion. If this hydrogenation has occurred, it would be interesting to know what storage capacity the material has. In reference to slide 21, it is unclear if the pores are sufficiently large to allow "infiltration" of $PtCl_6^{2-}$ anions. If the PI chooses MOFs that have little or no preference for the metals the researchers are doping, it is unclear if the MOF structure will stay intact. It is also not clear how one can measure the degree of infiltration. It is understood that only a portion of the funding was acquired this fiscal year.
- The approach was carried out carefully, and the results were reproducible and the sorption experiments were validated by NREL. One can have high confidence that the results shown are relevant to answering some of the questions about the mechanism(s) of storage of small amounts of $H_2$ by spillover on surfaces, and have the potential to lead to some understanding of the chemical and physical limits of this storage approach. If there is a criticism to be made, it could be that the techniques used developed qualitative data. With a substantial amount of additional work, some of this could have been made semiquantitative and might have enabled a slightly more compelling story regarding the potential of the relative concentrations of the absorbed species. As such is typically the case in such studies, this would be very difficult, but it would add tremendous value if more quantitative spectroscopic analyses were available.

### Question 4: Collaboration and coordination with other institutions

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

- A good team of collaborators has been assembled and this work is being leveraged with a present BES project.
- The PI demonstrated good effort in collaborating with other groups to address the reproducibility issues, both in measurement and materials synthesis.
- This project features good collaboration with NREL and others, as well as positive interaction and coordination with a BES-funded project.
- There is good collaboration with the NREL-led spillover validation effort.
- Collaborations are appropriate and the coordination, especially with NREL, was apparent.

### Question 5: Proposed future work

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

- Changing the isotherm shape is well worth pursuing.
- The project team should plan to make the doping methods more in line with the project goals. The value of the other proposed future work to the project and the Program is unclear.
- The planned work may result in further fundamental understanding. However, with the current understanding that spillover will not meet the DOE 2017 target of 5.5 wt.% $H_2$ system capacity, the PI should deliver a plan to address this issue.
- Plans for future work were fine, but in the very near future the researchers will be focused on meeting the go/no-go decision metric of demonstrating a 5.5 wt.% material. Given their current results and the comments the PI made, it seems unlikely that this criterion will be able to be met via spillover. This is not a criticism of the PI; this is just what the physics and chemistry appear to allow.

### Project strengths:

- Strengths of this project include the coordinated effort in understanding the real spillover effect and experimental reproducibility.
- Substantial rigor is applied to the experiments undertaken.
- This project features good collaboration with NREL.
- One strength is how the researchers are looking at a mechanism and not making a better material, per se.
- The project team has competently carried out experiments that are reproducible, with validation from others.

### Project weaknesses:

- Compared to the best baseline materials, it is not clear how much of the gap the spillover can address in order to achieve 5.5 wt.% excess $H_2$.
- It is unclear how this project will generate new materials that have a path to meet DOE targets. Although exploring spillover is of fundamental (BES) interest, without bounding the potential (perhaps with theory) of the proposed materials, it is difficult to conclude any progress has been made.
- It is unclear whether the few materials tested have any significant improvement in reversible room-temperature $H_2$ storage.
- The project has become a little unfocused over time, and the area is largely covered by other projects.
- In the approach, perhaps more emphasis should be placed on obtaining semiquantitative information on species adsorbed. Researchers could possibly look at other indirect methods to quantify hydrogen spillover; for example, via a chemical reaction to “trap” adsorbed hydrogen deuteride (HD), with potentially more chance of quantifying the chemically trapped HD. DOE is expending a significant amount of resources chasing down what appears to be a very minor probability of achieving anything more than a minor amount of $H_2$ stored. This is not a criticism of the PI’s effort, only a comment about the reality of practical $H_2$ storage via spillover.

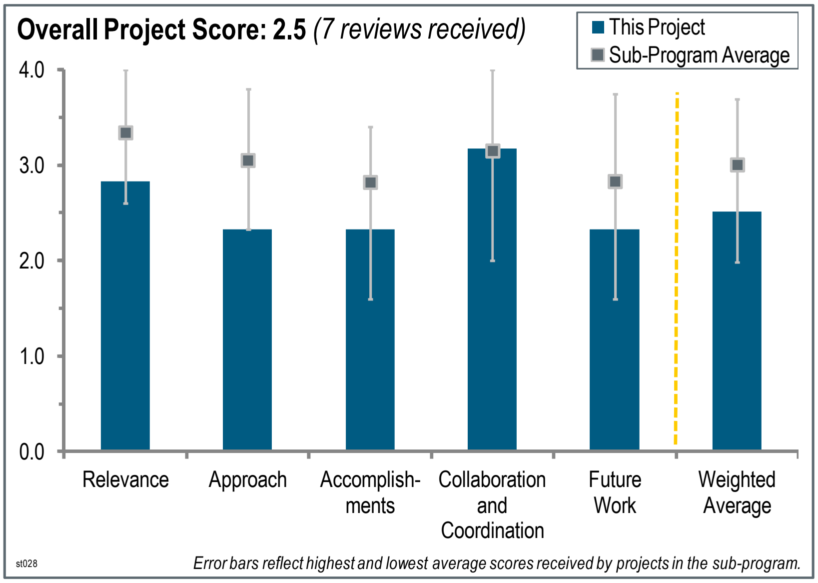
### Recommendations for additions/deletions to project scope:

- The PI could consider some material-level calculations based on the current available experimental data and recommend what kind of the material properties one should achieve in order to meet the system target of 5.5 wt.% $H_2$ with this spillover approach.
- The work done would be more highly valued by BES, and it would be wise if it were possible to hand this contract to BES at the go/no-go point. While this reviewer knows of no examples of this type of action, it would be best for all programs and the PI.
- The project team should develop a chemically viable "vision" of what the ultimate spillover mechanism would allow for with regard to the capacities, temperatures, and rates of $H_2$ release from the ultimate spillover material.
- No significant improvements in the verifiable ability of the materials being evaluated to be used as a practical means of onboard $H_2$ storage have been demonstrated. Given that the May 2012 go/no-go criteria have not been met, it is recommended that work on this project be stopped.

# Project # ST-028: Design of Novel Multi-Component Metal Hydride-Based Mixtures for Hydrogen Storage

Christopher Wolverton; Northwestern University

**Brief Summary of Project:**
This project aims to address the barriers of system weight and volume, charging and discharging rates, and the lack of understanding of hydrogen ($H_2$) physisorption and chemisorption by combining materials from distinct categories to form novel multicomponent reactions. Systems studied include complex hydrides and chemical $H_2$ storage material mixtures, as well as novel multicomponent complex hydride materials and reactions. The study's approach blends $H_2$ storage measurement and characterization, state-of-the-art computational modeling, detailed catalysis experiments, and in-depth automotive perspectives.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.8** for its relevance to DOE objectives.

- The goal is to design novel metal hydride materials using composites of materials.
- This is a fundamental effort that aims to understand reaction pathways and composite principles, yet it clearly considers DOE targets such as weight, volume, and impurities in the output $H_2$.
- This work is aimed at probing the effectiveness of combining theoretical predictions of complex hydride reactions with experimental work. The experimental effort appears to be the bottleneck in this effort. Two particular systems for study had been identified in 2011: magnesium borohydride ($Mg[BH_4]_2$) combined with lithium borohydride ($LiBH_4$) and the $Mg(BH_4)_2$ combined with magnesium amide ($Mg[NH_2]_2$). Theoretically, these systems appear to make some headway toward the DOE Hydrogen and Fuel Cells Program (the Program) goals.
- This joint project of Northwestern University (NWU); the University of California, Los Angeles (UCLA); and Ford Motor Company involves the prediction and demonstration of mixed component sodium-magnesium-boron-nitrogen (Na-Mg-B-N) hydrides with storage capacities that are potentially large enough so that the DOE targets for passenger vehicles might be met. Experiments are being used to determine the as-prepared and decomposition phases in order to ascertain reaction pathways. The team is also looking at catalysts to enhance the kinetics (apparently, only desorption so far with very limit reversibility being shown) and theoretically identify the mechanisms that control the kinetics. The team's objectives generally comply with the DOE targets and goals.
- The project is focused on addressing key barriers with a variety of high-capacity hydrides and is aligned with the DOE objectives. The first item listed under "Barriers to address" is the "Lack of understanding...," which seems more appropriate for a DOE Basic Energy Sciences Program project. It would be better to focus on a clear objective (e.g., kinetics, reversibility) rather than just the lack of understanding. The lack of understanding clearly needs to be addressed, but the goal should be improvements in the material, not just a better understanding.
- The $H_2$ sorption properties of multicomponent mixtures of complex hydrides are being investigated in this project. Although the project is providing some new understanding on $H_2$ reactions in mixed hydride systems, the technical barriers encountered in these systems, most notably slow kinetics and poor reversibility, have severely

limited their applicability for onboard storage applications. Although the initial idea seemed promising, the limited success in meeting DOE storage material goals is minimizing the relevance of the project.

### Question 2: Approach to performing the work

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

- This project systematically considers fundamental reaction pathways with mixtures and composite hydride materials. There is an unusually good combination of theory (calculation) and experimentation. Although this project should not necessarily be expected to result in a new material that would meet DOE targets, it will provide some useful basic understandings.
- As with a number of other projects that relate to destabilization reactions, the x-ray diffraction (XRD) work used to assess reaction product phases provides a limited amount of information. Other analytical techniques need to be pursued to identify the phases that have been formed. A clearer comparison of the results of adding a cobalt (Co) catalyst to the $LiBH_4$-containing system would be of use, especially if in the Co-containing case, diborane ($B_2H_6$) production is not observed. The activated carbon (AC) addition to pure $LiBH_4$ has been done before by Vajo at HRL Laboratories, with similar observations. The overall goals of the $LiBH_4$ work are not entirely clear. The reviewer wonders if the plan is to look into the reversibility of this material.
- The approach being used to discover improved $H_2$ storage materials is a combination of state-of-the-art first-principles calculations (NWU and UCLA) of possible hydrides and their structures along with predicted reaction pathways. More emphasis is now on the role of defects in diffusion processes with implications for better understanding of the kinetics. Conventional volumetric measurements of storage capacities and kinetics are performed at Ford and NWU, where the latter's researchers are mainly looking for more effective catalysts. While these materials are characterized by XRD and infrared (IR) spectroscopy, other more insightful techniques (e.g., Nuclear Magnetic Resonance [NMR], Raman, and neutron scattering) could be very useful to identify reactants and products more completely, especially with many systems that are amorphous and/or highly disordered. The last point was strongly recommended at the 2011 review as well, but it was not pursued.
- The general approach, as presented, seems to be effective, with computational predictions of reactions guiding the experimental efforts. However, there are a number of key problems with these materials that are not being clearly addressed: (1) decomposition temperatures are too high, (2) formation of stable intermediates limits reversibility, (3) slow kinetics. This project should have a clear focus on these barriers.
- An experimental and computational approach is being used to predict novel materials and reaction pathways, to synthesize and characterize the best candidate materials, and to improve $H_2$ sorption kinetics by catalysis. Unfortunately, the technical effort has not been placed in the larger context of the considerable work that has already been performed by other investigators in this area. If the principal investigator (PI) and his team had built upon that work more effectively, they would have avoided unnecessary duplication, thereby facilitating a more streamlined and focused approach. It is critical for the project team to clearly identify the remaining technical barriers and then provide a detailed plan on how those obstacles will be addressed.
- The project's approach did not acknowledge the long and well-studied history of this general approach. The existing literature in this area seems to have been disregarded. It seems the presenter is not an expert in the techniques of characterization of these complex, often amorphous materials, and was unfamiliar with basic requirements of $H_2$ release experiments using (partially) reversible materials. The presenter seemed unfamiliar with the chemistry of the boranes and of the metal borohydrides. The project did not incorporate an approach to address feedback given last year on characterization issues. The presenter was unfamiliar with DOE targets and their implications (materials versus system requirements).

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.3** for its accomplishments and progress.

- The experimental effort seems to be making some progress with the ammonia-containing systems. Overall, an effort aimed at looking at the ultimate lower temperature limits of solid-state diffusion (a difficult problem) might help to illuminate the overall effectiveness of using reactions of the type described in this work, and at least give appropriate temperature ranges for these reactions (which are very high from the standpoint of Program goals).

- A significant amount of good work has been accomplished and substantial understanding has been developed. Eliminations of unpromising materials have been logically made. From a truly practical perspective, statements such as "ammonia release is practically undetectable" are not very convincing. Discussions on the subject of impurity detection limits with the PI's equipment and optimum test techniques were unsatisfactory. There seems to be growing doubts that these particular materials will ever have adequate properties to allow vehicular application.
- More meaningful progress might have been made had the PI been more familiar with previous published work in this area. Proposed diffusion of $BH_3$ through $BH_4^-$ solid indicated a lack of understanding of borane chemistry, which is a prerequisite to working effectively in this area. $H_2$ release experiments on a (partially) reversible system were performed against a vacuum, rather than against $H_2$ back-pressure. There were not enough cycles on the (partially) reversible system to demonstrate even partial reversibility. The PI did not know the prior literature on $B_{12}H_{12}^{2-}$ anion chemistry with respect to $H_2$ storage mechanisms.
- Some potentially attractive candidate reactions have been theoretically predicted (e.g., those involving $Mg(B_3H_6)_2$ and $AlB_4H_{11}$ phases) that were at least partially verified by testing or other analyses, but none exhibit the highly desirable reversibility behavior at moderate conditions. The work on the $LiBH_4$-$Mg(BH_4)_2$ phases appears to show much of the same behavior as found and published by other research groups over the past couple of years. While the Co-AC catalyst does give some improvement in the desorption properties of various borohydrides, the impact is about the same as already noted by others using a variety of catalysts and additives. No systematic assessments have been done, nor do any novel insights seem obvious from the present results.
- A significant fraction of the technical effort in 2011/2012 was devoted to improving the sorption kinetics and reversibility of selected mixed hydride systems. Although minor improvements in dehydrogenation kinetics were observed in the borohydride and amide systems, the sorption rates and reversibility remained far below the limits of acceptability for practical storage applications. There are several specific concerns: (1) the role played by $B_{12}H_{12}^{2-}$ in the sorption reactions in the borohydride/amide systems must be clarified in more detail—a more complete characterization (especially NMR analysis) is needed to elucidate the role of $B_{12}H_{12}^{2-}$ in the sorption mechanism; (2) the path forward for significantly improving the kinetics remains problematic—a solid plan to overcome this barrier is lacking; and (3) poor reversibility is apparent in all systems—the reasons for limited reversibility have not been fully articulated, and a detailed plan to overcome this obstacle has not been provided.
- There are a number of deficiencies with the experimental work. First, it is not at all clear what the reaction pathways are. An accurate description of the pathways is necessary to inform the computational work and ultimately refine the material in some way. The appropriate characterization techniques (XRD + NMR + ...) need to be performed to determine exactly what phases are formed during decomposition and, similarly, what phases are formed upon re-hydrogenation. Second, experimental measurements need to be made to determine the thermodynamics of the reaction. From the results shown the temperatures are very high, and it is not clear how they will be reduced. It is proposed that $B_{12}H_{12}^{2-}$ is an intermediate, but others have clearly shown that this is a very stable compound that is an end state, not an intermediate. If $B_{12}H_{12}^{2-}$ is an irreversible side product (as opposed to an intermediate), it would suggest that other reaction pathways are taking place other than those used in the modeling. In this case, the predicted thermodynamic values would not apply. The work on $LiBH_4$ does not seem logical. This compound has an equilibrium pressure of 1 bar at approximately 400°C—new catalysts are not going to reduce the equilibrium temperature.

### Question 4: Collaboration and coordination with other institutions

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

- This project is an excellent example of good collaboration.
- The collaboration with Ozolins works particularly well.
- It appears that there are a number of nice collaborations with other researchers, but it is not really clear how they all fit in and what they did.
- Collaborations with multiple university, government laboratory, and industrial laboratory partners have reinforced and augmented the total technical effort. A strong collaboration with an expert in solid-state NMR would be an important addition.
- Excellent interaction was indicated between the theoretical members of this team and also with some experimental groups on some complex hydrides such as $Mg$-$BH_4$-$NH_2$ and $AlB_4H_{11}$. There seems to have been limited collaboration concerning catalyst work on the Co-AC catalyst with the $LiBH_4$-$Mg(BH_4)_2$ system.

- The lack of knowledge on the current literature indicates that there is limited collaboration.

### Question 5: Proposed future work

This project was rated **2.3** for its proposed future work.

- Given the nature of the project, the proposed work makes sense. The computational effort directed at mass transport would be of particular value.
- The proposed future work is good, but the researchers should extend their work into compositions that might offer more immediate hope.
- It is a very good idea to stop work on the borohydride/amide mixtures, because they are not promising. Experimental verification of proposed reactions of $(NH_4)_2B_{12}H_{16}$ and related phases may be useful, providing appropriate methods that are used for characterization. Also, extending and expanding the first-principles calculation of the roles of defects and diffusion processes on reaction kinetics could be very productive because the intrinsic kinetics appear to be rate limiting for reactions of the alanates and borohydrides. However, there does not seem to be any vision or focus regarding future experimental efforts on catalysts with complex hydrides or developing systems with better reversibility.
- There are a number of weaknesses with the proposed future work. It is not clear what is going to be done to reduce the equilibrium pressure. Simply stating "optimize reversibility conditions" is not sufficient—it would be good to know exactly how this will be done. The computational effort has predicted a number of promising multicomponent hydrides, but there is a clear disconnect with the experimental work. The actual reactions that are occurring are not the ones predicted. It is not clear if there is any plan on how to deal with stable (irreversible) side products that limit the reversible capacity.
- The future plans provide insufficient detail to allow for an objective review. (For example, Future Plans, slide 23: "Optimize reversibility conditions for $5Mg(BH_4)_2$+$2LiBH_4$ mixture.") This provides no information about what will actually be done to "optimize reversibility." The same criticism holds for the future work on catalysis. Given the daunting challenges posed by prohibitively slow kinetics and poor reversibility in all of the systems that are being investigated, it is essential that a clearly stated and more detailed plan for addressing those problems is provided.

### Project strengths:

- The computational work seems to be pretty good and has identified a number of interesting multicomponent hydrides.
- The project team is looking at reaction pathways in a good, fundamental sense, and trying to develop an understanding of catalysts.
- The initial idea of exploring multicomponent mixtures of complex hydrides was compelling. The research and development team comprises acknowledged experts in computation and modeling, catalysis, and materials characterization. The computation and simulation effort is especially impressive.
- The two theoretical groups at NWU and UCLA have developed very insightful and effective computation procedures and are performing the prediction and modeling of potential storage materials. A strong working relationship has been established between the theoretical and the industrial partner, Ford. However, the sense of direction for the effort is not clear because the original Ford co-PI left the project for another position.

### Project weaknesses:

- For the empirical effort, there is some redundancy of previous work. More analytical techniques need to be employed for phase identification.
- The compositions presently being studied are not likely to have near-term vehicular application.
- Mostly $H_2$ desorption behavior was described from the experimental studies, where the investigated materials have limited reversibility. The large amount of additives needed to improve desorption kinetics is a concern, and a scheme to create a more fundamental approach is still lacking. Using primarily X-rays and IR to characterize these materials is insufficient, because often the most interesting species are amorphous. It is a very long reach for connecting first-principles calculations of defect formation and migration to establishing reaction pathways and kinetics.

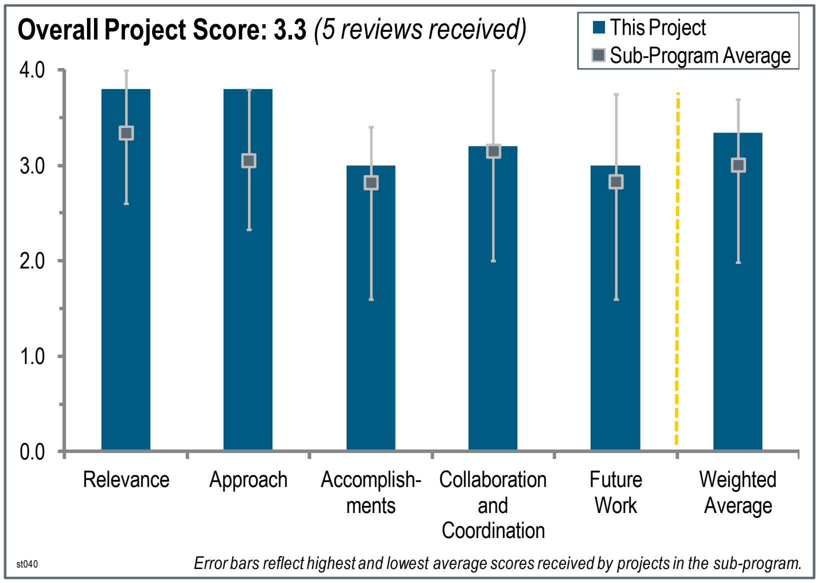
- The experimental work seems weak. Little characterization work has been done to determine the actual reaction pathways. XRD is useful but insufficient; other techniques (e.g., NMR) are necessary to identify amorphous phases. There seems to be a disconnect between the computational work and the experimental work. There does not seem to be a clear pathway for making the necessary improvements.
- Only limited success has been achieved in identifying a candidate material that even approaches the DOE targets. A detailed plan for overcoming the serious obstacles of slow kinetics and lack of reversibility is not evident. The role of $B_{12}H_{12}^{2-}$ could be critical in the overall sorption reaction mechanism—a careful and definitive investigation should be conducted.

## Recommendations for additions/deletions to project scope:

- During the question and answer session, there were strong suggestions to add NMR studies. Otherwise, the project team should continue development of fundamental understandings and begin thinking about more practical mixtures.
- A strong recommendation is that additional characterization techniques such as NMR and neutron scattering should be used to identify any promising materials and their reaction products. To have a more complete vision of the catalyst/additive, phases that exhibit both absorption and desorption should be investigated along with reducing the amount of these additives. The system $LiBH_4$-$Mg(BH_4)_2$ has been widely studied in the international research community and shows virtually no promise as a reversible $H_2$ storage candidate under moderate conditions. Hence, its role as a model system is doubtful. The team should focus the remainder of its project resources on alternative materials. Ideally, these hydrides would have greater reversibility than the borohydride and amide mixtures.
- There should be a feedback from the experimental studies back to the computational work because the predicted reaction pathways seem to be different than what is really going on. Better characterization is needed to understand these complex reactions.
- The results to date on this project do not provide a very compelling case for the adoption of these materials in reversible $H_2$ storage applications. The PI and the DOE Technology Development Manager should have a candid discussion about continuation of the project in view of the weak future plans for overcoming the existing technical obstacles. Specifically, a well-formulated NMR study should be included in the project. This should provide useful information about the role of rate-limiting intermediates such as $B_{12}H_{12}^{2-}$ in the overall mechanism.
- This reviewer recommends discontinuing the project.

## Project # ST-040: Liquid Hydrogen Storage Materials

Benjamin Davis; Los Alamos National Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) develop ammonia borane (AB) (approximately 15 wt.% usable hydrogen [$H_2$])/ionic liquid (IL) mixtures that have sufficient $H_2$ capacity, release kinetics, stability, and fluid phase properties; and (2) work with the Hydrogen Storage Engineering Center of Excellence (HSECoE) to ensure compatibility with system designs. To prevent or forestall the formation of insoluble products after extensive $H_2$ release from AB, the team will develop strategies to define usable temperatures and times as a guide for future designs.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- This project is well aligned with the DOE objectives. The development of a high-capacity liquid carrier based on AB will likely meet many of the DOE targets.
- This project is oriented directly toward HSECoE needs for data on and improvements of AB to assess and develop practical vehicular systems. It is therefore directly related to DOE objectives and targets.
- A suitable liquid-phase carrier for AB slurries is very important to chemical $H_2$ storage systems for vehicular applications.
- The project is highly relevant to DOE objectives and supports the DOE Hydrogen and Fuel Cells Program well. Fluid chemical storage materials offer compelling advantages over solids (such as pure AB), so this is a valuable project. The project has good ties with the HSECoE, which helps to guide goals and maintain relevance.
- The goal is to develop a liquid-phase $H_2$ storage material using ILs. This is fully consistent with DOE goals in this area and is important to making AB a practical storage material. There are close interactions with the HSECoE. The relationship with the HSECoE started early on, so this project is now well connected and integrated with the HSECoE. The target of 5 wt.% is a little low. The cost focus is good. The optimization of AB fuel blends for use in ILs is a good idea.

### Question 2: Approach to performing the work

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

- ILs have significant promise for use with AB. The project features good approaches of striving for simplified AB product structures and looking at a greater range of ILs.
- The approach to the project is sound—concentrating effort on two strategies to deliver materials with the desired properties. These strategies are based on reasonable scientific assumptions. With regard to specific formulations, it would be good to see a greater trend toward an understanding of core principles rather than what appears to be at the moment a trial-and-error approach.
- The overall approach is good. The integration with engineering and dealing with the cost up front is also good. The survey of the properties of AB in ILs is a good idea. The use of Boron Nuclear Magnetic Spectroscopy is a good idea, and the researchers have developed a good measurement method. There is an issue with how much $H_2$

is released at room temperature in the IL—it seems to be quite high. The work on minimizing impurities such as ammonia, borazine, and $B_2H_6$ is good. There is an issue with unidentified impurities, which can be way too high, based on the researchers' observations.
- This project is nicely focused on the key barriers for liquid carriers of this type: (1) keeping the solution/slurry in a liquid phase before and after hydrogenation and (2) finding the appropriate liquid to maximize capacity, rate, stability, etc.
- The principal objective is to develop AB in an IL form that will not precipitate solids during the onboard dehydrogenate reaction. This will greatly simplify the system. The work is designed to directly satisfy HSECoE needs. In addition to coming up with a stable liquid system, other important side considerations are being considered; for example, loss of effective $H_2$ capacity, stability, compatibility with regeneration, etc.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Interesting ILs and additives to ILs that optimize the use of AB have been identified.
- A number of accomplishments were achieved this past year that address the key issues. The demonstration that a slurry, formed from AB and a hexyl-AB (6 wt.%), transforms into a liquid after dehydrogenation is an important achievement. The ability to keep the fuel in liquid (or slurry) form is critical, and this is a nice proof of concept. It is nice to see stability measurements are being performed during the screening stage. It would be useful to expand these to higher temperatures (i.e., above room temperature). The project team has made good progress overall. It must identify and deal with the impurities in a better way. The use of binary materials with AB is a good idea. More details on the properties of the pure mixtures without ILs would be good. The researchers have the measurement method under control. It would have been good to have seen more details on the new additive synthesis. The researchers are going to have to deal with how much $H_2$ is released at room temperature. An interesting and important result is the difference in solubility of polyborazylene in ILs.
- Good progress has been made in developing new ILs for the solution of AB. However, it is difficult to understand the trade-offs involved. How much of the excellent $H_2$ capacity of AB is lost by dilution in a stable IL version. It appears that the 40% AB levels required by the HSECoE have not been met thus far. A problem is the presentation of the results in mols $H_2$/g liquid, instead of simply wt.% $H_2$. Volume dilution effects do not seem to be covered. It would be useful to clearly show that the best products to date still have the potential to meet the DOE system targets when built into an HSECoE demonstration system.
- Some progress has clearly been made, but there remains a lot to do. The reviewers-only slides indicate that there have been several factors that may have impeded progress this year, and it is understood that this is a difficult and complex problem. Data from only one AB/IL/additive blend was presented and, although improved over the non-additive control, the amount of $H_2$ released before the phase change is a long way short of the targets. Recognizing the water content of ILs is an advance in one sense, but it has probably consumed appreciable resources and calls many of the previous results into question. However, everyone recognizes the need to avoid boron-oxygen bonds, so hopefully further progress will ensue from the new understanding of the raw materials. It is good to see quantitative data on the impurities from these systems, and this is progress in a sense, even if the borazine quantities in particularly are alarmingly high. It seems that the fluid environment may encourage borazine formation, and this is intuitively understandable when considering the need for three AB-derived units to align correctly in the ring (solid AB starts with all molecules aligned in a single direction). It is possible that the additives under consideration will counteract this problem by forming less volatile species; nevertheless, this is an issue the project team needs to watch closely and consider alternatives to AB if necessary.

### Question 4: Collaboration and coordination with other institutions

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

- The collaborations are excellent. L. Sneddon of the University of Pennsylvania is transitioning to a consultant role. The project team is bringing on T. Baker from the University of Ottawa to deal with the catalysis issues. The team has excellent collaborations with the HSECoE.
- This project seems well integrated with the HSECoE, and there are a number of good external collaborations.
- It is unclear if there is any collaboration with the Pacific Northwest National Laboratory AB slurry project.

- There are a few useful collaborations in addition to the basic one with the HSECoE.
- The project has close ties with the HSECoE and is responsive to its needs. There have been extenuating circumstances with other collaborators, but those issues appear to be resolved. This PI should engage the university collaborators in selecting additives and ILs, respectively. The PI surely intends to do this anyway, but the importance of sound choices in these areas if the project is to make significant gains toward the targets must be stressed.

### Question 5: Proposed future work

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

- The future work seems reasonable.
- The proposed future work is essentially a continuation of the current work, incorporating new formulations to synthesize and test. While this is reasonable, a new or more focused approach may be justified if purity or capacity (before phase change) characteristics remain substantially short of targets.
- Although not very detailed, the general directions for the future work seem to be adequate.
- It is good to see that the solubility survey will be complete in fiscal year (FY) 2012. A number of key studies are necessary to complete this milestone. It is a little less clear exactly what will be done in FY 2013.
- The idea of using a reproducible source of polyborazylene is good for making comparisons between different IL formulations; however, it becomes less valuable if the solubility characteristics are different from the “real” dehydrogenation product. The exact AB/R-AB mixture and the thermal conditions used to generate $H_2$ will probably have a major influence on the structure and solubility of the spent fuel. The team should ensure that it does not get sidetracked too far by considering the solubility of what may be an unrepresentative material.
- The researchers have a good plan on where to go. They will continue to interface with the HSECoE. They need to get back to getting $H_2$ release rates. They also need to deal with regeneration issues. They did not specify much on the catalysis that they will be doing. They need to deal with the impurity issues as well as the release of $H_2$ at room temperature.

### Project strengths:

- This project is well aligned with DOE objectives and the work is sharply focused. Good progress has been made over the past year.
- The project team has significant expertise with AB and ILs.
- The project team has a good relationship with the HSECoE. The project team and collaborators are experts in this field.
- The researchers are making excellent progress on a good idea. The use of a liquid fuel system makes it more likely that some of the existing infrastructure can be used. There are close interactions with the HSECoE, and the project team is dealing with cost issues up front. The researchers have a lot of the experimental issues under control.
- This project’s strengths include its excellent chemistry, aims toward HSECoE data needs, and how it is helping the HSECoE’s work toward Phase III.

### Project weaknesses:

- The project has a strong flavor of trial-and-error at this stage; it is difficult to assess where breakthroughs to overcome barriers will emerge.
- The researchers must identify and deal with the impurities in a better way. They are going to have to deal with how much $H_2$ is released at room temperature. The project needs to provide more details on the new additive synthesis and catalysis. The project team needs to deal with regeneration issues and determine what the byproduct is on the release of $H_2$.
- Although the regeneration falls outside the scope of this particular project, it should be integrated in some way. A legitimate concern is that the IL may be incompatible with the regeneration effort. It would be nice to see the regeneration group(s) demonstrate compatibility with whichever IL is selected in FY 2012.
- There seems to be a distinct possibility that the limits to a dilution level to ensure a fully liquid system will result in missing the DOE weight and volume system targets.

- This project has no weaknesses.

### Recommendations for additions/deletions to project scope:

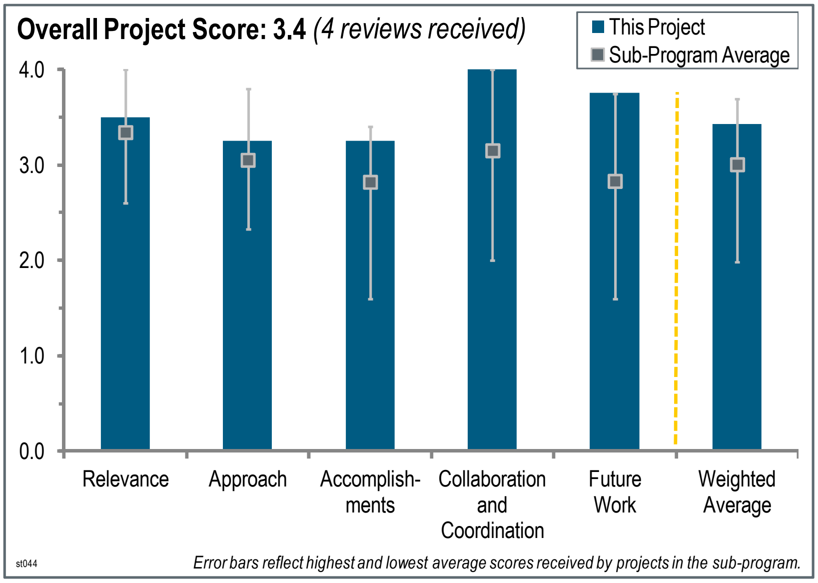
- The project team should not make any deletions.
- Successful storage materials, of course, require all performance criteria to be met. However, at this stage long-term stability tests are less important in this project than $H_2$ purity and maintaining suitable fluid characteristics over the required $H_2$ release range.
- Compatibility with regeneration efforts should be considered. It would be useful to collaborate with regeneration efforts elsewhere to determine the viability of this type of carrier. Stability measurements should be expanded—dehydrogenation should be investigated at higher temperatures. The project team should investigate how the liquid impacts kinetics (this may be done within the HSECoE). Impurities need to be better identified and ultimately reduced.
- The project team should accelerate interactions with the HSECoE and analysis groups to ensure that a practical and economic product will result from this work.
- This reviewer has no recommendations.

## Project # ST-044: SRNL Technical Work Scope for the Hydrogen Storage Engineering Center of Excellence: Design and Testing of Metal Hydride and Adsorbent Systems

Ted Motyka; Savannah River National Laboratory

### Brief Summary of Project:

The project's objectives for 2011–2013 are to: (1) develop innovative onboard system concepts for metal hydride and adsorption materials-based storage technologies; and (2) design components and experimental test fixtures to evaluate the innovative storage devices and subsystem design concepts, validate model predictions, and improve both component design and predictive capability.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This project is highly relevant to DOE objectives and works toward DOE goals by providing good engineering input to the Hydrogen Storage Engineering Center of Excellence (HSECoE).
- This project features development of innovative onboard system concepts for metal hydrides and adsorbents through systems modeling. The project eliminated metal hydride work and focused on adsorbent materials.
- This multifunctional project addresses virtually all aspects of the hydrogen ($H_2$) storage system in a comprehensive manner. As such, it generates and provides important information that is critical to the success of the DOE Hydrogen and Fuel Cells Program (the Program), and it fully supports DOE's research, development, and demonstration (RD&D) objectives and goals. With the decision to "divert" metal hydrides within the HSECoE, the presentation rightfully focused on one of the remaining leading candidate storage concepts—the cryo-adsorbent-based approach.
- The Savannah River National Laboratory (SRNL) project is an important component in the overall HSECoE technical effort. The project provides technical support to the HSECoE in the development of innovative system architectures and subsystem design concepts, as well as detailed modeling and validation of selected design concepts. The technical focus in 2011–2012 was on the development and validation of models for $H_2$ refueling and delivery in cryo-adsorbent systems. This effort directly supports the overall technical effort in the HSECoE and is relevant to the Program's RD&D objectives.

### Question 2: Approach to performing the work

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

- The approach used in this project embraces an excellent balance of data assimilation, modeling, experimentation, and system validation. Each task is sharply focused on a critical barrier to meeting DOE's 2017 $H_2$ storage system targets. A clearer picture of what it will take for cryo-adsorption-type $H_2$ storage to evolve into a system that meets all DOE performance targets is emerging from this work. The 2013 DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) presentation for this project is worth looking forward to.
- The technical approach is focused on experimental and engineering modeling of flow-through cooling, design of cryo-adsorbent systems, and modeling of cryo-system performance with variations in media packing and operating conditions. Prior work included the system analyses required to make an objective go/no-go decision

on metal hydride system approaches. A logical approach that addresses the important technical barriers faced by cryo-adsorbent systems for onboard $H_2$ storage and delivery applications has been adopted in this project. Metal-organic framework-5 (MOF-5) has served as a prototype system in these studies. It will be important to continually assess the status of new material development to ensure that MOFs or other cryo-adsorbers with improved storage/delivery characteristics can be implemented in a straightforward fashion.
- The technical barriers are carefully addressed in a systemic manner, especially system weight, volume, efficiency, and qualitative cost. The overall engineering approaches are sound. The electrical heating/flow-through cooling concept, testing, and analysis are interesting. The current work is almost exclusively on cryo-adsorbent systems. Although discussed briefly in the HSECoE overview, more on why reversible hydride activities have been abandoned would have been interesting from this project's perspective.
- This project involves a significant amount of work testing models against experimental data. The researchers made adjustments to the models to better fit the experimental data. It is assumed that the project team performed a sensitivity analysis to convince the modelers that they were changing the right parameters for the right reasons. If the "experimental error" in the experimental data was determined, some elaboration would have been helpful. Perhaps it is possible that the earlier models are fine, and that parameters were adjusted to meet an experimental outlier. The research team had an excellent idea to test a series of gases, specifically nitrogen ($N_2$), $H_2$, and helium (He). It would be good to know if the model predicts different outcomes for the different gases and, if so, whether the model, adjusted for $H_2$, can fit the observables for $N_2$ or He. If this is the case, it would be a great way to test the model. If not, then it is unclear what can be learned from measuring gases besides $H_2$.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Excellent progress has been made toward identifying and, to some extent, resolving issues that control or limit the performance of cryo-adsorbent-type $H_2$ storage systems. Slide 6 of the presentation at the 2012 AMR nicely summarizes how this comes together for cryo-adsorbent refueling and desorption. Slide 12 of the presentation contains an informative summary of modeling results for several alternative cryo-adsorbent approaches that includes a snapshot of where things appear to stand with respect to meeting DOE $H_2$ storage system performance targets. The nature and number of new experimental and modeling results revealed in the presentation gave evidence that a significant amount of productive effort was expended on this project over the past year.
- An impressive amount of work has been conducted in 2011–2012 on engineering modeling of the cryo-adsorbent systems, particularly on the flow-through analyses, dependence of system performance on variations in operating conditions and material characteristics, and improved $H_2$ refueling and delivery schemes. It would have been helpful to provide more detail concerning the assumptions that are included in the model, as well as a straightforward analysis of model "sensitivity" to the key parameters. Without that information, it is difficult to assess the validity of the models or be confident about their predictive capability in an operational environment.
- The results are technically excellent and do much to define the basic challenges in achieving a cryo-adsorbent system that will meet DOE targets. The MOF compaction work seemed to be very useful. The results seem to be trending toward really low temperatures (e.g., 40 K), further adding to concerns about the general operational practicalities of cryogenic storage. The biggest gap between reality and DOE targets seems to be the loss of usable $H_2$ (dormancy). This problem was discussed by the principal investigator (PI) in the question and answer session. It was pointed out that this is a major component of the Jet Propulsion Laboratory effort.

### Question 4: Collaboration and coordination with other institutions

This project was rated **4.0** for its collaboration and coordination.

- Strong collaborations are apparent for this project.
- Extensive and diverse collaborations, especially with numerous partners in the HSECoE, are ongoing, and those interactions are serving to significantly augment and leverage the SRNL project.
- The collaborations are excellent within the HSECoE. They are especially valuable with the University of Quebec Trois Rivieres (UQTR).
- Slides 4 and 21 of the presentation depict how this project fits into the larger HSECoE infrastructure. The project appears to be well coordinated with appropriate HSECoE partners, but only the National Renewable Energy

Laboratory, UQTR, Ford Motor Company, and Oregon State University are indicated as collaborators on the accomplishment slides. It would be interesting to know how all of the different modeling/simulation/validation studies mentioned from one partner presentation to the next actually fold together (one assumes they do).

### Question 5: Proposed future work

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

- The project team proposed several specific ideas that show thoughtful planning.
- The proposed future directions are very appropriate and will help complete the story.
- The future plans presented for this project do clearly build on past progress (particularly progress since the 2011 AMR) and are indeed sharply focused on mitigating barriers to meeting DOE's 2017 $H_2$ storage system targets. Surely some truly definitive findings will emerge from this project in the coming year with respect to cryo-adsorbent-based $H_2$ storage for fuel-cell-powered passenger vehicles.
- The future work is clearly stated and described in sufficient detail to allow reviewers to fully understand and appreciate the directions that will be taken in the future. The future plans represent a logical and compelling continuation of the present work and should provide useful information that will support the final system design. The decisions made by the PI and HSECoE management to conduct an objective evaluation of materials and down-select only a few promising candidates has allowed the project to focus more effectively on important technical barriers.

### Project strengths:

- SRNL is doing a commendable job coordinating many partners and providing technical contributions.
- This project has an excellent engineering team and collaborations. This is an excellent example of what contributions to a Center of Excellence should look like.
- This project's strengths include having a comprehensive, well-structured research and development plan; appropriate expertise and experience; and a realistic approach to defining system requirements, addressing critical (progress-limiting) issues, and validating system performance.
- The SRNL team has a solid understanding of system needs and technical barriers that apply to the development of $H_2$ storage and delivery systems and subsystems. The PI and his colleagues have expertise and a strong background in all engineering aspects of the experimental and modeling efforts in the project. Extensive collaborations with other partners in the HSECoE provide useful support for the SRNL effort.

### Project weaknesses:

- The most significant problem is that no single material meets the DOE targets. Consequently, the SRNL team is forced to develop a system based on less-capable materials.
- It seems to be a bit of a stretch that the practical complexities and costs of cryogenic storage systems will be justifiable over relatively simple, ambient temperature compressed $H_2$ storage.
- This project has no apparent weaknesses.

### Recommendations for additions/deletions to project scope:

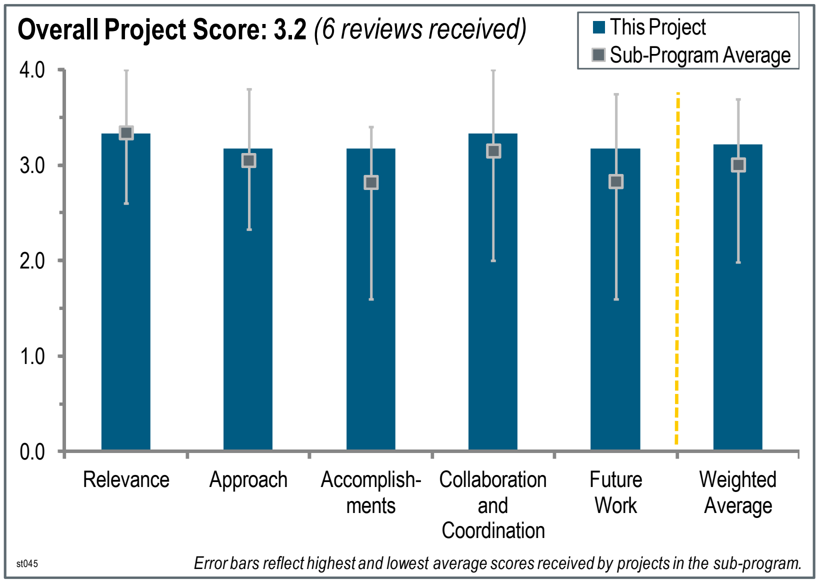
- The project scope is well formulated. Not much can be done to improve it at this time.
- The project team should get some preliminary metrics on cost in the near future.
- A more detailed sensitivity analysis is needed in the modeling work in order to provide confidence in the accuracy and validity of the predictions derived from the model. It will be important for SRNL to be flexible with its engineering architecture and design efforts in order to be able to effectively incorporate improved materials that may emerge from other studies.

# Project # ST-045: Key Technologies, Thermal Management, and Prototype Testing for Advanced Solid-State Hydrogen Storage Systems

Joseph Reiter; NASA Jet Propulsion Laboratory

## Brief Summary of Project:

The objectives of this project are to: (1) identify state-of-the-art concepts and designs of hydrogen ($H_2$) storage systems; (2) discover and identify technical barriers to system development; (3) develop means and/or identify trajectories to overcome barriers; (4) describe and develop enabling technologies toward achieving targets; and (5) design, build, and test hardware components for model validation.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The ability to accomplish the objectives of this project is critical to a key candidate $H_2$ storage pathway for transportation.
- The project is addressing several technical aspects related to cryogenic storage that could support efforts to meet DOE goals and objectives.
- This project is relevant to the objectives of the Hydrogen Storage Engineering Center of Excellence (HSECoE). The Jet Propulsion Laboratory's (JPL's) work scope is consistent with the needs for designing a cryo-absorbent onboard storage system
- JPL has redirected part of its effort to address important cryo-system engineering issues. All aspects of the project (as it is currently framed) are well aligned with the DOE Hydrogen and Fuel Cells Program goals and fully support DOE research, development, and demonstration (RD&D) objectives.
- The JPL project is generally well aligned with the HSECoE goals and the DOE RD&D objectives. JPL has extensive experience and expertise in cryogenic system modeling, design, and testing for spacecraft applications. That experience is directly relevant to the present work on cryo-adsorbent systems for $H_2$ storage and delivery.
- The JPL role, developing cryo-pressure vessel technology, is very relevant to HSECoE efforts to develop a viable solid-state $H_2$ storage system. Because it appears that an ambient temperature system will not be an option going into Phase III, a low-temperature system will be required to enable a sorbent-based system that can be capable of meeting DOE onboard requirements.

## Question 2: Approach to performing the work

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

- JPL's approach is to validate model results with coupon experiments and apply the model to the engineering design of system components. This is a sound approach toward achieving the objectives defined in Phase II. The approach lays the groundwork for Phase III prototype testing. JPL has the unique capability to perform burst-testing of Type I and Type IV pressure vessels at cryogenic temperatures.
- Each task is directed at one or more critical barriers to meeting $H_2$ storage system performance targets. The work done in the past year and proposed for the coming year on cryo-system design, modeling, and testing couples nicely with efforts at Lincoln Composites and Pacific Northwest National Laboratory. The emphasis on

insulation, outgassing, heat transfer, and durability is highly appropriate and much needed as prototype testing and concept validation become increasingly important aspects of the overall HSECoE program.
- JPL's focus has shifted to novel cryogenic technology development and implementation as the HSECoE has evolved and the reversible metal hydride development has been stopped. JPL is no longer the system architect for the cryo-sorbent system; that role has been assumed by Ford, which brings the perspective of an original equipment manufacturer end-user into clear focus. The remaining JPL tasks are well focused on resolving the issues of advancing thermal isolation designs, measuring outgassing from the pressure vessel, investigating downstream heat exchanger design, and pursuing cryo-burst testing of pressure vessels.
- There is a good and appropriate mix of modeling and experiments. One concern regarding the outgassing task of characterizing the impact is that it is a function of temperature, gas pressure, and the gas species. It is unclear if there is a way to understand the species' evolution as a function of temperature, life, and species partial pressure. The refocusing of the project and this investigator should aid in the timeliness and quality of the results at the end of the project.
- The approach is technically sound, but it could use some performance-related refinement, particularly in better understanding the product requirements for dormancy/venting. The objective of the cold composite pressure vessel burst is not clearly stated.
- The technical approach comprises work on cryogenic vessel insulation, vessel outgassing, design and testing of heat exchangers, and burst testing of cryo vessels. These tasks are important elements in the overall engineering effort on cryo-adsorbent systems in the HSECoE. The approach is logical and focuses on the technical barriers that confront the engineering development of an efficient cryo system. The de-scoping of the metal hydride effort in the HSECoE required a nimble transfer in technical direction by JPL. This was handled in a straightforward and timely way. The connection between an important aspect of the technical approach in 2010–2011 and the effort reviewed this year is confusing. In prior years, the principal investigator (PI) served as the "system architect" for the cryo-adsorbent system design process. In that role, he was responsible for coordinating engineering efforts across the HSECoE—an important and time-consuming task. However, beginning in 2011 it seems that role was assumed by D. Siegel (University of Michigan); in other words, the JPL PI no longer serves in that capacity. The presentation does not clarify that situation, and it does not discuss the revisions to the roles and responsibilities that accompanied that transition.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- JPL appears to have taken a leadership position in the design, development, and testing of cryo-adsorbent-type $H_2$ storage systems. Testing and validation studies are providing critical design data and seminal insights concerning cryo-vessel performance. Some encouraging performance improvements were reported. Inconsistencies in outgassing results need to be ironed out. It would be interesting to know what gases are contributing to the pressure rise, and whether this effort relates to permeability studies being done by other HSECoE partners.
- Considerable progress was made in 2011–2012, especially in the area of downstream heat exchanger design and modeling. Important new results were also obtained on simulation and modeling of dormancy in a cryo subsystem. That understanding is critical to fully validating the system-level thermal performance in practical system operating conditions. Also, the development of a burst-testing chamber that was started in 2011–2012 will provide an excellent test facility for use in future HSECoE work.
- The heat gain reduction effort looks promising. It would be helpful to better understand what level of heat gain is acceptable—liquid natural gas storage products and characteristics could provide some guidance. Vibration tolerance, not just shock, needs to be evaluated for the Kevlar suspension system. In the downstream heat exchanger work, the extent of supplemental heating needs to be quantified.
- Despite the reduction in funding, progress since the last DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) has been good. The advanced Kevlar suspension system model has been validated and shows almost a 40% performance improvement over state-of-the-art systems. Outgassing experiments have been started, but the results have been somewhat inconsistent. More work is needed here to determine if outgassing will have long-term detrimental effects on the thermal isolation properties. Also, the species that are evolved need to be identified and a mitigation strategy needs to be developed. The cryo-burst facility design, which will

have the capability for 15,000 pounds per square inch burst of <20 L composite over wrapped pressure vessels at 77 K, has been completed.
- Modeling results showed a 38% reduction in parasitic loss for JPL's advanced thermal isolation design, which is welcoming news. However, for practical application, a three-dimensional dormancy is probably not acceptable to the consumers. JPL has developed a facility for cryogenic burst-testing of pressure vessels at 77 K and 40 K. Carbon fiber overwrap test vessels (6-L) have been manufactured by Lincoln Composites for burst testing at JPL's facility. JPL should consider extending the facility to collect fatigue data for carbon fiber composites (coupon size) at cryogenic temperatures.
- The project team has achieved a very nice design and build of the cryo vessel and the reduction of thermal conductivity using the Kevlar webbing. Visually (slide 9), concerning the loading and coupling of the suspension web between the tank and jacket, it looks like there may be an issue with a challenging vibration load with the somewhat complex structure. The dormancy general behavior is expected, but it is good to see some quantification from the model. While new tests are needed with the new system, the initial outgassing tests imply that there can be significant gas evolved if a vessel is allowed to reach the end of its dormancy period and reach close to the ambient temperature. Once cooled down again, the pressure will decrease, but with the increase of mass in the gas phase, the researchers should look at that effect. It would be interesting to know if there are any components of the composite overwrapped pressure vessel (COPV) that reach equilibrium partial pressures at the cryo temperatures.

### Question 4: Collaboration and coordination with other institutions

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

- The clearly identified collaborators are qualified and contributing.
- JPL has close interaction and collaboration with members of the HSECoE and frequent participation in Storage Systems Analysis Working Group meetings.
- While the partners were listed and there was mention and a slide referring to the collaborations, the degree was not clear in the presentation.
- Numerous collaborations and interactions with multiple partners across the HSECoE are effectively leveraging the impact of the JPL project. The coordination of this project with the "system architecture" effort (University of Michigan) could be defined in greater detail.
- The presenter provided a very clear picture of how the JPL project fits into the overall HSECoE program and outlined the specifics of the relevant connections with other HSECoE partners. DOE and HSECoE have developed a set of "specific, measurable, achievable, relevant, and timely" (SMART) milestones to "align" and "coordinate" technical work in Phase II. This should ensure a well-integrated effort throughout the HSECoE. Outgassing and permeability studies across the HSECoE should be carried out in a correlated manner.
- The collaborations with the other HSECoE partners are very good. The good communication between the team members developing sorbent-based systems has enabled good progress in this area. Additional collaboration with Lawrence Livermore National Laboratory (LLNL) is encouraged to take advantage of the knowledge gained with the cryo-compressed storage system.

### Question 5: Proposed future work

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

- Future work is well defined for the three main tasks. The cryogenic burst-testing facility is a nice addition to the portfolio of the HSECoE.
- The future work is clearly identified and appropriate. More detail on schedule and decision points would be helpful.
- The presenter did an excellent job of laying out the future plans. The illustrations made it clear that JPL is ready to execute its plan. The modeling tools and test facilities are in place (or close to being in place). JPL has positioned itself as a key participant in the Phase III up-select process for the cryo-adsorbent concept.
- The future work represents a straightforward continuation of the 2011–2012 effort. Well-formulated milestones are in place, and the work addresses important technical barriers that remain in the development of an operational cryo subsystem tailored for onboard vehicle applications.

- The plans for future work look well thought out. The last task listed in the "Thermal Isolation" task could wind up being very important to the success of cryo pressure vessels in general. Scaled dormancy modeling and testing is also very important.
- The future work plans align well with the HSECoE objectives. However, there is some uncertainty regarding the role of JPL beyond 2012. If a cryo-sorption vessel is built in Phase III, it is unclear where it will be tested. There is also an opportunity for further collaboration with LLNL.

### Project strengths:

- The project is generally well thought out, well executed, and covers important technical areas and barriers.
- The project has achieved overall solid progress on diverse activities.
- The project has a highly qualified team with extensive experience in a wide range of cryo system applications. All of the engineering efforts in this project directly support the overall goals of the HSECoE.
- JPL is focused on areas of its expertise. The team is well qualified and knowledgeable, and its capabilities for experimental validation appear to be very good.
- JPL has strong expertise in the area of thermal management modeling and testing. The new cryogenic burst testing facility and composite material outgassing facility are unique capabilities for DOE's Hydrogen Storage sub-program.
- Expertise and facilities are well suited to the barriers being addressed. The cryo-adsorbent system development effort across the HSECoE appears to be well organized and closely coordinated. JPL seems to be playing a key role. The JPL project presentation at the 2012 AMR was one of the best—clear, crisp, and concise.

### Project weaknesses:

- Although much work has been devoted to improving the thermal insulation for cryogenic tanks, it does not appear that a viable solution to meeting DOE targets and consumer acceptance for dormancy is within reach.
- A minor issue is that it is not clear whether the engineering work on this project is sufficiently general to encompass new adsorbent materials that may emerge in future work.
- It is not clear if any consideration has been given to cost. It may not be part of JPL's scope, but some rough estimate should be considered to determine if a cryogenic system has any chance of being viable.
- The project team should assess potential COPV manufacturing and/or treatment approaches to mitigate outgassing effects.
- There are no apparent weaknesses in this project.

### Recommendations for additions/deletions to project scope:

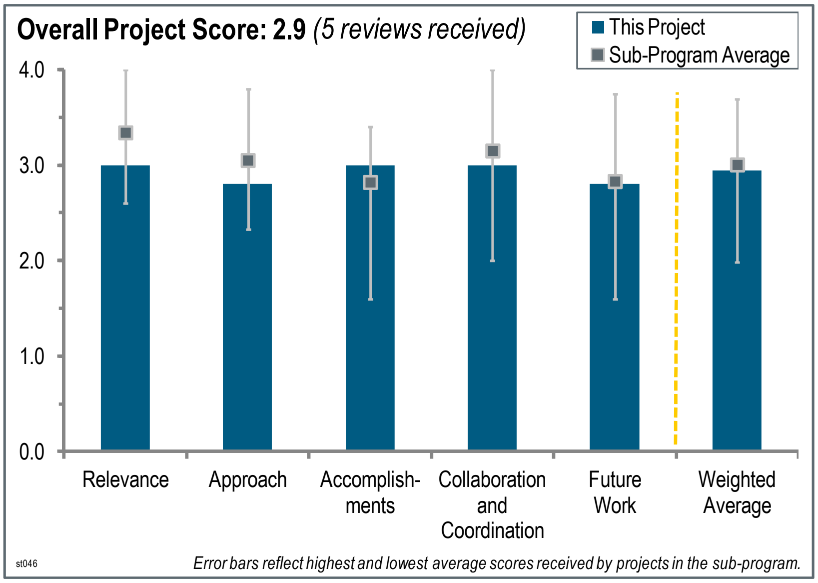
- Researchers should extend the cryogenic burst testing facility and procedure to allow the collection of fatigue data for T700 carbon fiber at 77 K and 40 K.
- The project scope looks fine as presently formulated.
- The project scope and level of effort seem fully consistent with the needs of the HSECoE. Clarification of the perceived change in the "system architect" role would be helpful.
- This project should expand its interactions with LLNL. Much of the development that has gone into the cryo-compressed system could benefit the HSECoE cryogenic systems work, particularly in the area of dormancy.
- The project team should pursue vibration and shock testing for cryo tank model validation. Unfortunately, this requires an additional tank to be built, or testing the current one prior to burst testing. The team should also get fatigue data on Kevlar in this webbing configuration. It would not be good if a thermally perfect cryo pressure vessel for cryo-adsorbents is created and performs well in static environments, but fails in shock, vibration, or fatigue environments. Perhaps the researchers can do an outgassing test with an inner chamber a few percent volume more than the sample and see if there is an equilibrium pressure as a function of temperature. Perhaps passivation of COPV is a possibility for a solution for outgassing. The project team should consider gettering as a purification method a little more. If there is a way to understand the outgassing overwrap component evolution as a function of temperature, service life, and species partial pressure, that could lead to new mitigation options.

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

Kevin Drost; Oregon State University

### Brief Summary of Project:

The objective of this project is to use the enhanced heat and mass transfer that is available from arrayed microchannel processing technology to: (1) reduce the size and weight of storage systems, (2) improve the charging and discharging rates of storage systems, and (3) increase the performance of thermal balance-of-plant (BOP) components. This project addresses the barriers of reducing system size and weight, charging and discharging rates, and BOP.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The project has relevance if it improves performance.
- Maximizing heat transfer effects is critically important to sorption-based hydrogen ($H_2$) storage systems that operate at liquid nitrogen ($LN_2$) temperatures.
- This project deals with heat and mass transfer for $H_2$ storage within the DOE Hydrogen Storage Engineering Center of Excellence (HSECoE). The project is directed toward applying microchannel technology to reduce system size and weight, enhance charging and discharging rates, and reduce BOP complexity. The project is relevant to stated DOE $H_2$ storage goals and objectives.
- The major objective of this project is to use microchannel technology to enhance heat (and mass) transfer in a cryogenic adsorption $H_2$ storage system to permit rapid refueling of the media using $LN_2$ as the coolant. Using the microchannel approach, the additional weight and volume of the inserts is projected to be quite small (but not negligible, perhaps about 10%). This appears to address a relatively minor issue in the big picture of using off-board $LN_2$ for media cooling in the cryogenic adsorption option. A second objective is to design and test a microchannel combustor-recuperator-heat exchanger for $H_2$ during discharge. This would be a compact add-on to a conventional heat exchanger that would add only slightly to the weight and volume of the $H_2$ storage system. Although mass transfer enhancement was mentioned in the presentation, there was no specific example given.

### Question 2: Approach to performing the work

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

- This project features a unique approach toward improving tank design.
- The project is sharply focused on the key issues. The stacked disc-microchannel approach is a good one. The use of aluminum rather than stainless steel is a key aspect.
- The project approach focuses on identifying and prioritizing opportunities for applying microchannel techniques. Novel concepts have been suggested for microchannel applications supported by subsystem prototype fabrication and testing for performance validation.
- The approach is to identify and prioritize opportunities for use of microchannel techniques in $H_2$ storage systems. Modeling, design, and testing for those opportunities is then conducted. The concept is to optimize the design and performance of a single unit cell, which would then be “numbered up” to the necessary full-scale

performance. One obvious question this approach raises is that of the effects of flow maldistribution among the multiple stages. This aspect was not adequately discussed. The system concept shows a 30-cm diameter "hockey puck." Because there was no discussion of the size needed for a full-scale system of 5.6 kg $H_2$ capacity, it appears that the full-scale system would also use 30-cm diameter sorbent pucks. This would likely lead to very high length-to-diameter ratios for the storage tank and potentially lead to high flow maldistributions.
- It is not clear whether this effort is fully integrated with the HSECoE. It would be good to see modeling efforts that show how much modular adsorption tank inserts (MATIs) improve fueling time for systems. The design seems to be unique to the hockey puck design, which may be too slow due to limited permeability. It would be good to know how it would work with the packed bed designs that the General Motors (GM) work suggests work well. Mass transfer limitations do not appear to be considered. The combustor is interesting, but earlier investigations have shown that catalysts deposited on microchannel walls are not durable enough for long-term applications. The investigators show their goals, but the baseline performance without the MATI is not clear, and it is not clear how much the MATI would improve the performance over the baseline system design.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project has made significant improvements on the design since last year.
- Significant progress has been made on system designs and experimental verifications of system designs.
- Significant progress has been made in developing the MATI design concept for cooling and $H_2$ distribution. Performance and cost modeling have been accomplished based on volume manufacturing. Preliminary testing has been done on an adsorbent test bed in an effort to validate design concepts.
- The project has completed the initial feasibility study of the MATI and experimental validation of the oil heater design (it is not clear how this second item is related to the cryo-adsorption storage system concept). Process-based cost modeling is being conducted. The project is also exploring options for cost reductions. The significance of the results of the room temperature filling experiments is not readily evident. Laser welding of 316 stainless steel and 6061 aluminum plates has identified process issues that are being addressed. All of the components to be tested have been designed and fabricated.

### Question 4: Collaboration and coordination with other institutions

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

- The project features an excellent set of collaborations that provide expertise from users of the technology.
- Extensive collaborations with HSECoE partners have benefitted this project substantially.
- This project is part of the HSECoE, which has multiple team members from industry, national laboratories, and universities.
- Although this is a different approach, collaborating with members (e.g., GM) working on heat management of the cryo-tank is recommended.
- It is not clear whether HSECoE modeling efforts are being applied to the MATI.

### Question 5: Proposed future work

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

- The future work looks reasonable.
- The future plans basically involve a straightforward continuation of the various tasks currently underway.
- The proposed fiscal year 2012 and future work is consistent with the objectives of the project.
- Investigators say that the loss of physical integrity of the "hockey puck" will not be detrimental to system performance. It would be nice to see that assumption verified experimentally.
- It would have been helpful to see what the weight and volume of this tank system is (based on modeling optimization) compared to a standard baseline tank, in order to establish an apple-to-apple comparison, which would ultimately help down-select the best tank design. For the combustor recuperator work, it is highly recommended that this project consider other non-precious-metal catalysts in future designs.

### Project strengths:

- This project has an excellent approach.
- The project features a strong, dedicated team and beneficial collaborations.
- A strength of this project is its novel concept.
- The experience in fabrication of microchannel devices is a strength of this project.
- The project has good collaboration and good modeling and model validation components. Another strength is the identification of important issues in the design and implementation of the project approach.

### Project weaknesses:

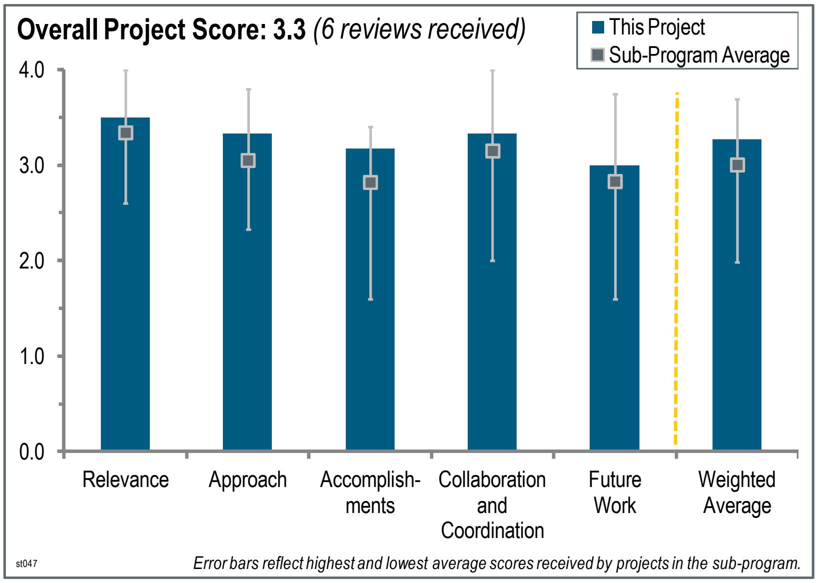
- The benefits of the MATI design are not as obviously quantitative as compared to others tank designs.
- As with other HSECoE projects, no attention is given to the forecourt implications of this technology.
- The joining of aluminum is a key aspect that may prove difficult.
- The proposed possible use of $LN_2$ means that there is a second cryogenic fluid onboard—this should be avoided if at all possible. Microchannel devices have been known to have fouling, plugging, and clogging problems under certain operational conditions. Likewise, catalyst delaminations in the channels in microchannel combustors and reactors have been a problem in some situations. Thorough prototype testing and validation of the candidate components and subsystems should be conducted as a part of this project. Compaction of the adsorption material in the "hockey puck" may damage conduction fins, if used.
- Manifolding and flow maldistribution in the multiple-stage MATI should be analyzed and addressed. The interface with the pressure vessel should be addressed because inserting the proposed assembly into a cryo-compressed $H_2$ storage vessel will likely not be a simple matter.

### Recommendations for additions/deletions to project scope:

- The project team should establish the benefits of the MATI design quantitatively (using modeling optimization), as opposed to the test bed experimental approach.
- The researchers should consider long-term testing of the combustor to verify catalyst integrity.
- In addition to laser welding of aluminum, the project team might also consider looking into friction-stir welding. The team might want to look at aluminum cellular materials for increasing the conductivity of the hockey puck.

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

Norman Newhouse; Lincoln Composites




## Brief Summary of Project:

The objectives of the project are to: (1) meet U.S. Department of Energy (DOE) 2017 hydrogen ($H_2$) storage goals for storage systems by identifying appropriate materials and design approaches for the composite container; (2) maintain durability, operability, and safety characteristics that meet the DOE 2017 targets; (3) work with Hydrogen Storage Engineering Center of Excellence (HSECoE) partners to identify pressure vessel characteristics and opportunities for performance improvement in support of system options selected by HSECoE partners; and (4) develop high-pressure tanks as required to contain components and materials of the selected $H_2$ storage system and operate the tanks safely and effectively in the defined pressure and temperature range.

## Question 1: Relevance to overall DOE objectives

This project was rated **3.5** for its relevance to DOE objectives.

- The efforts in the project are important for a number of storage technologies.
- Lincoln Composites (LC) is developing Type IV vessels for materials-based systems and looking for vessel characteristics and opportunities for performance improvement and cost reduction. The work is relevant to DOE's goal of reducing the weight, volume, and cost of onboard $H_2$ storage systems.
- The tank work should be applicable to a wide variety of medium-pressure storage technologies.
- This project is very relevant to the HSECoE program. LC is investigating methods to reduce the costs of composite overwrapped pressure vessels, which will likely be the near-term solution for compressed $H_2$ storage on board vehicles. LC is also cognizant of the possibility of incorporating sorbent-based materials in the tanks and the possibility of operation at cryogenic temperatures.
- The outcomes of this project are progressing well toward the DOE objectives. This work appears more focused on utilizing existing technology and applying that science to the objective as opposed to creating novel technology. This lends this work to quicker commercialization, and therefore the work is in line with the objectives.
- This project is absolutely essential for meeting the DOE goals for an $H_2$ storage system for many well-established reasons. In the first place, pressurized $H_2$ gas (with or without adsorbent media) may end up being the best methodology when all Hydrogen Storage sub-program targets are considered collectively. Secondly, pressure vessels are likely to be required for sorbent-type systems, and possibly for metal hydride or chemical $H_2$ storage materials as well. Issues of cost, weight, durability, and safety are paramount for the pressure vessel. LC is clearly locked in on all of these issues.

## Question 2: Approach to performing the work

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

- The work plan is designed to appropriately assess the major concerns associated with improving pressure vessel performance and cost.
- The principal investigators are taking a systematic approach to optimizing the cryo-tank.
- The project shows good integration between the collaborators, with each collaborator having a distinct and well-framed role. Consequently, the approach is logical and seems to follow a good flow with a focus on the objectives.
- Having established the baseline design in Phase I, LC is evaluating alternate designs and alternate materials (fiber, resin, liner, and boss) in Phase II to improve vessel performance and reduce cost.
- The approach that LC is taking in this project (i.e., slides 4–6) is well thought out, sharply focused on progress-limiting issues, and fully directed at exploring the best pathways to meeting $H_2$ storage system performance targets and costs. Norm Newhouse's presentation was excellent and left no doubt that the pressure vessel is presenting some daunting challenges that are being skillfully addressed. The research and development approach is based on consensus input from all HSECoE partners, as it should be.
- The approach by LC is well organized and is taking well-thought-out pathways for improvements in tank design and cost. The approach has been modified to investigate alternatives to Type IV tanks in response to comments from the 2011 DOE Hydrogen and Fuel Cells Program Annual Merit Review. The Phase I approach evaluated materials for cost and weight reduction as well as an increase in internal volume. Tank designs and materials were evaluated against operating requirements that were provided by the HSECoE team. The Phase II approach is to confirm operating conditions, select a baseline design and materials, evaluate alternatives, and fabricate bench-top test vessels. A common test vessel will be supplied to HSECoE partners to save time and cost for the project.

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- A large number of Type IV tanks have been fabricated and made available to the HSECoE.
- Given that in some instances LC's pressure vessels are only a part of a larger storage system, it is not simple to assess the company's direct contribution toward overcoming barriers. Nevertheless, LC is executing important technology assessment and validation work in support of the HSECoE.
- This project is meeting the necessary objectives and appears to be online for setting the stage for success in the subsequent phases. More effort, however, could be expended in looking further into the future to optimize the results beyond the current technology. Key accomplishments include resin characterization, even at cryogenic temperatures, and the manufacture of pressure vessels.
- The project team fabricated 21 six-liter test vessels (200-bar operating pressure)—three were used for burst tests, three were used for testing at cryogenic temperatures and leak tests, and 15 were made available to the HSECoE. Phase II test vessels were improved from Phase I baseline designs: 11% lower weight, 4% larger volume, and 10% lower cost. A significant portion of the improvement was attributed to reducing the carbon safety factor to 2.0 from 2.25.
- The lower safety factor, however, is not consistent with current guidelines established in SAE documents. The team also improved the winding technique to reduce peak stress near the boss opening, and cold-tested two vessels at 108 K and 205 bar for two cycles each. The number of cycles needs to be significantly higher to characterize fatigue behavior.
- Excellent progress has been made in the past year. The design basis vessel is well conceived, the level of analysis presented at the review was impressive, and the opportunity to actually hold on to a Type IV test vessel gave genuine encouragement that the project is moving forward into real systems. In addition to having made a sizable number of Type IV vessels for testing within the HSECoE, good progress appears to have been made in the area of materials development for the vessel. Modest improvements in terms of system weight, available storage volume, and cost were also reported.
- Accomplishments have been good in 2011. The test vessel criteria have been established based on consensus input from HSECoE partners. A baseline test vessel design has been established and 21 test vessels have been

fabricated. To address the reviewer comments from 2011, alternative all-metal and metal-lined composite designs were also prepared. Characterization of the behavior of the tanks at cryogenic temperatures is also planned. Preliminary results were shared for cold vessel testing that showed no effect on room-temperature burst properties. Preliminary designs for tanks that contain sorbents have also been considered.

### Question 4: Collaboration and coordination with other institutions

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

- The project features close collaborations and interactions with the other members of the HSECoE.
- There is good collaboration between the HSECoE and LC.
- This effort is supporting several other HSECoE projects, and the collaborations appear to be well coordinated.
- All parties seem to understand their role and have their roles defined well enough that there is little overlap, resulting in an efficient project.
- The HSECoE is doing an excellent job of ensuring that projects such as this one are well coordinated and integrated with the other partnering institutions. Frequent communications within the HSECoE partnership and timely meetings with the Tech Team are ensuring that design-related input is being transmitted in an effective manner.
- LC has established good collaborations with other team members to establish design criteria for test vessels and to consider alternative tank designs and materials going forward into Phase III.

### Question 5: Proposed future work

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

- The future work is well planned. The project team should conduct more cycling tests and quantify the effect of the thermal expansion coefficient mismatch between different materials.
- The future work is reasonable and well defined. Further detail regarding schedule and success metrics would be helpful.
- The future plans do indeed build on past progress and will remain sharply focused on barriers. This project is orchestrated so that transitions from Phase I to Phase II and subsequently onto Phase III should occur seamlessly. It looks like LC has built enough flexibility into its approach to accommodate a reasonable range of design revisions based on future input from the other partners in the HSECoE.
- The future work plans are logical and flow from the results of previous activities. Efforts will focus on further design trade-offs and the closure of ongoing efforts in vessel characterization, alternative materials evaluations, and vessel designs.
- The future work looks to overcome a couple of key obstacles, such as optimization of the liner, validating the pressure vessels at cryogenic temperatures, and the ability to insert key components into the liner. These are excellent and necessary tasks to complete. However, resources could be spent looking at what the future $H_2$ storage equipment could look like in a novel approach instead of conforming and improving current technology to fit the DOE goals.

### Project strengths:

- This project features a strong team.
- LC has substantial experience in developing and manufacturing Type IV tanks.
- The program is well thought out and addresses several important aspects of pressure vessel design and improvement.
- LC has the right experience, expertise, and resources to conduct this project in an effective manner. The tight collaboration structure within the HSECoE should ensure that the final pressure vessel design will be as close to optimum as possible.
- LC is a commercial supplier of Type IV tanks and composite materials and their experience should provide a realistic approach to cost estimation based on real-world practice. There were significant accomplishments in the past year. The work contributes to the efforts of other team members in materials-based systems and well as to

improving the picture for compressed storage. The funding for LC was increased substantially in 2012, enabling progress at a faster pace.

- This project is led by competent and experienced leaders and collaborators. This is especially true with the LC team; it is the right company to lead this effort. The team was able to manufacture the pressure vessels associated with the full storage system and complete cryogenic testing of the resin system. These are significant accomplishments pertaining to the ability of this project to reach commercialization.

### Project weaknesses:

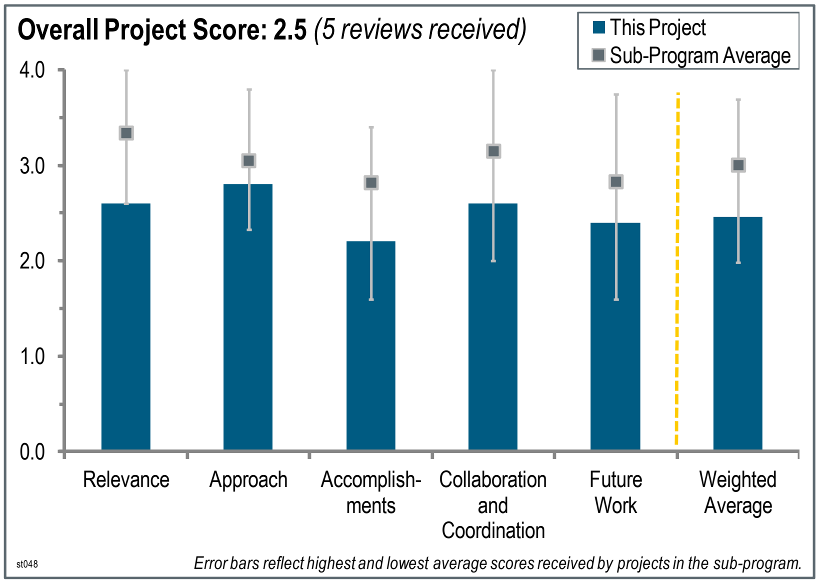
- This project has no apparent weaknesses.
- The amount of money expended toward this project appears to be a bit high in accordance with the accomplishments. Also, the efforts to optimize resin or fiber materials, or to characterize the desired properties of such materials, appear to be missing. With the volume potentials that this project and the other $H_2$ projects hold, material suppliers could be part of the collaboration to see if the ideal materials are possible.

### Recommendations for additions/deletions to project scope:

- Investigators should involve polymer and resin manufacturers in work aimed at improving these materials.
- There is no need to change or modify any aspect of the scope of this project.
- LC should conduct pressure cycling tests for up to 1,500 cycles to meet SAE guidelines. The project team needs to quantify the effect of the coefficient of thermal expansion mismatch between different materials (fiber/resin, liner/fiber, boss/liner).
- Compatibility studies between vessel materials and storage materials that may be placed in the interior of the vessel are encouraged. There is the possibility for cooperation between the HSECoE and the Lawrence Livermore National Laboratory cryo-compressed team. If cryo-capable tanks are fabricated, a direct comparison of the capacity of a cryo-compressed system and the best available sorbent-based system could be made.
- New technologies are ever emerging in thermosets, thermoplastics, and fibers that could be an asset to this project. As a parallel effort within the collaborations established, time could be spent on identifying and determining the potential of new technologies. However, with such a good and practical start, the project should not get distracted by new technology and should continue its focus as currently developed.

## Project # ST-048: Hydrogen Storage Materials for Fuel Cell Powered Vehicles

Andrew Goudy; Delaware State University




### Brief Summary of Project:

The objectives of this project are to: (1) identify complex hydrides that have the potential to meet the U.S. Department of Energy's (DOE's) goals for hydrogen ($H_2$) storage and demonstrate the optimum temperature and pressure ranges under a variety of conditions; (2) improve the sorption properties of systems that have been identified as good prospects for $H_2$ storage; (3) determine the cyclic stability of new materials and develop strategies for improving reversibility; (4) perform kinetic modeling studies and develop methods for improving kinetics and lowering reaction temperatures, thereby reducing refueling time; (5) extend the studies to include other complex hydrides that have greater $H_2$ storage potential; and (6) improve the rate at which the $H_2$ gas can be charged into a hydride-based $H_2$ storage tank, as well as improve the $H_2$ storage density.

### Question 1: Relevance to overall DOE objectives

This project was rated **2.6** for its relevance to DOE objectives.

- The development of improved metal hydride systems that can meet the DOE targets is important.
- The project is largely relevant to DOE goals and targets, at least in terms of mass, volumes, and rates. Some targets are not much considered, such as $H_2$ impurities.
- Many of the reviewer comments made last year could be applied to this year's presentation. A number of the complex hydride systems that are being investigated are of very limited interest to the DOE Hydrogen and Fuel Cells Program (the Program) goals, given the high temperatures required for solid-state diffusion. Whether phase boundary or diffusion limited or something in between, a careful mechanistic evaluation would be in order. While some work has been performed on the lithium amide/magnesium hydride ($LiNH_2/MgH_2$) system that was suggested last year, with some apparent improvement to kinetics, the lack of detail in describing the reasons for kinetic improvement is problematic.
- This project addresses the behavior of a small group of complex metal hydrides or their mixtures (e.g., $Mg(BH_4)_2$-$Ca(BH_4)_2$, $MgH_2$-$LiBH_4$) with theoretical $H_2$ capacities that could meet DOE vehicle performance targets under ideal conditions. However, all of the systems evaluated during the life of this project have severe limitations due to poor kinetics, too low desorption pressures at practical temperatures (i.e., <473 K), or irreversibility after $H_2$ desorption. None of the systems is currently considered viable within the DOE Vehicles Technologies Program, although some might be appropriate for other early market fuel cell systems. However, there have been a number of published results by others in the international literature on essentially all of the materials being studied in this project.

### Question 2: Approach to performing the work

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

- Many of the proposed reactions are well studied and not new. It was not clear if the principal investigator (PI) is aware of the considerable literature on previous work.

- The constant pressure ratio thermodynamic driving force approach is good for doing comparisons of different metal hydride materials. The research should concentrate on the most promising advanced metal hydride system identified to date and analyze it in more detail, particularly with regard to the understanding and optimization of additives on the metal hydride capacity and kinetics. Modeling of reaction pathways has been incorporated.
- The presentation of spectroscopic data, including X-ray diffraction (XRD), nuclear magnetic resonance (NMR), and other techniques that might identify product phases, should have been included. With such a limited set of data presented, essentially showing only high-temperature dehydrogenation results with little indication of possible cyclability, the value of these systems, even as high-temperature reversible systems, is unclear. Part of the task 1 approach is to use XRD for phase identification. In the absence of knowing reaction products, there is no way of knowing the value of the systems that have been studied.
- Reactions (nearly always desorptions) of chosen hydrides were monitored by conventional volumetric/gravimetric methods either during heating ramps or isotherms. An emphasis was to produce kinetic data at a fixed free energy (i.e., constant pressure ratios) to extract time constants and presumably parameters such as activation energies, although none were obvious in the presentation. Only limited phase identification or characterization was apparently attempted (mainly via powder XRD), although some solid-state NMR was alluded to in the DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) presentation package, resulting in little insight into reaction pathways. Due to the properties of the selected hydrides, nearly all reported experiments were done at temperatures around 673 K, which is much too high for vehicle applications, and extrapolations were not made to more practical temperature regions. This is a definite limitation of scope for the project.
- The project looks at some relatively classic systems, both neat hydrides and destabilized mixtures. The data supports other DOE-funded projects. The very important property of kinetics is studied in detail and in a correct manner, scientifically—using constant pressures (absorption) and backpressures (desorption), as well as maintaining near isothermality. This careful technique is appreciated. Fundamental mechanistic studies, important for the whole picture, are still weak. Although such classic hydrides have been judged by the DOE Hydrogen Storage Engineering Center of Excellence as being unlikely to meet DOE system targets, continued work on them in this project is wise. It will contribute to the better understanding of hydrides in general.

## Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.2** for its accomplishments and progress.

- A significant quantity of experimental results has been obtained. The comparisons of different catalysts are interesting.
- Much data has been generated. It is not only interesting, but it is also of potential practical value. The new catalytic results seem especially valuable. More mechanistic interpretations should have been developed beyond simple diffusion versus phase change limited rate control. For example, the NMR collaboration with the Jet Propulsion Laboratory should have been used to better help determine reaction pathways.
- It is interesting to try and understand the rate-limiting mechanisms of kinetics. However, the approach is somewhat simplistic—fitting data to diffusion controlled versus interface controlled mechanisms. There is a question about nucleation and various growth models (e.g., Johnson-Mehl-Avrami using different shapes of the nuclei). If the reaction is diffusion controlled, it would be interesting to know if it is possible to measure the diffusing species. This would be very important. Also, measurements of the activation energies for the various reactions would be extremely useful to the field.
- The presentation of work on $MgH_2$ provides no insights or data of value over the extensive quantity of literature published previously. The borohydride work shows improvement to desorption behavior, but as noted last year, no mechanism has been identified to indicate why a lower temperature desorption has taken place. An indication of the expected reaction pathway in the absence of presenting diffraction or NMR results of the product phase is necessary. Residual gas analysis of the desorbed gas phase would be important (e.g., $NH_3$ or $B_2H_6$). An indication of the desorption conditions, which can alter the kinetics of phase formation, is also critical. Desorption into vacuum can result in different phase formation than desorption into several bar of $H_2$ pressure. $B_2H_6$ formation would result in $B_{12}H_{14}$ formation that would essentially be irreversible. An analysis probing for the presence of all of these phases is necessary.
- Although the results reported in the experiments for the studied hydrides are probably reliable with respect to behavior under the test conditions, there is insufficient information on the decomposition products or

intermediate phases to allow detailed reaction pathways to be formulated and compared to theoretical analyses. While $NbF_5$ might be the most effective additive in decreasing the $H_2$ desorption temperature for the $LiBH_4/MgH_2$, and KH improves the kinetics for some $LiNH_2/MgH_2$ mixtures and not others, there is little insight being given into the actual microscopic processes. The project should do more in depth assessments of the actual changes in phase compositions and develop the underlying mechanisms if these systems are to be models for developing improved hydrides for storage applications. Evidence for extended cycling (i.e., hundreds of absorption/desorption reactions) of any hydrides in this project, which is needed for practical applications, was not apparent.

### Question 4: Collaboration and coordination with other institutions

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

- The researcher is collaborating with theorists.
- The collaborations are very good, but the results of these collaborations are not clearly shown here.
- While Johnson and Sholl are listed as collaborators, the list of destabilization reactions (which should also include the reaction enthalpies) was published by them. It was unclear whether these reactions were simply gleaned from the literature or if active discussions were involved. It was also unclear where the results are of the collaboration with the Caltech Solid State NMR Facility.
- The PI has explicitly interacted with at least a couple of theoreticians to select promising candidates for his experimental work, but it seems the PI has not contacted those researchers at the University of California, Los Angeles; Northwestern University; the University of California, Santa Barbara; and the University of Missouri-St. Louis who have been intensively doing first-principles modeling of defects and diffusion mechanisms for complex hydrides, including some that are being measured at Delaware State University. Although he has not previously explored collaborations to characterize samples from his experiments, the PI reported recent solid-state NMR studies, though none were included during the presentations. It would be interesting to see how these observations compare to the reactions given at this AMR. Some collaboration on applications of metal hydrides is occurring with researchers at the University of Delaware.

### Question 5: Proposed future work

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

- The proposed future work is good and should continue as planned. The project team should emphasize the development of more fundamental understanding via existing and new collaborations.
- The proposed future work seems to be a continuation of what is currently being done. There should be more focus on the most promising materials.
- The $MgH_2/LiBH_4$ system has been studied already. While the continuation of cycling studies is listed, it is not clear that this work has been initiated on the basis of the presentation. Unfortunately, there is nothing in the list of proposed work that suggests any more insights that are of relevance to the Program are to be gained from the planned work.
- There are some questions about whether any of the candidates being considered for future assessments are really viable for $H_2$ storage for vehicle applications. However, the project team should receive credit for supplementing the existing volumetric measurements of thermodynamics and kinetics characterizations with NMR and perhaps neutron scattering where deuterides should be used to maximize information content. Side reactions involving the formation of excessively stable phases have plagued borohydrides and amides.

### Project strengths:

- The PI has extensive expertise and equipment to perform studies on the thermodynamics and kinetics of $H_2$ reactions with metal hydrides. He has developed systematic approaches to conduct these experiments.
- The project's sound experimental work is an area of strength.
- The project features good kinetic studies that are done properly.

### Project weaknesses:

- The investigators need to better understand the details of the mechanisms that are going on.
- The project is still lacking important mechanistic connections.
- Much of this work has already been published or highlighted in previous Program reviews by other groups and the DOE Metal Hydride Center of Excellence. It is not clear whether deeper insights are to be gained by continuing to pursue this work if the level of effort expended is reflected by this presentation.
- Less-than-ideal complex hydrides and mixtures have been previously selected for study. In particular, those having significant reaction rates at temperatures above approximately 600 K will not meet DOE performance targets. Using only XRD to characterize reaction products is clearly insufficient for borohydrides, amides, and their mixtures.

### Recommendations for additions/deletions to project scope:

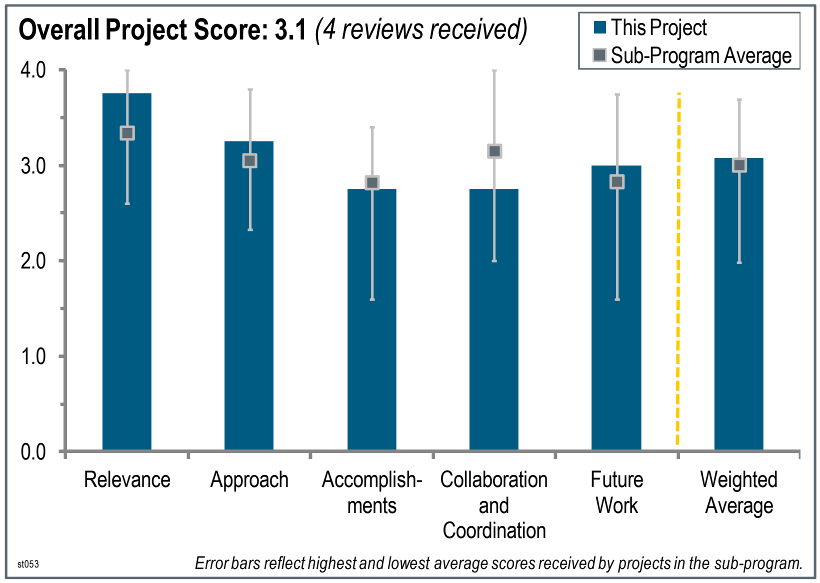
- The project team should focus the project on the most promising system. Work on systems that require the higher temperatures for absorption/desorption should be dropped.
- The only recommendation is that the researchers should increase collaborations in order to expand the scientific and practical value of the nice kinetic and catalyst results.
- The PI should carefully review the most current hydride research literature to identify a system that would serve as a strong candidate to support both theoretical modeling of reaction pathways and have the potential to be a practical $H_2$ storage medium. The PI should not consider a system such as $LiBH_4/MgH_2$ or $LiNH_2/MgH_2$ that has already been widely studied, unless he can provide new insights into the fundamental processes. Finally, the PI should take full advantage of NMR and neutron scattering techniques to evaluate the phase compositions of his as-prepared and reacted samples.

## Project # ST-053: Lifecycle Verification of Polymeric Storage Liners

Barton Smith; Oak Ridge National Laboratory

### Brief Summary of Project:

The objectives of this project are to address operational cycle life issues, meet or exceed applicable permeation and leakage standards, and prevent the loss of usable hydrogen ($H_2$) in $H_2$ storage systems. Temperature cycling-permeation measurement tests are conducted on tank liners, including tests on new liner materials and post-cycling analysis, and test methods are developed for cyclic testing of sectioned storage tanks in collaboration with manufacturers.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The durability and permeation of liners is important, though not so much in question now. Understanding the mechanisms could be helpful.
- The $H_2$ permeability through polymer liners for $H_2$ tanks is a key issue related to tank safety and lifetime.
- The scope of the project is relevant to the goal set to test the durability of polymer liners. This is a key element in being able to establish standardized test methods and data for future companies involved in providing $H_2$ storage equipment. With the difficulty associated with safely storing $H_2$ and the large number of companies that will enter the field, test standards and methods are critical to ensure safe equipment.
- It is clear that the separate activities as reported are very relevant to DOE objectives. However, for someone not intimately familiar with the effort, there is some confusion. The effort to understand effects by separating elements (liner by itself) is important, but it took reading the questions and answers (Q&A) after the presentation to realize that only in future testing is it proposed to look at the liner behavior with the composite shell attached. It is unclear if additional support from the shell could change the results. In the Q&A period it was pointed out that $H_2$ absorption within the liner could cause volumetric expansion, and this could pose a delamination concern. Delamination is a real issue, and when it occurs, pockets form between the liner and the shell. The pockets would readily accumulate $H_2$. This sounds important, but it is not addressed. It would be helpful to present the “broad” picture or plan showing the relationship of information gained in this effort and information sought in terms of meeting the overall goals. It would be nice to see a presentation of the research goals addressed by this effort within the context of a good, high-level description of all of the issues and information people would like to know about how liners function in the composite overwrapped pressure vessel system. The technical target for operational life is identified as 1,500 cycles. This research effort has addressed the number of cycles as a DOE-specified target, but this does not seem sufficient by comparison to natural gas vehicle standards.

### Question 2: Approach to performing the work

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

- The project features excellent facilities for $H_2$ permeability testing at high $H_2$ pressures.
- The cycling work is perhaps a little slow in coming; this is not largely considered a problem now. However, the new section testing could be very important.

- The team has put together a very good approach to creating a standardized and repeatable test method. However, testing is already ongoing in the industry by Type IV cylinder manufacturers, and there is no evidence that previous work was considered or used to help advance the project quicker.
- The approach makes good sense, but there is not enough description on the number of samples and how data from one manufacturer's liner is compared to another. It is understood that the information should not lead to revealing the manufacturers, but the presentation does not sufficiently distinguish results. The presentation could take more time to describe experimental apparatus and procedure.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Progress appears reasonable for efforts undertaken toward DOE goals.
- Test specimens machined from actual polymer tank liners have been evaluated, which is good because the behavior of real production material is being characterized.
- The test methods have shown to be adequate in measuring permeation and in conducting the cycle testing. The project meets the goals as presented.
- The measurements of permeation and extracting kinetic factors from these are nicely done. It is also nice to see the changes offset to some extent. However, the knowledge merely allows for better understanding of a problem that has already been fixed. It is nice to substantiate the manufacturers' learning, but it would have been more important a few years ago. The argon measurements will provide an interesting bit of knowledge that will separate the effect of $H_2$ from the effect of pressure cycling. That analysis should be done expeditiously—as soon as there is enough of a data range to estimate the effect. The use of larger discs is a good improvement. It will be good if they do test alternate liners or liners subjected to some specific insult.

### Question 4: Collaboration and coordination with other institutions

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

- The project features useful collaborations that are sufficient and worthwhile.
- It would be nice to have collaboration with a polymer chemist who has experience in the permeation of gases, particularly $H_2$, through polymers.
- The team members are strong. However, one comment from Q&A suggests participation by a chemist. This seems like a necessity.
- It was difficult to understand in both the oral and documented presentations the roles of the collaborating partners. It would seem that more collaboration would be necessary from existing companies doing polymer liner research and from testing standard organizations in order to establish and promote the work completed as the future standard and to not duplicate efforts. The slideshow does mention these organizations as collaborators, but it is not clear how or what roles were played.

### Question 5: Proposed future work

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

- The proposed future work looks reasonable.
- The plans are fine. It would be nice if some novel candidate that a maker of tanks was interested in was evaluated. The section testing would be a very nice addition to the capabilities. It would be nice to see a time frame for this.
- The proposed future work is aimed at expanding the testing to other materials. The scope of this project appears to be aimed at creating a test method and in establishing a standard. Testing of other materials is good to develop a database, but it is not as important as the work to make this research stand out as the future method and standard. The future work for increasing the speed of the test is very good.
- The proposed work described is important. There has been no mention of the study of effects likely present in the "real" world. This would include the differential effects of flow from rapid filling and the potential harms caused by the occasional impingement of a high-speed particulate (it is not supposed to be there, but it is likely it will

happen, perhaps as a contamination introduced by the dispensing system). Also, the effects of pump oils should be studied.

### Project strengths:

- This project features excellent capabilities for high-pressure $H_2$ permeability testing.
- There is good focus on the study of the liner materials with regard to permeation that is separate from the rest of the vessel system.
- This project's strengths include its unique equipment, ability to determine the effects of the pre-exponential factor and the activation energy in determining the liner permeation rate, and the ability to separate pressure from chemical impact.
- Very good methodology is being used and the results show repeatable, meaningful data. This approach should be used as the future standard. The technology being employed by the project team is excellent.

### Project weaknesses:

- The researchers are not moving fast enough to be as helpful as they could be or as needed.
- The project could benefit from additional polymer materials expertise.
- There appears to be a lack of drive to make this technology the industry standard. This work is good enough to do that and would save time for other projects and companies in being able to better screen and test liner materials.
- The study must include the liner behavior as incorporated into the vessel system. Effects of interest are the support from the composite shell and the effect of liner expansion/delamination on permeation. It is not clear if 1,500 cycles sufficiently defines vessel performance for lifetime service. The study should attempt to understand the how and why for progressive changes in the slope and pre-exponential factor.

### Recommendations for additions/deletions to project scope:

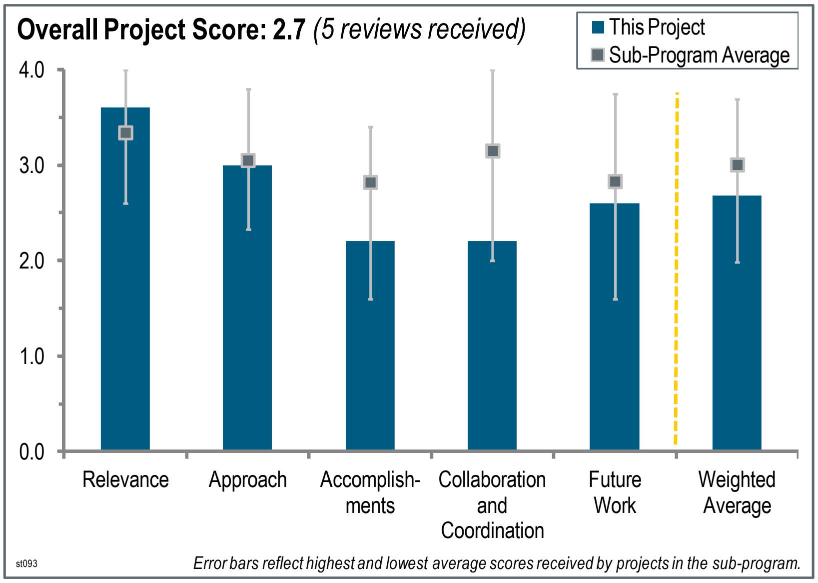
- The project team should set a time frame to collect the liner manufacturers and the standards organizations to promote this work and to advertise the ability to test multiple materials. A goal should be set to characterize a determined number of specimens and to make sure that each liner manufacturer understands and can access this technology.
- The project scope should include an increased number of cycles, effects of the shell, and effects of delamination. In addition, the project should include an investigation of the effects of likely contaminants, pump oils, and particulate.
- This reviewer could not identify any recommendations.

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

Felix Paulauskas; Oak Ridge National Laboratory

## Brief Summary of Project:

The objective of this project is to reduce the manufacturing cost of high-strength carbon fibers (CFs) by means of: (1) significant reduction in the production cost of the polyacrylonitrile (PAN)-precursor via hot melt methodology, and (2) the application of advanced CF conversion technologies in development at Oak Ridge National Laboratory (ORNL) to down-selected formulations. The key technical issue is improving PAN melt stability by reducing the melt temperature below the degradation temperature.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- The availability of low-cost and high-strength CF is critical to the success of the DOE Hydrogen and Fuel Cells Program.
- Developing cost-effective CF is critical for onboard hydrogen ($H_2$) storage, but this project is focused on producing precursor, not fiber.
- Decreasing the precursor costs by approximately 30% for CF is essential to substantially reducing compressed $H_2$ storage tank costs.
- This project is highly relevant to DOE's objective of reducing the cost of onboard storage systems. Melt spinning also has the potential to reduce the manufacturing cost of CF and increase the production rate. Because precursors account for approximately 50% of CF cost and CF dominates the cost of the compressed gas storage systems, success in this project can bring down the onboard storage system cost.
- Melt spinning of the precursor holds much potential in achieving cost reductions for CF. This project is relevant in that it directly addresses the cost reduction goal with unique technology. However, the storage of $H_2$ is difficult and ultimately may require only high-performance CFs in excess of 700 kilo-pounds per square inch (ksi) tensile strength.

## Question 2: Approach to performing the work

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

- The melt-processable PAN precursor provides an attractive path to achieve the goal.
- The approach focuses on modifying PAN to develop viable precursors for making CF using textile base processes. The key is the use of non-toxic solvents to form appropriate materials that form good fibers.
- The melt-spun approach has been partially proven by BASF in the 1980s and demonstrated in various U.S. patents and publications. The project seeks to improve PAN melt stability by reducing the melt temperature below the degradation temperature. In partnership with Virginia Tech (VT), ORNL is developing a new fiber spinning system with a multi-hole spinneret.
- This project aims to develop the precursor for the production of high-quality CF. The performance of CF depends not only on the properties of the precursor, but also on other procedures for production. The relation of

the properties of the precursor and those of the final products was not clearly shown by the principal investigator (PI).
- The basis of this project is keying on lessons learned from previous work that was completed but abandoned due to market conditions. This previous work provided an excellent baseline and established the direction of the project. The environmentally friendly elements of the project are key technologies and are fully integrated into the project plan.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.2** for its accomplishments and progress.

- The achievement of the team seems to be behind schedule.
- It is not clear why the project has only completed 20% of its work since the start of 2007. It is unclear if there was any risk mitigation plan during the execution of the project.
- The project seems to spend a substantial amount of time resolving issues from changes in precursor processing to form fibers. Hopefully, the lessons learned can be used to more quickly work through the issues and accelerate progress.
- This project appears to have made significant and lasting impact on the chemical barriers associated with melt spinning and has set the stage for further development. The filament quality is impressive. The area where progress is required, and is also being addressed, is in the handling and winding of the fibers. The chemical progress is impressive and the overall accomplishment would be rated as outstanding had it not been for the difficulties with the physical handling of the fibers.
- The project team has achieved good progress in producing the initial small count PAN precursor tows and demonstrating initial spinning with a hydrated melt of acrylonitrile/methyl acrylate ratio of 95:5. Physical properties and characteristics are approaching commodity-grade PAN precursor fibers. Some milestones were not met in fiscal year (FY) 2012 because of the unexpected challenge encountered with the winder system in the pressurized chamber. For several years, the PI has cited the 2007 Kline report that predicted an approximate 31% reduction in CF cost compared to the conventional wet-spun method. However, the Kline estimates were based on oil costing $60/barrel, which is about 40% lower than recent oil commodity prices. Additionally, the true gain cannot be quantified until the tensile strength and modulus of elasticity of melt-spun fibers are measured.

### Question 4: Collaboration and coordination with other institutions

This project was rated **2.2** for its collaboration and coordination.

- VT is the only external partner in this project. The project could benefit from collaboration and interaction with industry.
- This project has been conducted by a limited numbers of scientists. Collaboration with other people is recommended.
- If there is any coordination with partners outside of this project, the status of that coordination is unclear.
- It is not clear how much collaboration is occurring within the project.
- It is clear that the collaboration between VT and ORNL is highly functional and professional. The effort could have been improved had the partners looked earlier to experienced CF process engineers and/or consultants such as Izumi when they faced obstacles concerning fiber handling. Basic issues such as fiber winding and handling may be something with which the laboratory scientists are not fully experienced.

### Question 5: Proposed future work

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

- The efforts to continue improving formulations and fiber variability are good.
- The future work is well thought out. Emphasis should be placed on achieving good conversion of PAN filaments into CF because ultimately it is the quality of the CF, not the precursors, that matters.
- To continue the development of the precursor only is not favorable to realizing high-quality CF that is applicable to the high-pressure cylinder. It seems to be the time to extend the project to the fiber production.

- A risk mitigation plan is lacking from the future work. It is unclear if the CF strength will be measured after the conversion. If tensile strength does not meet the target, it would be nice to know what can be done to improve the performance.
- The future work discussed and presented focuses on the ability to make longer spools using proper filament winding. The FY 2013 “Future Work” is very vague and requires more detail and goals to continue the progress.

### Project strengths:

- Fiber precursor cost reductions are essential.
- The PI is highly experienced in melt-spun precursor development. The project benefits from more than a decade of prior development in CF research and development at ORNL.
- Using an original idea, the precursor to high-quality CF has been developed. The enthusiasm of the PI is a strong driver of this project.
- The melt-processable PAN precursor approach is innovative and provides an attractive opportunity to significantly reduce the CF cost.
- The strengths of this project lie in the advancements made on the chemical capability to melt-spin the PAN precursor.

### Project weaknesses:

- It is still early to project that the melt-spun CFs will achieve the target strengths (>600–700 ksi). Uncertainty in PAN conversion to CF could pose a serious risk to the success of this project.
- The relation between the performance of the CF and the property of the precursor is not clear. Thus, what appears to be an acceptable precursor may not necessarily result in high-quality CF.
- The project is moving slowly. A risk mitigation plan is lacking in the project execution phase.
- It was not clear from the presentation what other precursors and precursor processes may be used to reduce costs. It also seemed that a lot of effort was spent in an Edisonian way to resolve issues with small changes in formulation.
- The project’s weakness lies in what appears to be a manual melt spinning apparatus that would be well served to be automated to the point where the extrudate feeds directly into the melt spinning device and the winding and future post-treatment portion of the process.

### Recommendations for additions/deletions to project scope:

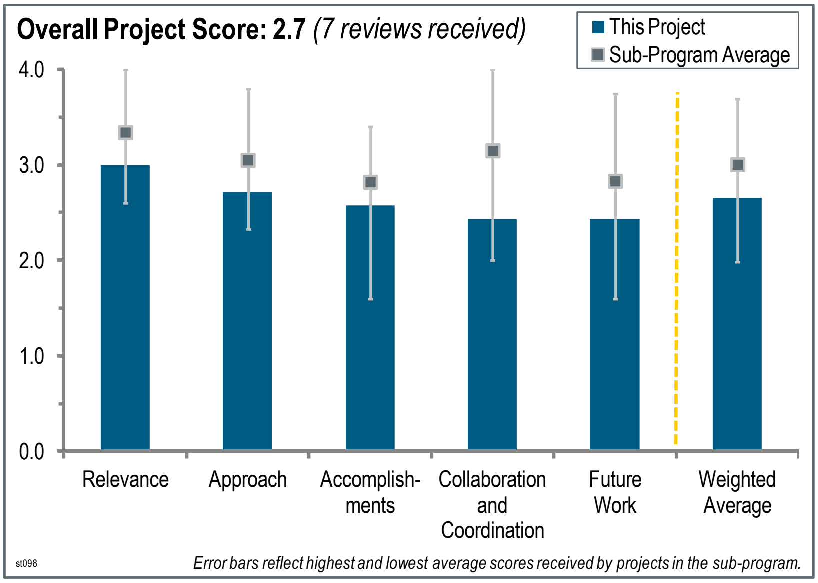
- Collaboration is very important for the next step of this project. It is recommended to collaborate with experts in CF processing.
- Perhaps a table of the different materials and processes that could be used and what their impact might be would be helpful to illustrate where the project should go and what could be achieved.
- To full understand the conversion capability of this precursor and to set the stage for further development, even to the point of commercialization, the goals of making continuous fiber of sufficient size and number should be added. These goals should be based on the CF line that will be used and contain multiple tow runs. Also, the cost data and goals should be recalculated and better understood, as this project should have more cost benefit than ST-099, which shows different numbers. Efforts should be taken to set goals and establish a plan for further scalability beyond just noting that consideration will be made.

## Project # ST-098: Development of a Practical Hydrogen Storage System based on Liquid Organic Hydrogen Carriers and a Homogeneous Catalyst

Craig Jensen; Hawaii Hydrogen Carriers, LLC

### Brief Summary of Project:

The objectives of this project are to: (1) identify the liquid organic hydrogen carrier (LOHC)/pincer catalyst combination that gives the best combination of high cycling capacity and rapid dehydrogenation kinetics without LOHC degradation upon cycling; and (2) design a space-, mass-, and energy-efficient tank and reactor system to house the LOHC and facilitate hydrogen ($H_2$) release that can be easily interfaced with a fuel cell. The system needs to be able to store relevant amounts (6.6–8.8 wt.%) of $H_2$, be affordable and abundant, eliminate thermal management problems, and make use of existing and established infrastructure and materials.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The research is aligned with DOE Hydrogen and Fuel Cells Program objectives. While the liquids under study do not have a direct path toward gravimetric system targets, they do offer many advantages for an engineered system.
- The project is highly relevant to DOE objectives. The presentation notes a number of advantages of LOHCs; in many ways these materials can be seen as the ideal $H_2$ carrier.
- This project is well aligned with DOE objectives. An inexpensive LOHC with a homogeneous catalyst seems to be one of the more promising options for low-pressure automotive $H_2$ storage.
- The project seeks to obtain liquid carriers with adequate rates of $H_2$ delivery. Liquid systems have significant benefits for off-board regenerable materials. The project falls short of being able to meet DOE system gravimetric/volumetric targets. Low-pressure systems have benefits.
- The idea is to use organic liquid carriers, which is very close to Air Products' research and patents. The difference is the use of homogeneous catalysts rather than heterogeneous ones. It is not overly innovative. The overall idea is good, as organic liquids can exploit the current gasoline infrastructure. A serious concern is that the iridium (Ir) pincer catalysts are too expensive in terms of the metal and the ligands. There is a lack of further catalyst development using cheaper ligands and first-row transition metals, which are cheap. It is built on previously supported DOE work. There is an issue with weight percent if only a small amount of $H_2$ comes off. It is unclear if it will be enough to be useful. The use of a liquid will improve thermal management. Regeneration can be performed off-board.

### Question 2: Approach to performing the work

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

- The key barriers are identified and there is a clear pathway to address them through chemistry and tank engineering. The development of the reactor in parallel with the chemical studies seems to be working well.

- Utilizing liquid carriers has been done and proven to have thermodynamic issues with $H_2$ release and discharge (apart from low $H_2$ content). Focusing on increasing the kinetics and reactor design for these carriers (which have already shown to have these issues) would not lead to carriers with properties approaching DOE targets.
- This work seems to build on the considerable amount of work done by a previous project headed at Air Products. It is not clear whether all of the work in that previous project and the knowledge gained are being most effectively leveraged.
- This project brings together two well-qualified, creative co-principal investigators (co-PIs) who have chemistry and engineering expertise to optimize the liquid $H_2$-carrier and reactor. There was some introduction about how modeling was being used to provide insight into catalysis design, but the description in the approach could have been more detailed. The results from each individual approach were communicated clearly and understood, but it was less obvious to see how the engineering was helping the chemistry and how the chemistry was helping the engineering. This is not to say it is not there, just that it needs to be communicated more clearly. The novel approach uses a homogeneous catalyst instead of a heterogeneous catalyst and looks to offer some advantages over heterogeneous catalysts, especially regarding the challenges with three-phase boundaries (gas/liquid/solid).
- The approach is built on demonstrated chemical principles and modeling methods to predict reactor performance. The plan and subtasks outlined in slides 12 and 13 represent a reasonable strategy for addressing the targets. The main drawback is that chemical factors for selecting new LOHCs (and perhaps catalysts) for study are not evident. The criteria in slide 9 give some background, but perhaps there are secondary chemical features in addition to hetero-substitution that can be identified and related to how well these criteria are likely to be met.
- The investigators have a good overall plan. They recognize issues beyond thermodynamics—for example, the constraints of melting points and boiling points. They need to consider and deal with the potential toxicity of reactants, intermediates, and products. They have an excellent connection to reactor designs with the General Motors (GM) partner. They need to optimize the catalyst and get away from Ir if possible because it is likely to be too expensive and potentially too rare. A survey of the amount of Ir available is needed. It would be nice to know what percent Ir catalyst is needed. The number quoted was that it cannot be more than 500 ppm. It is unclear what is currently being used. There is a lack of studies on catalyst properties such as the catalyst lifetime and turnover number (TON). There are close interactions with the GM reactor design team.
- The approach is generally reasonable, but incremental, compared to earlier DOE-funded work by Air Products (that was discontinued for lack of ultimate capacity), so the approach is lacking innovation.[Editor's note: The former Air Products' liquid carrier project funded through the Hydrogen Storage sub-program was not "discontinued" by the DOE but came to its contractual end.] If DOE targets are used as guidance, then the approach will not be able to meet the DOE gravimetric target for a system. The major difference is that the present research is focused on using more active low-temperature homogenous catalysts while Air Products focused on heterogeneous catalysts. The present approach does not address the disadvantages of having a catalyst in contact with spent fuel (reverse reaction limiting conversion). The approach did not address delivering $H_2$ at DOE target pressure, so the experimental approach of measuring kinetics and conversions at 1 atm seems unrealistic. The team should use 5 atm of $H_2$ backpressure as an experimental condition on which to perform these experiments on a reversible system, especially with spent fuel in contact with $H_2$ and a homogeneous catalyst. It seems the team has not thoroughly addressed the implications of the pressure regime for operation and the impact of these parameters on the absolute amount of $H_2$ that may be released. Ultimate capacity is limited to around 7 wt.% in the material (1 hydrogen atom per carbon or nitrogen atom); it is not likely to exceed this value and still have a regenerable system. The endothermic release of $H_2$ from the carrier must address heat inputs (and associated penalties, etc.). Higher-capacity compounds trend toward lower molecular weight, and thus higher volatility. Scrubbing of fuel and spent fuel from the $H_2$ stream were not addressed in the approach or in the future plans.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- The project team designed the reactor and found improved kinetics using the pincer catalyst.
- The progress is hard to judge because this appears to have been an ongoing project that was not being funded by DOE. It is not clear how much progress was the result of DOE funding versus previous funding.
- This project has shown a few nice accomplishments over the past year. The kinetic studies are useful, but it looks like there has been limited progress getting $H_2$ off six-membered rings at reasonable temperatures.

- For such a short project, the amount of progress accomplished (the project is 40% complete) seems quite low.
- Many reactor designs were considered and many were eliminated, but it would have been useful to understand why specific designs were eliminated to appreciate the progress. Perhaps this insight would be helpful to the Hydrogen Storage Engineering Center of Excellence (HSECoE), specifically the chemical storage concepts being considered for endothermic alane. The presentation included a good description of the down-selection process to five-membered rings, but it would be useful to hear about plans for improving or maintaining catalyst stability. It is unclear if there is anything more to do beyond keeping the temperature lower, and how the different ligands affect stability. For example, a slide shows the TON for various ligands of about 250. It is unclear if there is a path forward to improve this TON for the Ir pincer catalysts, or if this TON is sufficient for the technical approach.
- There has been some progress; however, it appears the six- and nine-month milestones (slide 14) have not been met. The reactor design and modeling appear to be progressing according to plan. The dehydrogenation accomplishments presented also seem to be relatively small advances. For example, data presented on the system identified as most promising in the summary, methylperhydroindole, shows only the five-membered ring dehydrogenated at 160°C. This is a significantly lower temperature, but also a much lower $H_2$ content than the full dehydrogenation of decalin presented as background to the research. The PI gave the impression that further (proprietary) progress had been or is about to be made, but it is difficult to take this into account in a public review. It would be nice to see a more complete presentation of dehydrogenation that gives some measure of how facile the dehydrogenation is of the other parts of the molecules under investigation.
- The investigators have made good progress overall with some significant accomplishments, but there are critical issues that have not been addressed in terms of catalyst lifetime and TON, going beyond Ir and the current pincer ligands. It was nice to see the close connectivity with the GM reactor design team. This is a very positive part of the project. The reactor design work looks to be quite nice. It would be nice to know how much of the activation energy is just overcoming the endothermicity of the reaction. The researchers have very interesting work on five-member ring chemistry. They also remove the bridgehead hydrogen atom, which is an interesting scientific result. They have down-selected a set of compounds. There are no real details on the compounds they will use or their cost for first fill.

### Question 4: Collaboration and coordination with other institutions

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

- Their collaborations are satisfactory. Hawaii Hydrogen Carriers and GM work together effectively. The only external collaborator is Oregon State University.
- The collaborations seem somewhat limited. Collaborations with other groups interested in liquid carriers (e.g., Shih-Yuan Liu at the University of Oregon or Air Products) would be useful.
- Although the project seems to be a good fit within the HSECoE, no collaboration between the two seems to exist.
- Scott Jorgensen at GM and Craig Jensen at Hawaii Hydrogen Carriers work really well together; they are highly collaborative and synergistic.
- The collaboration between the co-PIs is good, but the level of collaboration was not so clear with the HSECoE, especially the chemical storage groups working on reactor designs. It would be interesting to know if any of the discarded reactor designs in this project are similar to the reactor designs considered by the HSECoE. It would be invaluable to share thoughts on what makes a good reactor for a liquid carrier, either exothermic ammonia borane or endothermic alane.
- There is some collaboration, perhaps even beyond the partners listed on slide 20, judging from the co-authors on the presentations listed. However, collaboration does not seem to be a strong feature of this project; for example, there are no accomplishments acknowledging assistance from outside institutions. It is likely that there are significant ties between the reactor work and the HSECoE in particular, and that some of the other participants in DOE $H_2$ storage research could make contributions. These relationships need to be developed and highlighted in future reviews.

### Question 5: Proposed future work

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

- It is highly recommended to focus on new carrier design.
- Proposed future work involves completing kinetic studies of perhydro-indolizidine dehydrogenation. It appeared that the studies were over a good temperature range. It was not clear what was needed to complete the studies. It is unclear if there are plans to measure the vapor pressure and volatility of the smaller five-membered ring organic liquid carriers. The reactor design could not be any more ambiguous: "Determine properties to meet different DOE targets."
- Much of the future work proposed is to conduct kinetic studies of various LOHCs. While this is important, the thermodynamic issues noted in slide 12 and discrimination between kinetic and thermodynamic limits should be addressed because many of the LOHCs presented have undergone incomplete dehydrogenation. The cycling studies proposed are a valuable component of the project, and it is sensible to conduct this for only the best-performing LOHC identified.
- The investigators have a good plan on where to go with what they have done. They need to look at new catalysts beyond Ir. They also need to do studies of catalyst lifetime and TON for the ones they do have. The proposed regeneration work is good. The proposed reactor work is good.
- Cycling studies will be critical to determine viability. In addition to determining the capacity retention during cycling, it will be important to determine how much catalyst is lost with cycling and how this will impact cost.
- It is hoped that the PIs will take the recommendations offered and improve their approach to address the potential for reaching equilibrium-limited (low) conversion when running against 5 bar of $H_2$ (delivery pressure to the fuel cell). However, even if the equilibrium limitations are removed, these materials cannot exceed around 7 wt.%, so it is exceedingly difficult to imagine how they can meet DOE targets, particularly the "ultimate" targets.

### Project strengths:

- This project features a very capable staff.
- This project has a very capable team of co-PIs with extensive experience to carry out the required tasks.
- The project is well founded in good scientific principles, and the background literature has been researched and understood. The reactor design is being carried out using sound methodology with good links to commercial application.
- The researchers are making good progress on a good, but not original, idea. The use of a liquid fuel system makes it more likely that it can use some of the existing infrastructure. They have a potentially working system. There is a good effort with the GM reactor design team.
- This project is focused on one of the more promising options for automotive $H_2$ storage. The project seems to be making nice progress, especially with the development of the liquid organic carrier in parallel with the reactor.
- Strengths of this project include two really good PIs working together; a liquid carrier; low-pressure systems; and simple, well-understood chemistry and catalysis.

### Project weaknesses:

- There seems to be a lack of coordination with HSECoE on the reactor design. One would assume there is much to be learned from each party.
- The project has looked only at incomplete dehydrogenation of LOHCs so far, and a pathway to increasing the realizable $H_2$ capacity while maintaining low-temperature release has not been articulated. Rehydrogenation has not been investigated. The project duration—less than two years—is relatively short to achieve difficult goals.
- The investigators will need to get all of the reactions near 100%. They need to go beyond Ir pincer ligands, and they need to address catalyst lifetime and TON as well as cost. The presentation was lacking details on the compounds to be used and their cost. The team needs to address toxicity issues with the proposed process; there may be none, but they should state that.
- $H_2$ release from the five-membered rings is 2–3 wt.%, and it is approximately 6 wt.% from the six-membered rings. Given these constraints, it seems likely that this work will fall short of the targets (but it will probably still be better than most of the other options). Catalyst cost is a weakness, and it is unclear if the low catalyst

concentrations needed to keep the cost down will be suitable. Use of a homogeneous catalyst may result in some catalyst loss during operation.

- Although the project seems to be a good fit within the HSECoE, no collaboration between the two seems to exist. Utilizing liquid carriers has been done and proven to have thermodynamic issues with $H_2$ release and discharge (apart from low $H_2$ content). Focusing on increasing the kinetics and reactor design for these carriers (which have already shown to have these issues within the project) would not lead to carrier properties approaching DOE targets.
- Regarding the homogeneous catalyst, the ligand is likely to be quite expensive. Keeping the homogeneous catalyst in contact with spent fuel in the presence of $H_2$ makes dealing with the back reaction a significant problem—there could be equilibrium limitations. The limited ultimate capacity of around 7 wt.% material makes it difficult to meet system targets, and there is no path forward to increase capacity. There exists only one hydrogen atom per carbon or nitrogen atom. This has the identical approach to the Air Products liquid carrier project that was discontinued by DOE, due to the lack of ultimate capacity. [Editor's note: The former Air Products' liquid carrier project funded through the Hydrogen Storage sub-program was not "discontinued" by the DOE but came to its contractual end.]

## Recommendations for additions/deletions to project scope:

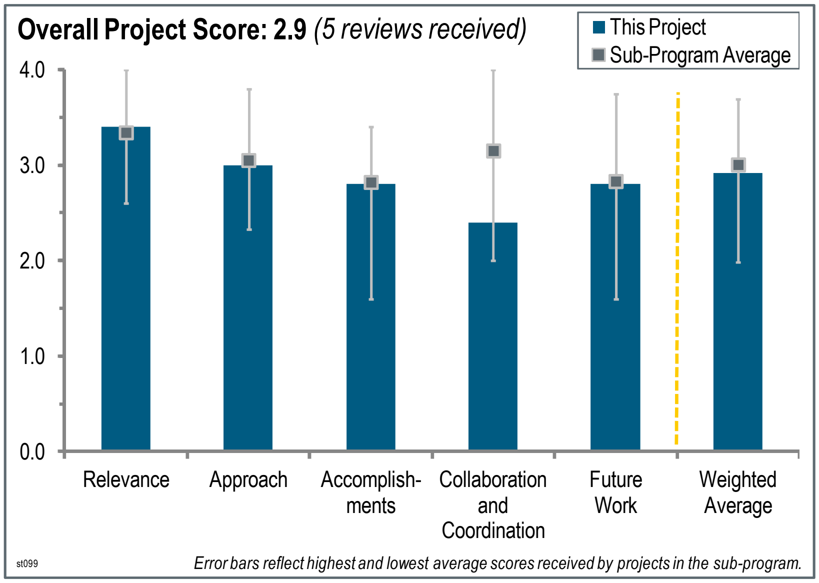
- It is unclear if using Arrhenius rate parameters revealed what temperature, catalyst concentration, and reactor length is required to achieve a release rate of 2 g $H_2$/second to provide the fuel cell under full load.
- The project team should address other types of catalysts and study catalyst properties.
- The future plans and project scope seem appropriate. It is highly recommended to focus on new carrier design with acceptable thermodynamics and high $H_2$ weight percent.
- If the project is continued, the team needs to perform kinetics for conversions at the delivery pressure that the DOE target specifies (5 bar for fuel cells to demonstrate that there are or are not equilibrium limitations on conversions, or rates of $H_2$ evolution).

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

Dave Warren; Oak Ridge National Laboratory

## Brief Summary of Project:

The purpose of this project is to develop a textile-based precursor that uses polyacrylonitrile (PAN) produced in high-volume textile mills (carpet, knitting yarn, etc.), and to develop a reduced-cost high-strength carbon fiber (CF) based on textile spinning processes. High-strength CF enables durable, lightweight, compressed hydrogen ($H_2$) storage vessels to be manufactured. The project approach includes identifying candidate PAN-methyl acrylate (PAN-MA) resins, determining fiber spinning parameters, and determining the conversion protocol.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- Developing cost-effective CF is critical.
- The development of lower-cost fibers is essential to low-cost $H_2$ storage systems.
- This project is relevant to DOE's objective of reducing the cost of CF for use in high-pressure $H_2$ storage tanks. Development of a low-cost commercial textile precursor provides a short-term, fast-track approach to achieving the objective.
- This project is extremely important to many of the DOE goals, both in automotive and non-automotive fuel cell technologies. From an automotive view, this may be the most important short-term work funded. From a non-automotive view, it is still important in advancing technology.
- This project is aimed at creating an identical CF to one that is already commercially available today. The goal of the project is based on creating cost savings through improved throughput of the precursor and the use of MA as a co-monomer. With little data available on the competition's relationship between throughput and cost, and MA only being around 5% of the polymer, this project lacks more detailed and specific goals other than the overall cost reduction based on a theoretical cost model. The availability of a lower-cost, air-gap-spun fiber would assist the overall DOE efforts concerning global precursor supply.

## Question 2: Approach to performing the work

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

- The overall approach is sound—looking for appropriate fiber resins, spinning them, and determining the conversion protocols to make them CFs. Working with a potential supplier to the world is excellent. The materials chosen are good ones.
- The CF precursor is not produced in the United States, but by a partner in Portugal. To avoid risk, it is better to have the precursor producer in the United States. The "Underestimating of the difficulties of that task" section on the summary slide makes it seem as if there is an issue of planning with this project.
- This project is leveraging previous projects funded by the DOE Vehicle Technologies Program to deliver a fast-track approach to produce high-strength commercial textile precursor. FISIPE is responsible for producing the

precursors and Oak Ridge National Laboratory (ORNL) is responsible for developing the conversion protocol and fiber spinning parameters in order to achieve the highest fiber properties possible.
- Identifying ways of making fibers to decrease costs is extremely important. The use of standard textile manufacturing is a good approach. However, the use of a limited number of precursors and manufacturing techniques may not achieve all of the possible benefits.
- The principal investigator (PI), his team, and the primary collaboration partner have a very good technical approach to this project. They have sufficiently addressed many of the obstacles in modifying an acrylic fiber line into an air gap spinning line, and notable progress is being made. The key to the project will be the post-spun treatment (stretching, washing, drying, etc.) and the resulting operability of the CF during stabilization and carbonization. The improvement will need to be an increase in the target mechanical properties of the resulting CF to meet industry standard properties of 700 kilo-pounds per square inch (ksi) and 2.0% strain.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The milestone of March 2012 has been completed, despite the delay with the equipment setup.
- The second trial exceeded the milestones with a variability of 2%–10%. It seems that little progress has been made, but in fact there was a lot of work required to get to where they are.
- There have been significant accomplishments made in creating the air-gap-spun technology. This represents a notable advancement over the technology employed by the collaborating partner. However, the real results will be the processing of the precursor in a larger-scale CF trial. However, the line retrofit did take longer than expected.
- This project has just completed its first full year. Progress was somewhat slow because of longer-than-expected time for FISIPE to retrofit its equipment to produce a PAN-MA precursor and a delay in setting up the polymerization and fiber spinning equipment at ORNL. Three precursors were down-selected for further development. One of the three precursors (F1921) was chosen for spinning trials. Results showed a large variation in tensile strength (282 to 419 ksi) and tensile modulus (27 to 36 million pounds per square inch [msi]). Goals are to reach 650 msi tensile strength and 33 msi modulus. If the goals in tensile strength and modulus are achieved, the commercial textile precursor has the potential to bring down the cost of CF substantially.

### Question 4: Collaboration and coordination with other institutions

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

- This project has few collaborators, but they are the right ones.
- ORNL partnered with FISIPE. There is no interaction or collaboration with others in the industry.
- The precursor is produced in Portugal and the carbonization is conducted at ORNL. It seems to be a good collaboration. However, it would be perfect if both organizations were in the United States.
- It was not clear how much collaboration was occurring and if the project was actively looking at the most state-of-the-art techniques that could be used to reduce costs.
- ORNL has been providing very effective and timely work to the collaborating partner, with whom ORNL states it has an excellent working relationship. The lack of cooperation appears to be concerning passing information from the collaborating partner, FISIPE, to ORNL, because the key elements that would help ORNL validate its effectiveness in meeting the cost target are not available, per the PI's comment that it is proprietary to FISIPE's business. More detailed conversion information concerning the plant and its operations parameters is necessary for ORNL to quantify that it is actually able to meet the cost targets.

### Question 5: Proposed future work

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

- The plans are OK, but they are not overly ambitious.
- A lot of work needs to be done just to optimize with present manufacturing processes.

- Future plans are to improve precursor purity and spinning of rounder fibers, as well as optimize tension limits in stretching fiber. The team also plans on converting the other two down-selected formulations. The time constraint in this two-year project could compromise the planned activities.
- The gate is significantly high. Plans explaining how to reach the target of 550–750 ksi strength in April 2013, which is double to that of March 2012, should be shown in much more detail.
- The future work to convert the latest precursor into CF is the major effort going forward. However, the “Future Plans” slide does not address the post-spinning processes of stretching, washing, drying, and finish application, which are key contributors to meeting property goals, achieving stabilization efficiency, and eliminating “baby” filaments. It is unclear in the work whether or not ORNL will be an integral partner in this phase of the process, or how this part of the work will proceed.

### Project strengths:

- The use of textile manufacturing techniques is a good approach.
- ORNL has many years of experience from previous projects in the development of low-cost CF.
- ORNL’s collaboration with the Portuguese company seems to be a good idea, under the condition that no United States company can make the precursor of high-quality CF.
- Strengths of this project include its possible cost reduction and partners that are perfect to do this and move it to production.
- The strength of this project lies in the technical team that is assembled and the use of MA as a co-monomer. The overall goal of providing additional precursor capacity in the global market at a reduced cost is key to the CF industry as a whole.

### Project weaknesses:

- There is no producer of precursor in the United States.
- Lack of speed is a weakness of this project.
- The project has passed its mid-point in terms of duration (for this two-year project), and there is still much to prove. One does not get the feeling that the schedule has room for delay or error, thus there is significant risk that the project will not achieve its goals on time and on budget.
- The project should be very active in identifying potential methods that could reduce costs and increase manufacturing rates. Also, the effort should at least look at what other precursors could provide and if cost reduction can be achieved through this route.
- This project is helping to fund the conversion of an acrylic fiber line into a line that produces PAN fibers for conversion into CFs. The weakness lies in the fact that this is not new technology, as this exact transformation has been completed by most of today’s CF producers as long ago as the mid-1970s. The use of MA is a unique factor in this project and appears to be a key element in the cost reduction. However, the realized impact of using MA has yet to be fully characterized because impacts to conversion yield and fiber quality are key cost elements that do not appear to be fully quantified in the cost model.

### Recommendations for additions/deletions to project scope:

- Combining or merging ST-093 and ST-099 should be considered.
- The project team should bring the polymer choices as far forward as possible in the process to maximize process development time on the polymer that will be used, not other polymers. The team should do what is needed to pick up the pace.
- A low-cost manufacturing roadmap should be developed to identify the baseline proposed and to identify other state-of-the-art manufacturing steps or processes that could be used to bring costs down even further.
- The addition of more detailed and incremental goals would assist in the down-selection of the precursors. Measurements and analysis surrounding the physical properties of the precursor and how those properties are affected by polymer dope filtration and all aspects of the PAN spinning process need to be developed to reduce the expense and time associated with running large-scale CF trials on the pilot lines associated with the collaborating partners. In addition, conversion rates and qualitative fiber parameter goals need to be established in order to understand whether or not the cost goal will be met.

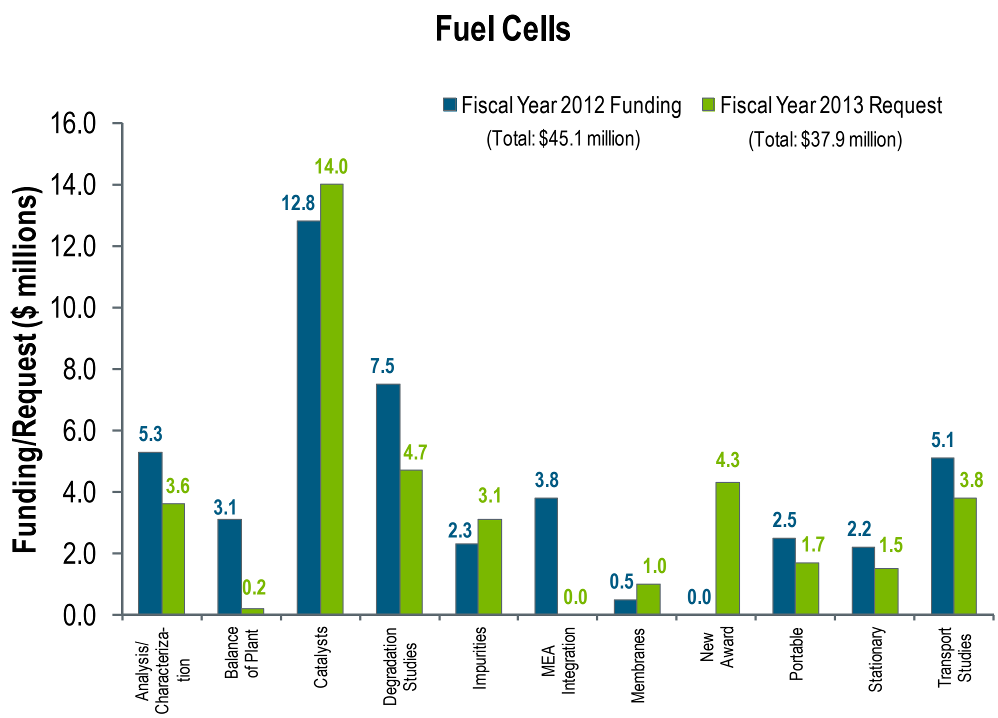
## 2012 — Fuel Cells
## Summary of Annual Merit Review of the Fuel Cells Sub-Program

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

Reviewers commended the Fuel Cells sub-program for being well managed and clearly focused, with annual progress demonstrated. They noted that key challenges were identified, and good plans and a project portfolio were in place to address the key challenges. However, reviewers also commented that certain areas of the portfolio, such as membranes, need to be further strengthened. They also expressed concern about whether current funding levels would be able to support expanding the portfolio.

### Fuel Cells Funding by Technology:

The Fuel Cells sub-program received $45 million in fiscal year (FY) 2012 and $38 million is requested for FY 2013. The sub-program continues to focus on reducing costs and improving durability with an emphasis on fuel cell stack components. Differences in the funding profiles between FY 2012 and the FY 2013 request are due in part to a mandate by Congress to fully fund projects in the year they are awarded rather than obligate future funds. In 2012, two new projects were awarded in membrane electrode assembly (MEA) integration and balance of plant (BOP), and the total cost of the projects was assigned to the FY 2012 budget.




### Majority of Reviewer Comments and Recommendations:

At this year's review, 66 projects funded by the Fuel Cells sub-program were presented and 45 were reviewed. Projects were reviewed by between four and eight reviewers with an average of seven experts reviewing each project. Reviewer scores for these projects ranged from 1.8 to 3.6, with an average score of 3.0. Both this year's highest score of 3.6 and average score of 3.0 were similar to last year's highest score of 3.7 and average score of 2.9. The lowest score of 1.8 for all projects reviewed in 2012 was lower than 2011's low score of 2.0 for all projects reviewed in 2011. The lowest scoring project in 2012 was different than in 2011.

**Analysis/Characterization:** Seven projects were reviewed and received scores between 3.3 and 3.4 with an average score of 3.3. The reviewers praised the projects for significant accomplishments and progress in areas of system analysis, imaging, and development of testing protocols. Analysis/characterization projects were noted for accomplishments related to optimal platinum loading, providing unique imaging capabilities, and addressing performance and durability. Suggestions for future improvement included development of in situ liquid transmission electron microscope (TEM) techniques, including more extensive error and uncertainty analysis; description of the connections between imaging and reaching DOE's technical targets; and enhancement of the accuracy of the decay prediction.

**Balance of Plant:** Three BOP projects were reviewed this year, each receiving a score of 3.0. According to reviewers, the BOP projects may lead to a significant positive impact on the fuel cell system in terms of volume, weight, and maintenance. The reviewers expressed concern about the long-term durability of humidifiers and coolant systems being studied and suggested that projects should increase the understanding of degradation mechanisms.

**Catalysts:** The scores for the 11 catalyst projects ranged from 2.7 to 3.6 with an average of 3.1. According to the reviewers, catalyst projects have demonstrated significant improvements in catalyst cost, durability, and oxygen reduction reaction (ORR) activity. Reviewers expressed concern about some projects relying on rotating disk electrode (RDE) measurements to predict performance and suggested that performance of catalysts should be tested in MEAs. The reviewers suggested that more emphasis be placed on direct measurements of conductivity after exposure to relevant conditions for some projects.

**Cross-Cutting:** The four cross-cutting projects received an average score of 2.3 and ranged from 1.8 to 2.8. Reviews were mixed for cross-cutting projects. One project was praised for having a good mix of industry and academic expertise. For the other projects, reviewers expressed concern that some are either not relevant to the sub-program's goals or have a scope that is too broad. Reviewers suggested that the remaining tasks need to be reevaluated and prioritized.

**Degradation Studies:** The average score for the six degradation projects was 3.1. Scores ranged from 2.3 to 3.5. Reviewers praised the degradation projects for focusing on key parameters that affect fuel cell durability and for collecting and analyzing necessary data to highlight the key factors that impact voltage loss. According to the reviewers, the projects have good collaborations. Reviewers expressed a desire to see model results that more accurately address commercial fuel cell systems and for more evident correlation between model and experimental characterization.

**Impurities:** The two impurities projects reviewed received scores of 2.9 and 3.5. Reviewers commended the projects for collecting an extensive knowledge base about contaminants, generating a reliable model, and collaborating with stack manufacturers. Reviewers were concerned there is not sufficient data to make the models as reliable as possible. Reviewers suggested developing a better understanding of the causes of the performance losses and that tolerance limits for contaminants be recommended.

**Membranes:** Two membrane projects were reviewed, receiving scores of 2.6 and 2.9. Reviewers felt the projects had good concepts and approaches. There was some concern about the focus of one project on developing better membrane supports rather than better membranes. Reviewers also had concerns about the likelihood of success of one project.

**Portable Power:** The three portable power projects reviewed received an average score of 3.0, with scores ranging from 2.6 to 3.4. Reviewers felt the projects were relevant, with strong teams making good progress in water management and durability. Reviewers recommended increasing focus on developing membranes with high conductivity and reduced methanol permeability. The reviewers suggested increased interactions with original equipment manufacturers where relevant.

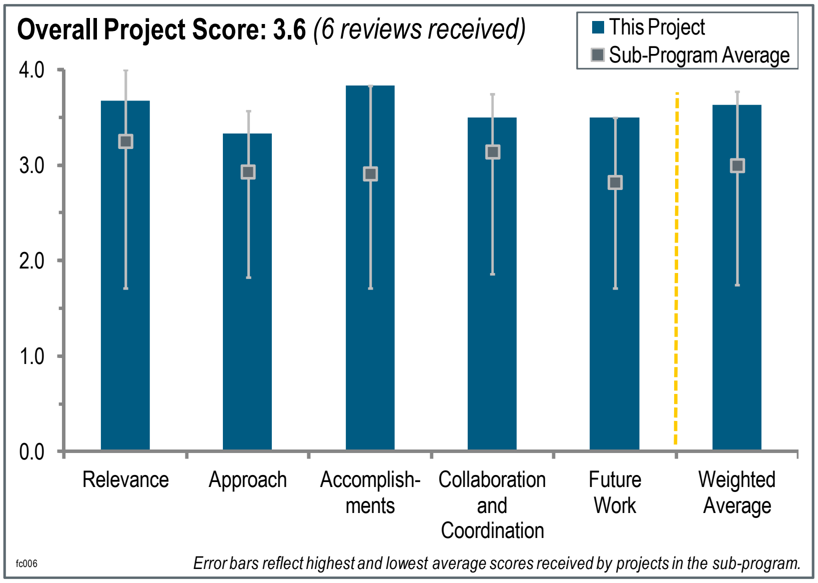
**Stationary Power:** The three stationary power projects reviewed received an average score of 2.9, with scores ranging from 2.7 to 3.4. According to the reviewers, the stationary projects are relevant to the sub-program's goals and have technically strong teams. Reviewers commended one project for making good progress towards addressing critical barriers of performance, durability, and cost. The two remaining projects received mixed reviews. One project was commended for making good progress, while reviewers were skeptical that the technical milestone could be met. Reviewers commended the final project for having a technically sound team, but were concerned about the lack of progress made.

**Transport Studies:** The four transport projects reviewed received an average score of 3.0, with scores ranging from 2.7 to 3.4. Some transport projects were praised for being well designed and for making good progress. Reviewers suggest adding studies of a larger variety of MEA configurations and of stack-level freeze cycle transients. For one project, reviewers expressed concern about lack of published results and recommended disclosure of the model in a peer-reviewed forum.

# Project # FC-006: Durable Catalysts for Fuel Cell Protection During Transient Conditions

Radoslav Atanasoski; 3M

**Brief Summary of Project:**
The objective of this project is to develop catalysts that will enable polymer electrolyte membrane (PEM) fuel cell systems to weather the damaging conditions in individual fuel cells during transient periods of fuel starvation, thus making it possible to satisfy 2015 U.S. Department of Energy (DOE) targets for catalyst performance, platinum group metal (PGM) loading, and durability. This project will develop a catalyst that favors the oxidation of water over the dissolution of platinum (Pt) and carbon at voltages encountered beyond the range of normal fuel cell operation and beyond the thermodynamic stability of water.




## Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- By design, the program addresses prescribed targets and therefore is relevant to national goals.
- This project addresses challenges related to catalyst performance, PGM loading, and durability. It is intended to meet DOE 2015 targets. The project is fully aligned with DOE research, development, and demonstration objectives.
- This project is working to use material-based solutions to increase durability (as opposed to engineering-based solutions), which is what a DOE project should do.
- Catalyst durability under start-up, shutdown, and cell reversal is an important issue to address and is relevant to DOE's overall objectives. A more rigorous milestone for PGM and electrochemical surface area (ECSA) loss may be able to be achieved.
- This project addresses durability, one of the key barriers to the adoption of fuel cell technology, using a passive materials-based approach that could reduce system costs and is relevant to the DOE objectives of reducing the cost and increasing the durability of fuel cell systems. This project addresses degradation due to start-up, shutdown, and cell reversal, key degradation modes in automotive fuel cells.

## Question 2: Approach to performing the work

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

- The approach is to add "depolarizers" to the catalysts that inherently limit potentials to values that preclude adverse side effects. This idea has considerable history and is a sound and proven concept.
- The approach section showed the employment of go/no-go decisions for specific quantified parameters, which were met and exceeded in the past year. The approach includes stepwise development toward the desired targets.
- The approach to use oxygen evolution reaction (OER) catalysts on cathodes to mitigate dissolution of Pt and carbon during start-up/shutdown and cell reversal is good. The use of these precious metals to prove the concept has been valuable. Exploring less-expensive, lower-loading, and perhaps more-durable OER catalysts would be the next challenge.

- The materials approach includes the use of ruthenium (Ru) and iridium (Ir) on the anode. The project must be careful in the use of these materials. Ru is known to crossover to the cathode and poison the oxygen reduction reaction (ORR). Ir is a relatively rare metal (worse than gold [Au] and Pt). The main approach appears to limit the loading of the Ir/Ru, which likely limits both issues and may also limit the long-term effect of the intended approach. The purpose of the material development is the protection of hydrogen ($H_2$) starvation and shutdown/start-up. All analysis is based on accelerated stress testing, including gas switching. It is not clear how the project intends to show that the catalysts are effective during $H_2$ starvation (which is often due to anode water flooding), and whether these materials suffer a degradation in activity from high water content.
- This project addresses degradation due to start-up/shutdown and cell reversal—key degradation modes in automotive fuel cells—and addresses key barriers of durability and cost. The project is designed to address key steps in the carbon corrosion mechanism at both the anode side and the cathode side, and it provides flexibility and an improved chance of success. The work at 3M is focused on addressing the durability of DOE target catalyst loadings.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.8** for its accomplishments and progress.

- This project shows promising results.
- Excellent progress was made toward improving catalyst stability.
- The objectives have been met and the enhanced durability is evident.
- The accomplishments and progress have been tremendous. The progress ranged from developing and improving SU/SD durability protocols for these types of catalysts to many rotating disk electrode and membrane electrode assembly (MEA) tests to show the durability of these catalysts. The scale-up of the catalysts to short stack and the evaluation of many stacks and MEAs by an independent institution are appreciated and impressive. To test Ru stability, the team should use CO oxidation.
- This project has already demonstrated significant improvements in catalyst durability to start-stop cycling protocols on the cathode, showing less than 10% loss in electrochemically active surface area on the cathode after 5,000 cycles simulating start/stop. This project has demonstrated a three orders of magnitude reduction in the ORR current at the anode and 200 cycles of 200 $mA/cm^2$ simulating cell reversal with anode loadings of 0.045 $mg/cm^2$ PGM with the upper potential remaining <1.8 V. This project has demonstrated improved tolerance to cell reversal in stack testing using the OER modified nanostructured thin film (NSTF) anode at an original equipment manufacturer (OEM). The OEM considers this anode to be "a promising MEA vehicle component" after evaluations including CO-tolerance and freeze-tolerance testing. 3M has demonstrated the importance of including cycles that bring the cell down to normal operating potentials during stop/start durability cycling.

### Question 4: Collaboration and coordination with other institutions

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

- The collaborators are appropriate for the tasks.
- The partners are full participants and the project is considering expanding the role of an automotive OEM from a collaborator to a subcontractor, which will be beneficial in the future stack-level testing.
- This project shows a very good mixture of project participation from national laboratories, academic institutions, and industry. The clear distribution of tasks, regular communication, and data indicates an excellent collaborative effort.
- The collaboration with the Automotive Fuel Cell Cooperation (AFCC) is clear and excellent. It is unclear how much characterization is being done by Oak Ridge National Laboratory. It is entirely unclear what Dalhousie is adding to the project and if Argonne National Laboratory did the OER activity measurements, or what they exactly did for the project.
- The 3M projects tend to confuse collaborators with vendors, which is fully acceptable. One suspects that the real collaborations are within the 3M community, a rather unusually smart group of people who have a long history of invention and progress. The catalyst business is highly proprietary, as fuel cells become "car parts." It is apparent that some cards need to be held close to the chest.

### Question 5: Proposed future work

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

- Future works includes progressing toward meeting DOE 2015 targets combined with fundamental studies.
- It would be nice to include some $H_2$ starvation due to water-induced flooding experiments to see the durability effect of the anode catalysts and if they are stable over operation.
- Future work plans, including fundamental materials studies, engineering to understand interaction with other MEA components, and lowering PGM loading with enhanced durability, are all appropriate.
- Work evaluating the OER catalysts (especially on the cathode) on a dispersed catalyst, such as Pt on a durable carbon support (graphitized carbon?), would be beneficial.
- The results suggest that major program objectives have already been met. One wonders about the contamination implications with this approach. The PGM additives have seen earlier fuel cell applications (and electrolyzer). There is evidence that some additives tend to migrate away from the catalyst or move from the cathode to anode. Most likely they will end up in the membrane. The effects of these new "contaminants" need to be considered. Others who used similar formulations found that the "additives" tended to dissolve and migrate.

### Project strengths:

- This project has a good team, a good approach, and good effort.
- The strength of this project appears to be the 3M and AFCC collaboration.
- The project concept, approach, accomplishments, and collaborators are all strengths.
- This is a comprehensive study leading from fundamental understanding over development to in situ full-size stack testing.
- The concept to attack carbon corrosion by interrupting several steps in the corrosion mechanism is a strength.
- The task successfully addressed key durability issues in PEM stacks. The work was excellent and showed clear direction and progress.

### Project weaknesses:

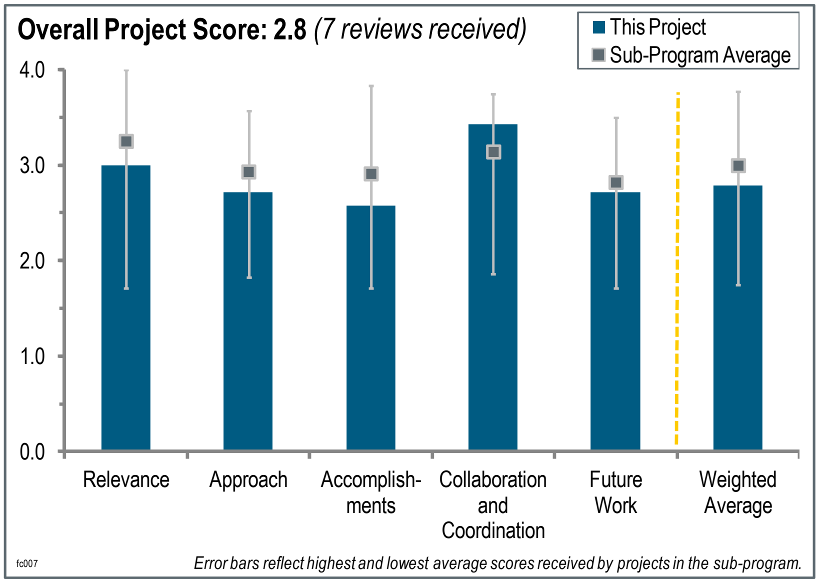
- It is unclear what Dalhoussie is adding to the project. There is no information on other degradation mechanisms, such as $H_2O_2$ formation results, for these catalysts. The accelerated stress tests that are used to evaluate the catalysts (the DOE accelerated test protocol) have been shown to be useful, but they also need to include lower voltages (e.g., 0.1–0.65 V) to test the stability of the added metals.
- There was lots of information on the slides, making it difficult to follow; the oral presentation was difficult to understand as well. For example, the difference between nm1 and n1 cycles was unclear. There is so much color-coding and information that the key messages and significance of the data shown is lost.
- This project has no weaknesses.
- Nothing appears weak in this project.

### Recommendations for additions/deletions to project scope:

- The researchers should include a test that involves an $H_2$/air front on the anode, such as start/stop on a single cell. They should also apply this concept to dispersed catalysts on carbon supports.
- Work evaluating the OER catalysts (especially on the cathode) on a dispersed catalyst, such as Pt on a durable arbon support (graphitized carbon?), would be beneficial.
- Possible contamination should be explored. It would be interesting to compare the cost of this modification to the cost of improved stack management (control of anode and cathode gaseous constituents to preclude oxygen), and to control the stack potential using an external power source (battery).

# Project # FC-007: Extended, Continuous Pt Nanostructures in Thick, Dispersed Electrodes

Bryan Pivovar; National Renewable Energy Laboratory




## Brief Summary of Project:

The objective of this project is to overcome the most critical barriers for fuel cell membrane electrode assembly (MEA) development: durability, cost, and performance. The project works to develop extended thin film electrocatalyst structures for increased activity and durability, and to explore the incorporation of these structures into robust, high-efficiency MEAs. The long-term goal is to incorporate materials with improved mass activity and voltage cycling stability into high-performance electrodes.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- This project addresses the challenges of performance and catalyst durability in a novel and exciting way.
- This project addresses durability and precious metal thrift through novel geometries of platinum catalysts.
- This program is consistent with the DOE project aspects of higher oxygen reduction reaction (ORR) performance to minimize metal loadings. Further, it swings widely to look at a breadth of geometries, catalyst formation methods, support types, etc. The approach is good if sufficient iterations can be tested in a timely manner to glean insights.
- This project is directly relevant to DOE's goals of developing high-activity, durable catalysts for polymer electrolyte membrane fuel cells. The thin film catalyst approach has implications for the durability and performance (especially in improving mass activity) of catalysts that are distinctly different from traditional nanoparticle catalyst systems.
- This project is focused on trying to flush out new versions of one of the most successful approaches identified thus far for effective fuel cell cathodes, namely extended surface area catalysts. In that respect, it addresses the activity and platinum group metal (PGM) loading barriers. High-volume manufacturability and associated costs are apparently not being considered as a criterion at this early stage.
- The project objectives are relevant to DOE's overall objectives, but the project has too many unanswered questions. The deposition method is not finalized, surface areas are still too low, quantities are still at the gram level (10 grams is the most that will be generated by completion of the project), and there is no time to evaluate the MEA if it is even fabricated.
- The relevance of this work is to "focus on overcoming the most critical barriers for fuel cell MEA development," but the experiments are all on rotating disk electrodes (RDEs). There seems to be a major disconnect because RDEs have been shown to have a correlation with MEA results for platinum (Pt)-based/carbon materials, but there is no evidence that any of the RDE work for Pt blacks will correlate to MEAs. The flooded electrolyte condition of the RDE cell makes the whole Pt surface available for H+ and mitigates the role of the Nafion® interconnectivity, which most assuredly will play a large role in the Pt electrochemical active area in an MEA.

## Question 2: Approach to performing the work

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

- This broad approach using several different methods of fabrication is well thought out. This late in the project, there should be some down-selection of the methods most likely to attract a potential supplier so that this work can be scaled up to stack testing in the near future if all targets are met.
- The approach is well founded and based on a strong team. While some of the more esoteric preparations were pursued in order to gain understanding, it may be time for the program to focus on the most promising spontaneous galvanic displacement (SGD), chemical vapor deposition (CVD) and work on making electrodes, the effort of which may be substantial. Using Pt black as a "surrogate" to understand electrode difficulties was a good approach.
- The efforts of this broad-based team are generating many new results and gaining experience discovering the issues with making effective electrodes with a wide variety of non-traditional catalysts. The wide variety of new catalyst forms the researchers have considered successfully demonstrate multiple ways to generate the high-specific activities of extended surface area catalysts.
- The sole focus on RDE is very concerning when the objective of the project is to understand the role of extended Pt structures (with and without carbon) in MEAs. With one year left, the researchers should move as quickly to MEA work as possible.
- The approach is good, although some of the processes are not realistic for manufacturing, but that is fine for understanding potential directions to pursue. The gains made in making optimized Pt black have been around for a number of years and there is no reason to believe further improvement will happen in that area, but they do make a good base case for comparison. Hopefully, more publications will come out that would be more about the catalytic activities than the process of formation. It would be best to test 3M nanostructured thin film (NSTF) to complete the breadth of geometries and types and plot that versus all the other catalyst types. The 3M NSTF does seem to be highly supported by DOE, so it should be a benchmark by which to compare other materials.
- It is not clear how the barriers will be overcome with respect to: (1) increased surface area; (2) a reduction in thickness using the atomic layer deposition (ALD) method (currently at 6nm, needs to be at 1–2nm); (3) the inability for the ALD method to scale up (continuing with this method does not seem reasonable); and (4) catalyst support not being selected. This project is not utilizing the model effort to make decisions/down-selection of catalyst particles, electrode structure, and performance. Very little information was provided on the modeling effort and its benefits, and data should include error bars when presented.
- Though the specific activities of CVD-grown Pt nanotubes are high, there does not appear to be a possible step forward for large-scale synthesis of these structures at volume. The mass activity of the CVD-grown structures is also low. The transition metal templates used for the SGD reaction still contain some of the template material post Pt deposition, and these are highly mobile cations (silver [Ag], copper [Cu]), which can strongly affect fuel cell performance. Images of ALD-grown films show films composed of small grains or Pt nanoparticles, similar in morphology to published images of the 3M NSTF catalyst system. If the growth mechanism for Pt on the chosen substrates is similar, is it unclear what the implications are for the ultimate required Pt thickness to reach high-specific and mass activities while maintaining aspects of thin-film durability.

## Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- Extremely good progress has been made on all fronts. It would be good to understand more about residual Ag in the SGD method and how this may impact both performance and durability. It should be noted that Ag solutions may be classed as toxic, making process scale-up costly.
- The quality and understanding of the complexity of RDE work is transparent and outstanding, but the overall progress seems stunted by the low throughput of materials tested. The presentation communicated more about the means and types of catalyst than the ORR results.
- The researchers have accomplished much, but at approximately 60% complete, it would be good to see a MEA/polarization curve from the best to-date preparation just to get a real sense of progress. Working on

mixing carbon with the catalysts and exploring the impact of Nafion is good progress in preparation for the electrode-making part (MEAs).

- The researchers have made good progress demonstrating many varieties of extended surface catalysts that do indeed have the higher specific and mass activities they are looking for. They are also discovering many issues with learning how to disperse them effectively for both accurate activity characterization and electrode formation. They are also learning new information about how measured surface areas and mass activities may not be a simple function of particle size, as is conventionally believed. This is good information for the whole electrocatalyst community.
- Durability data is still not available, MEAs are not fabricated and tested to-date, and the quantities generated are still too low for meaningful verification. Yield was not discussed during review and it was unclear what the yield of substrates was. This project has investigated and synthesized many novel catalysts with several geometries and has shown good cyclic durability, but at this stage in the project (60% complete) the project needs to focus and down-select the palladium (Pd) deposition method, fabricate MEA, and conduct durability fuel cell testing.
- Batch-to-batch variability for synthesized materials is of concern. The activities reported for the Pt black work, though useful in comparison to the synthesized structures, are different than accepted values in the literature. This project has made positive progress toward improving specific and mass activities of SGD scaled-up materials. There is low total PGM activity of multistep Pd and Au materials. CVD grown materials have not improved catalyst mass activity.
- Excellent progress was made on looking at different Pt extended structures and finding the best materials. But the materials cannot truly be compared until they are cast into MEAs. It is not clear if having high, specific-activity Pt materials by a modified RDE process should be counted as a success, especially because they show that the materials have poor mass activity. If the DOE target for high-specific activity was eliminated and the only goal was mass activity, they would have few accomplishments. If the only goal was mass activity in MEAs, which really is the most relevant parameter, the project would have no accomplishments. This is not to say that the project has not produced good materials, the researchers just need a better practical measure of their accomplishments, which should probably be Pt mass activity in MEAs.

### Question 4: Collaboration and coordination with other institutions

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

- This project has well-coordinated work with other organizations and institutions.
- This project appears to be very well coordinated among the very large team.
- A lot of institutions are involved with good collaboration on this project.
- There has been very good coordination between the teams. It appears some of the lessons from one track are being applied to others.
- The principal investigator (PI) clearly has good command of the activities and the contributions of the various collaborators. Perhaps due to the limited presentation time, it was not clear what the contributions were from three or four of the principal partners.
- An outstanding team has been assembled for this project, with the qualifications/strengths needed to accomplish the goals of the project. It is unclear how much interaction is taking place among the team members. It would be helpful to know how often the team interacts via conference calls, emails, visits, etc.
- There is a large list of collaborators, including national laboratories, industry, and academia, which is very good. It is, however, difficult to see a significant engagement with industry. A catalyst manufacturing company such as TKK or Cabot should become more involved in the scale-up of the materials that show promise, especially in the later stages of the project.

### Question 5: Proposed future work

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

- There should be more focus going forward. The modeling work and the ALD work should be cut and the focus on SGD, and CVD with alloys, should be continued.

- Focusing on a much smaller subset of the approaches investigated to date is the right direction to go. Being able to demonstrate that good performing electrodes can be made and useful fuel cell results can be obtained with at least one of the three approaches (CVD, ALD, or SGD) is critical for the next year of the project.
- Continued studies of the role of carbon in RDE electrodes should be eliminated and the researchers should move all effort to optimizing mass activity in MEAs (via optimization of carbon and Nafion® loading) for the last year. There still may not be enough time.
- Given the suitability of Pt black as a surrogate, and the risk of unknowns in scaling up some of the other avenues, it is questionable whether the team should pursue making high(er) surface area Pt black. A now defunct company called E-TEK was commercially selling high-surface area Pt black with B.E.T. (N2) areas of >45 $m^2$/gram. With the tools and knowledge from some of the solution phase methods here, trying to recreate this material would be worthwhile. Also, a lot of focus should be put on electrode/MEA making as the next step.
- The proposed work is fine. A published paper summarizing the efforts and finding common threads that are not currently being communicated would be helpful. Full fuel cell testing coming at this late stage in the project seems problematic, as there will be a lot of issues in integrating any of them and realizing their full potential in less than a year. Starting earlier with the Pt black would give insights into future issues that may be battled.
- The proposed future work plan is clear for the electrode studies and the modeling effort. The Pt deposition task still continues down the path of evaluating three deposition methods; this task should be down to one method so that optimization can take place during this final year. The ALS method cannot scale up and should be eliminated. The team should down-select between CVD and SGD methods based on the work to date and modeling predictions. The presentation did not address the possibility of catalyst leaching and degrading fuel cell performance and should be evaluated before long-term fuel cell testing.
- With the inclusion of Los Alamos National Laboratory, there is a partner with demonstrated capabilities for incorporating materials into fuel cell MEAs. Though mentioned in last year's comments, there was no discussion of what alternate materials could be used to replace the nanowire templates. The lower-temperature ALD approach seems to be in conflict with the higher-temperature CVD annealing approach with respect to Pt nucleation and growth, as well as surface diffusion of deposited Pt during the formation of catalysts by both methods.

### Project strengths:

- Large teams are able to look at a lot of different materials.
- The researchers are highly capable, well directed, and very knowledgeable.
- This is a distinctly different approach to catalyst development through thin-film methods.
- This is a great research team. Valuable work has been accomplished in learning about the synthesis of novel catalysts and their properties.
- This project's strengths are the breadth of constructions and the quality of RDE work, although that can be a "Catch 22" if only a few samples get tested.
- This is a well coordinated and managed effort across a broad front. There is lots of technical innovation and exciting progress has been made and demonstrated.
- The research team has good depth in the area of carbon-free (Pt) materials and a wide assortment of approaches. This project has good approaches to understanding limitations, such as the dispersion of these novel materials and adding carbon.

### Project weaknesses:

- Too much emphasis has been put on RDE to predict performance in MEAs.
- There is a lack of focus going forward that will not reach a conclusion that has value to the industry. Also, the engagement of catalyst suppliers could be much stronger.
- Electrode (MEA) making appears late in the program. Earlier attempts, even at a small scale, may have assisted in down-selecting from the numerous approaches in the portfolio.
- Sample testing throughput seems slow, based on the limited data; if this is not true, it should be communicated in next year's review. It is understandable why some reviewers might not like the breadth of catalyst types researched, but the breadth is great as long as there are conclusions that can be drawn across platforms.

- The team must down-select a deposition method, generate MEAs, and test them to learn the benefits of electrodes and fuel cell improvements. The researchers also need to incorporate the modeling effort to make decisions.
- The use of readily leachable transition metal nanowire templates is of concern with respect to fuel cell durability. There is an unexplained difference in the project reported Pt black activities versus literature values.

### Recommendations for additions/deletions to project scope:

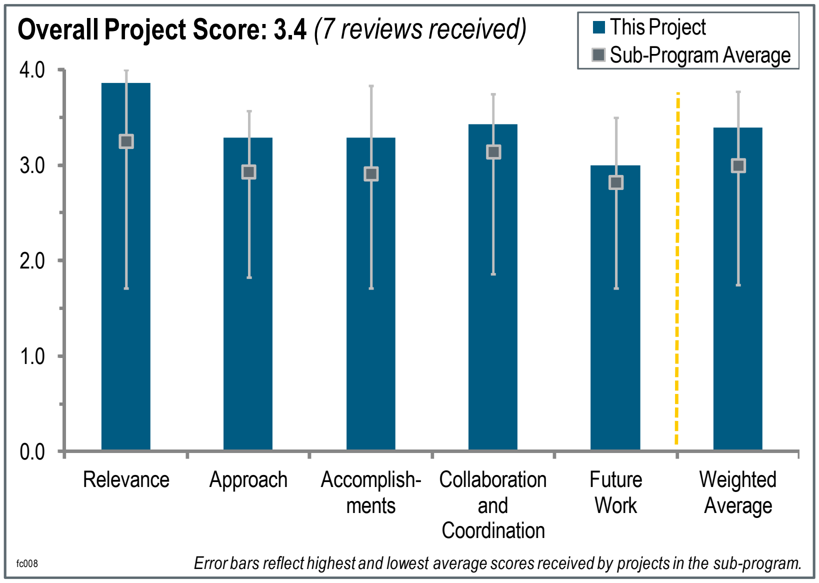
- The researchers should stop doing RDE work.
- The project should focus on one Pt deposition method for future work. The other tasks are reasonable and achievable.
- The researchers should cut out modeling efforts and stop ALD work. They should scale up the SGD method with suppliers and study the effect of residual Ag. The CVD method can be used to try alloys.
- The PI's listed focus on electrode making is endorsed. The project should consider pushing the limits of Pt black. It is questionable whether this group can make a higher surface area version that would be appropriate for the electrode structures they envision.
- The researchers should add 3M NSTF data if at all possible, because it is held as a standard now and is a necessary comparative to draw conclusions.
- A greater emphasis on Pt only or Pt and non-PGM alloy systems to focus improvement on total PGM mass activity would be beneficial.

## Project # FC-008: Nanosegregated Cathode Catalysts with Ultra-Low Platinum Loading

Nenad Markovic; Argonne National Laboratory

### Brief Summary of Project:

The main focus of this project is to develop an understanding of the oxygen reduction reaction (ORR) on multi-metallic systems of PtMN-alloys (M=Co, Ni; N=Fe, Mn, Cr, V, Ti, etc.). This knowledge will lead to the development of highly efficient and durable real-world nanosegregated platinum (Pt)-skin catalysts with low Pt content. Argonne National Laboratory's (ANL's) technical targets exceed or match the 2015 U.S. Department of Energy (DOE) targets for specific activity, mass activity, and electrochemical area loss. The target total content of platinum group metals (PGMs) is less than 0.1 g/kW, which is less than the original DOE target of 0.2 g/kW.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.9** for its relevance to DOE objectives.

- Simultaneous improved catalyst activity and durability is relevant to the program goal. The stretch target is even better.
- This is a very relevant project, with both cost and durability being considered from a fundamental point of view.
- Higher ORR activity is one of the key targets that need to be addressed for lowering the cost of polymer electrolyte membrane (PEM) fuel cells.
- Developing high-performing and durable catalysts, as well as lowering costs by reducing the PGM total content, is highly relevant to the DOE Hydrogen and Fuel Cells Program. The technical targets that the researchers set for themselves are also high.
- This project is outstanding. This thorough work is providing an important framework to guide others in what are the most important factors in making better, more active ORR catalysts.
- Fundamental studies of optimized activity and durability of nanosegregated Pt-skin catalysts are a promising approach to achieve high utilization of expensive Pt catalyst—one of key technical gaps for PEM fuel cell commercialization.
- The overall approach for making Pt skins on top of Pt alloys is relevant from the perspective of attempts to improve mass activity of these catalysts for oxygen reduction. The results reported are a culmination of nanoparticle synthesis and studies conducted on single crystals.

### Question 2: Approach to performing the work

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

- The progress is excellent. It would have been nice to see catalysts in fuel cells sooner in the plan to understand any issues with integration and processing.
- It is good to see preferred crystalline orientation and ternary alloys used to improve material stability in the fuel cell environment.

- This is very careful work, including looking at particles in a disciplined way. Very few places in the world can take such a careful and thorough look at these catalysts.
- It is not clear whether the catalyst preparation is scalable.
- This project has a good approach. It encompasses studies to gain a fundamental understanding of the ORR on the catalyst systems, as well as practical membrane electrode assembly (MEA) fabrication and testing. The techniques used to characterize the catalyst to gain a fundamental understanding are also impressive. In terms of the catalyst synthesis, it would be good to know how easily these catalysts can be synthesized, how much it will cost to synthesize them due to the multiple steps involved, and if the process is scalable. It would also be good to know if the shape and structure of the catalysts are thermodynamically stable.
- The approach is structured and rigorous, uses well-defined model systems, and is based on surface science. This approach is a step above most catalysis work in that it is a forward design process determining the fundamentals of the activity/structure design.
- The stability aspect appears to be addressed adequately. However, there is no indication of other factors, such as mass transport or cost. This work has the appropriate balance of ex situ and modeling compared to fuel cell testing. Although there is a risk that MEA testing has been left too late and new issues will arise, and there has not been a lot of durability work, these efforts should be placed on the catalyst design.
- The approach of this effort is to engender with reproducibility thin films of Pt on top of Pt alloy to effect improvements in ORR activity for PEM fuel cell cathodes. The approach is based on nanoparticle synthesis with capping agents and understanding the increased activity in terms of careful structural studies. This approach though laudatory has to be tempered with realities, such as the challenges of translating these concepts into large-scale synthesis and understanding the issue of a limited potential window where such structures are maintained.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The accomplishments and progress appears to be on target with the goals and objectives of this effort. There seem to be nice, incremental improvements being made.
- The project showed preferred performance with ternary alloy. It is necessary to investigate the mechanism of catalyst activity improvement. It needs material stability under the fuel cell environment.
- The accomplishments presented are excellent and impressive. More fuel cell performance and durability testing would be good, which seems to be the plan for future work. It is great how the principal investigator (PI) ties the fundamental understanding gained to designing better catalysts.
- Between the 2011 and 2012 presentations, there are many results that are shown again, sometimes just reordered. While the quality of work is excellent, it would be nice to get a sense of the quantity of work and how many catalysts are generated, characterized, and tested. There are references to 3M nanostructured thin film (NSTF) catalysts, but there are no comparisons between that system, the nanoparticle approach, and conventional catalysts to get a true sense of the amount of change in activities seen.
- The activity and durability results are great. The level of understanding this project brings should not be ignored. Some may complain about the cost of this kind of work, and they should not. This is remarkably important and there are few places that can be as productive as this group, which happens to work at a national laboratory, which is expensive.
- The project is achieving results at a suitable pace. The reaction pathways and mechanism have been confirmed, but more understanding is required on the segregation profile.
- Whereas the catalyst has been prepared in an MEA with General Motors (GM), only mass activity was shown, but not the polarization curve. This may indicate problems with mass transport or ohmic losses.
- The practicality of scale-up is not clear, but the direction has been changed to improve this.
- The project was able to build on experience with binary systems to develop a shortcut approach for ternary systems, in order to get the desired structures.
- The stability of the catalyst was assessed at 0.6–0.95 V, with minimal change in catalyst structure after 20,000 cycles. A higher upper potential limit needs to be assessed. While a fundamental approach is very good and needed, an increased effort of understanding MEA scale effects should be introduced at this point in the project.
- ORR activity in rotating disk electrodes (RDEs) has not been correlated to that in MEAs of subscale fuel cells. Even though durability has been reported in MEAs, the ORR activity in MEAs has not been clearly

reported and compared to the extremely high values reported in the RDE half cell setup. It appears that the activity is no different than that of Pt/carbon in MEAs, based on incomplete information of the conditions in the slide on MEA durability studies.
- It seems likely that the activity in MEAs will be lower than those in a fuel cell based on the typical experience of the research community. Milestone 1.2 on ORR mechanisms has apparently been met, but simply obtaining a Tafel plot slope is not evidence for having accomplished this milestone.

### Question 4: Collaboration and coordination with other institutions

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

- The project has appropriate partners and they seem to be well coordinated.
- The researchers are working with the best.
- ANL has assembled a good team; however, it is not clear the extent to which some team members are contributing.
- This project has good collaborations with 3M and others; this is a bonus. The work is so valuable that reviewers should give them extra points for reaching out to industry.
- Good collaborations seem to be the case here, though the role of the Jet Propulsion Laboratory (JPL) is not clear in terms of the results presented, and neither are the roles of Brown University and the University of Pittsburgh.
- GM has tested durability but not reported the activity compared to the baseline Pt catalyst typically used in real-world fuel cells.

### Question 5: Proposed future work

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

- This project has well-targeted improvements that are not only scientifically important, but relevant.
- The plan forward looks appropriate, with the exception that the practicality of scale-up of the structures is not clear.
- Putting catalysts in fuel cells sooner would be better, because it will take some development to get the catalyst to reach its optimal performance.
- The proposed future work sounds very much like an incremental effort toward expectedly same results. No new ideas or concepts seem to manifest in the future efforts.
- It would be good to see more transmission electron microscopy, RDE, and fuel cell results for the catalysts on a carbon support. It would be good to know how high or low of a catalyst loading on carbon can be prepared. Some evidence that the catalyst shape and structure do not change with time would be valuable.
- ANL should focus on the materials stability of the ternary alloy catalyst, which shows high activity. It is questionable that the PI can take enough time for alternative approaches for fabrication of thin-film nanoscale catalysts with ultralow PGM content.
- The ORR activity of the new catalysts in MEA versus that in RDE is of primary importance. The values reported in RDE are extremely high and unlikely to be reproduced in a fuel cell. Independent laboratories need to evaluate the electrocatalysts in RDE and MEA to ascertain the ORR activity and ECA.

### Project strengths:

- The catalyst fundamentals are excellent.
- This project has good material synthesis and analysis capabilities.
- The researchers take a careful and disciplined approach.
- The novel electrocatalysts and preparation method with apparent high activity are strengths of this project.
- This project provides a very structured and fundamental approach to establishing active and durable catalysts using ternary systems.
- The project team is of the highest caliber.
- Project strengths involve the ability to control Pt alloy nanoparticle morphology and surface structure to engender high mass activity for ORR. This is carefully done in the context of studies conducted using single

crystals and nanoparticle synthesis, and advanced tools and methods, such as synchrotron-based X-ray absorption.

### Project weaknesses:

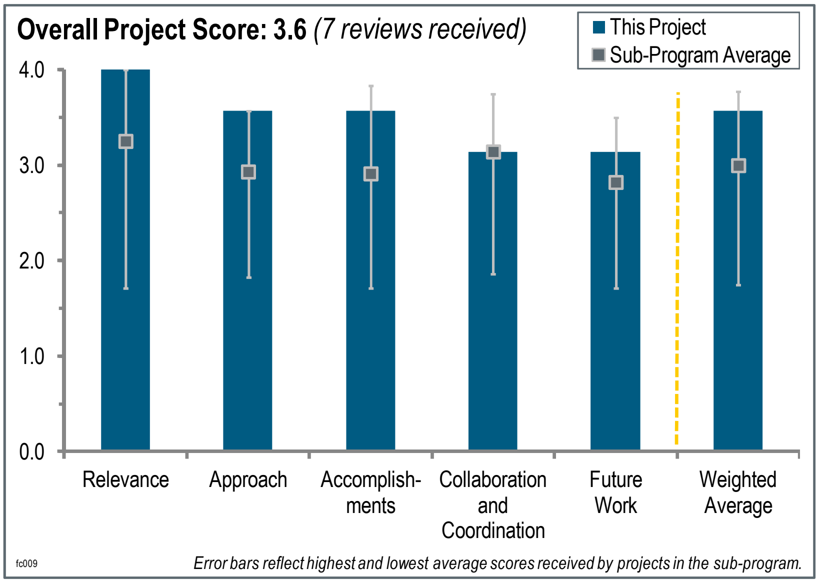
- Reliance on RDE may not map quickly into fuel cells.
- The principal weakness in this project is that this proposed effort is a marginal improvement over decades of similar work and is quickly getting dated.
- Expected costs and manufacturability of the catalyst need to be addressed. Also, MEA testing has been minimal.
- The scalability of manufacturing of the electrocatalysts has not been explored. Support interaction and durability with the electrocatalyst need to be evaluated to obtain a working catalyst.

### Recommendations for additions/deletions to project scope:

- A potential manufacturing partner and cost should be addressed.
- ANL should increase MEA testing and include a feasibility assessment on practical catalyst manufacturing.
- ANL needs an independent laboratory to evaluate the ORR activity in both MEAs and RDEs.
- This type of work should be a larger fraction of the portfolio, with this type of electrocatalysis at the center. The science this group produces is a national treasure and we are not supporting it well enough.
- The roles of the partners mentioned in this effort are not clear. For example, the role of JPL, which mentions a combinatorial approach, is at odds with the thought-based approach being shown in this effort.

## Project # FC-009: Contiguous Platinum Monolayer Oxygen Reduction Electrocatalysts on High-Stability, Low-Cost Supports

Radoslav Adzic; Brookhaven National Laboratory




**Brief Summary of Project:**
The objectives of this project are to: (1) develop high-performance fuel cell electrocatalysts for the oxygen reduction reaction (ORR) that are comprised of a contiguous platinum (Pt) monolayer on a stable, inexpensive metal or alloy nanostructure; (2) increase the activity and stability of the Pt monolayer shell and the stability of supporting cores, while reducing the content of noble metals; (3) maximize Pt utilization to use every Pt atom; (4) scale up the conventional synthesis of ultra-thin palladium (Pd) alloys (refractory metals or gold [Au]) nanowires or hollow nanoparticles as supports for a Pt monolayer; and (5) scale up synthesis based entirely on electrodeposition of nanowires or nanorods.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **4.0** for its relevance to DOE objectives.

- This project is critical. It has the best chance to lower Pt loadings to $\mu g/cm^2$ quantities.
- This project is in full alignment with DOE research and development objectives. It targets reduced Pt loading and cost while increasing activity and durability.
- The development of low platinum group metal (PGM)-loaded electrodes is absolutely essential to enable the commercialization of light-duty fuel cell electric vehicles.
- The project is very well conceived and is relevant to the DOE Hydrogen and Fuel Cells Program goals, specifically the goals related to enhancing catalytic activity and stability.
- This project does a very nice job of looking at derivatives of Brookhaven National Laboratory (BNL) core shell technology to address DOE activity objectives for Pt.
- This project provides one of the most promising pathways to reduce the use of expensive Pt in fuel cells and thus could allow fuel cells to become cost competitive with other power sources for a range of applications.
- BNL has made considerable strides in reducing total precious metal use, as well as reducing Pt. The activities are finally consistently listed both against Pt and against the total PGM use.
- BNL has succeeded in transferring its technology to a catalyst supplier (albeit not the supplier that is a formal partner within the project) that is making sufficient quantities of catalysts available to enable thorough evaluations of the concepts to be made under actual working conditions of applications.
- The most impactful possible development toward the commercialization of hydrogen ($H_2$) fuel cell vehicles is the development of high-activity, durable catalysts. BNL is working on exactly that topic.
- BNL understands that high activity must imply the reduction of Pt by expressing the activity in units of amps per mass of Pt. BNL understands that high activity must also imply the reduction of all precious metals, including Pd, ruthenium (Ru), and Au. However, there could be a better focus on the elimination of other precious metals.

### Question 2: Approach to performing the work

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

- This project has a clear set of goals to be accomplished by a clear set of pathways. The approach is sound and the results show that it is successful.
- The approach appears to be successful, as seen from the catalyst activity gains. The team seems to leverage basic research funding well to do complementary theory, which helps the project.
- There was no discussion of the approach to be used by Johnson-Matthey Fuel Cells, Inc. (JMFC) for catalyst scale-up.
- BNL's approach to develop core-shell nanoparticles is one of the most promising concepts to date for both maximizing the fraction of Pt available for the ORR reaction and enhancing its stabilization. Refractory-alloy and/or hollow particles could do a lot to reduce total PGM. It would be good to see continued effort on the hollow particle approach that BNL does not appear to be pursuing, because it eliminated the need for Au or Pd. It is nice to see membrane electrode assembly (MEA) testing done on promising concepts. The multiwalled nanotubes (MWNTs) work is a duplication of other efforts.
- The approach is very well conceived. There is a strong emphasis on fundamentals and there is a pathway presented for scale-up of promising systems. There are clear underlying hypotheses presented, and a well-designed research approach to test each hypothesis.
- The barriers addressed are very relevant and the targets set are challenging.
- Multiple pathways are being followed to reduce the total precious metal, so the project is not dependent upon any one approach working.
- The use of refractory metals in the core, alloyed with limited amounts of precious metals, could finally put to rest longstanding concerns about the amounts of precious metals used in the cores.
- Extensive use of Pd in the cores of many of the systems being studied raise concerns of a return to 2000–2001, when Pd was more expensive than Pt on a mass basis; however, the lesser density of Pd made it a bargain versus Pt, even under those unusual and unlikely-to-be-replicated pricing conditions. (There are twice as many Pd atoms as Pt atoms per gram, with a similar volume per atom.)
- The researchers should be wary of the false assumption that (111) crystal faces always give higher ORR activities. While this appears to be true for Pt alloys, it is not true for pure Pt.
- While some examples are included, BNL needs to provide more focus on cheaper core materials, such as those with hollow cores or more refractory cores (e.g., NiW).
- The approach is somewhat compromised by the tendency to use metals that, while capable of stabilizing a monolayer of Pt, may themselves be unstable in a fuel cell cathode environment; examples include Pd and nickel (Ni). To the principal investigator's (PI's) credit, there is an attempt to stabilize a Pd core with Au. However, the use of Au again prompts questions about cost and the usage of other precious metals.
- Modeling efforts to predict stability need to incorporate a more realistic environment that accounts for the presence of oxygen ($O_2$) and water. While the approaches of other projects may more clearly show a roadmap toward high-specific activity using thin-film structures, the approach of this project is worthwhile due to the need to see if nanoparticles and related morphologies can yield high activity.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- BNL has done an excellent job working with industry (General Motors [GM]) in the practical evaluation of catalysts and the evaluation of catalyst stability. Also, kudos for the transition of the BNL catalyst technology to industry.
- BNL has made very good progress toward the targets of the program. Specifically, the demonstration of high MEA performance with very low loading is impressive and shows applicability.
- BNL's fuel cell data on Pt/Pd/Au look encouraging. The GM data on the Pt/Pd is a decent first step, but significant improvements in activity are still required. The voltage cycling results are also encouraging, although the Pt/C MEA baseline seems surprisingly low performing by comparison. PtNi nanoparticles on MWNTs are showing high activities, but stability is unproven.

- The primary issue here is the noble metal content (Pd, Au) in the core of the catalyst. Minimizing Pt content is of little use if it is supported on an Au core. However, the project team is well aware of this issue and is making significant progress in modifying the core composition. The carbon nanotube approach looks promising.
- The team has made very good progress using multiple approaches. The use of Pt monolayers deposited on stand-alone or alloy cores has shown promising results. Accomplishments have been validated by MEA performance evaluation, yielding very encouraging results.
- The durability of the system has been demonstrated through potential cycling tests.
- The progress on refractory-metal-alloy cores is very encouraging. BNL's demonstration of a process to put down high-quality Pt monolayers without an intermediate copper underpotential deposition step could greatly reduce manufacturing costs and make this catalyst technology more widely accessible. BNL has made good progress on extending the testing of activity and durability in MEAs, which complements the rotating disk electrodes (RDEs) used in the past. This trend toward working with practical MEAs should be continued.
- More attention could be paid to the hollow Pt catalysts that were described several years ago, as these fully avoid residual concerns about the durability of any system containing Pd that are held by catalyst technologists with considerable Pd experience.
- Results for catalysts that include PGMs other than Pt need to have mass activity reported in A/mg PGM. Out of approximately seven distinct families of catalysts reporting, only three show A/mg PGM (Pt/Ru, Pt/Pd, and Pt/PdW). This needs to be done for the rest.
- Performance of the PtML/Pd/NiW catalyst needs to be reported in mass and specific activities to allow comparison with other catalysts. The in situ results shown are iR-free using $O_2$ at the cathode, and as such, they appear poor.
- Only stability results are shown for a catalyst that likely has poor total precious-metal-basis mass activity (Pt/PdAu). It would be interesting to see stability results for high-mass-activity PtNi supported on MWNTs, as well as anything involving a cheap core (e.g., PtML/Pd/NiW).
- BNL showed high-activity results for hollow core materials in past years, but there has been little follow up. This needs to be done.
- At present, no individual catalyst has been developed that is known to meet both cost and durability targets.

### Question 4: Collaboration and coordination with other institutions

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

- This project has a large team of partners and collaborators from industry, academia, and one national laboratory. The team provides the large pool of resources necessary to perform the intended work.
- It was nice to see MEA testing done and at original equipment manufacturer (OEM) (GM). Pt/Pd nanoparticles are being scaled up by a catalyst supplier. Massachusetts Institute of Technology (MIT) has done some work on PtNi nanoparticles on MWNTs. It was unclear from the presentation if there are key contributions from JMFC and UTC Power at this point.
- BNL showed good collaboration with industry. The extent and nature of collaboration with academic partners was not quite as clear. Perhaps the details of this collaboration will be presented on the next occasion.
- Next year (2013) should be a very important year for this project, and collaborations will be critical in areas such as scale-up of synthesis, technology transfer to licensee, and MEA and stack testing. All these activities depend on successful collaborations. If this team cannot successfully transfer and scale its technology, then a very promising approach will have been wasted.
- The roles of UTC Power and JMFC are not clear, and the MEAs appeared to be tested by GM. Because UTC Power is a Cooperative Research and Development Agreement (CRADA) partner, this may not be a significant issue, but hopefully in the last year of the project, the objectives of UTC Power can be met.
- There is excellent coordination of theory and MEA evaluation, especially as it seems to be at no cost to this project.
- There has been no apparent contribution in this year's slides from funded partner JMFC. Technology transfer and scale-up have occurred, but to a manufacturer outside the project that does not have production facilities in the United States.
- The licensing of BNL's monolayer catalyst technology to a catalyst supplier outside the project should make this material generally available to the industry. However, it is not clear that formal protection and licensing of intellectual property (IP) generated by national laboratories, rather than putting the information into the public domain, is the most efficient pathway to attain national technical goals; it has, in this case, inhibited similar scale-up and commercialization efforts by other competent manufacturers.

- MIT has done interesting work under the project, but no coordination between the MIT efforts and the main project at BNL was evident from the presentation.
- Contributions of partner UTC Power were not clear in past work, but they should become important in future MEA testing. There has also been effective collaboration with several organizations outside the formal project.
- The collaborations that involve MEA fabrication and fuel cell testing have resulted in data that generate quite a challenge in comparison to the program targets. The polarization data associated with Pt/PdAu nanorods and PtML/Pd/NiW are shown for very low loadings (0.027 and <0.04 mg Pt/cm$^2$, respectively), but also with iR-free potentials and $O_2$ as the cathode oxidant. It would be much better to use a loading, such as 0.075 mg PGM/cm$^2$ (which would total 0.125 mg PGM/cm$^2$ if combined with a 0.05 mg Pt/cm$^2$ anode), and then report both iR-free and cell voltage using air. This kind of experiment could yield data that would be useful for comparison.
- Collaborations on fuel cell testing should also aim to provide mass and specific activities in situ.
- Modeling efforts do not appear to be on track to provide useful information. They also need to require a realistic environment ($O_2$ and water).

### Question 5: Proposed future work

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

- The proposed future work is in line with the project requirements.
- The proposed future work is exactly what it should be. One hopes that the team can stick to this plan and not get sidetracked.
- It is not clear from here what happens to the MIT catalysts.
- The team should focus on MEA development for the last year. The tech transfer of the catalyst IP to industry was well done.
- It would be beneficial to understand how the progress currently made in this project would actually translate into a cost reduction of fuel cells. The project team should give an estimate of the potential cost benefits.
- BNL should emphasize Pt on refractory alloy cores and hollow Pt nanoparticles rather than Au- and Pd-based cores. A lot of the future work seems to focus on the nanowire supports, on which it will likely be more difficult to create very thin Pt films. There are also already other projects (National Renewable Energy Laboratory) focusing on these types of materials. BNL needs to keep stressing MEA tests and work to understand what gives high durability. Also, there should be increased focus on high current density performance.
- BNL needs to focus on scaling up the best precious-metal-basis mass activity materials, provided there is an expectation of stability. These materials should include the hollow cores, which have been reported in years past. Thankfully, returning to the hollow nanoparticles is mentioned in the future work for 2012.
- BNL should look at the stability of the PtNi nanoparticles on MWNTs before scaling up.
- The Pt/PdAu nanowire precious metal mass activity should be reported before there is any decision to scale up the production of this catalyst.
- Refractory core work should continue as noted.
- Refractory-alloy cores are being properly emphasized for future work.
- BNL should apply plans for microemulsion synthesis work on hollow Pd to hollow-Pt catalysts that avoid possible durability issues with Pd.
- It is not clear that the increased emphasis on nanotube and platelet cores is warranted, as the project claims benefits for (111) orientations of pure-Pt systems that have not been borne out by careful single-crystal experiments.
- Increased emphasis should be placed on the detailed characterization of these catalysts after they have been tested in MEAs for activity and durability; it is not clear that the active materials are the same as those characterized before testing.
- Further emphasis should be placed on understanding why the electrochemical surface areas of these materials are in the range of approximately 120 m$^2$/g Pt rather than the 240 m$^2$/g Pt that would be expected for a true monolayer catalyst.
- The researchers should be wary of overreliance on electrodeposition as a means of depositing catalysts onto diffusion media, as electrochemical methods are typically harder to scale up than chemical methods.
- The researchers should keep focused on durability testing under a range of conditions (including extended times at the $H_2$ potential) and further characterize the effects of core materials that may escape into the ionomer and then to other parts of the cell.

## Project strengths:

- This project has a good approach and thorough execution.
- The potential for order of magnitude reduction in Pt loading is a strength.
- The culmination of BNL's excellent catalyst work is playing out well in practical MEAs.
- This project has very promising catalyst concepts already scaled up for MEA testing at an OEM. Initial durability results are very encouraging. There was good engagement with both catalyst suppliers and end users. Thus, scalability is being considered.
- This is an excellent team and an outstanding PI. There is a strong focus on fundamentals and there are excellent results attesting to progress. There is an emphasis on stability in addition to activity.
- There is a good commitment to scale-up and interaction with industry partners.
- The creative choice of catalyst systems, synthesis, and pre-testing characterization of the catalysts is a strength of this project. There was effective technology transfer to get Pd-core catalysts scaled up and into the hands of industry in adequate quantities for thorough testing under a wide range of conditions.
- The researchers are making progress on testing materials in MEAs, though this needs continuing emphasis.
- The durability results suggest improved uniformity of Pt monolayers versus earlier efforts.
- BNL is able to leverage the investigators' well-established expertise in generating Pt monolayers on a variety of materials.
- BNL is able to quickly churn through the synthesis of a wide variety of catalyst families.
- RDE techniques and other experimental techniques are well understood and implemented.

## Project weaknesses:

- BNL needs to focus on improving materials stability.
- The impact of the modeling work was unclear. The most developed concepts still use PGMs (Au and Pd) in the cores. BNL seems to have abandoned the promising hollow core Pt nanoparticle concept.
- The nature of collaboration with academic partners and their roles was not very clear.
- The durability tests employed should perhaps be revisited.
- Further description of the methods needed to get improved uniformity of Pt monolayers should be given, accompanied by detailed characterization showing these improvements. Surmising increased uniformity merely from durability data is not enough.
- Noble metal (Pd, Au) use in catalyst core is a weakness of this project. There is some reluctance to work aggressively on changing the cores. The Pd or Au used maybe relatively small ($mg/cm^2$), but if the total noble metal content is similar to conventional catalysts, the benefits of this project are essentially wasted.
- BNL often fails to recognize that the inclusion of lower-cost precious metals will not in fact result in lower costs when catalysts are commercialized. BNL needs to be better focused on thrifting all precious metals.
- The nanoparticle focus puts the project at a disadvantage of usually beginning with materials that are going to be low for specific activity.
- The modeling assumptions were not well defined.
- The fuel cell data have not been generated in a manner that is easy to compare to program targets.

## Recommendations for additions/deletions to project scope:

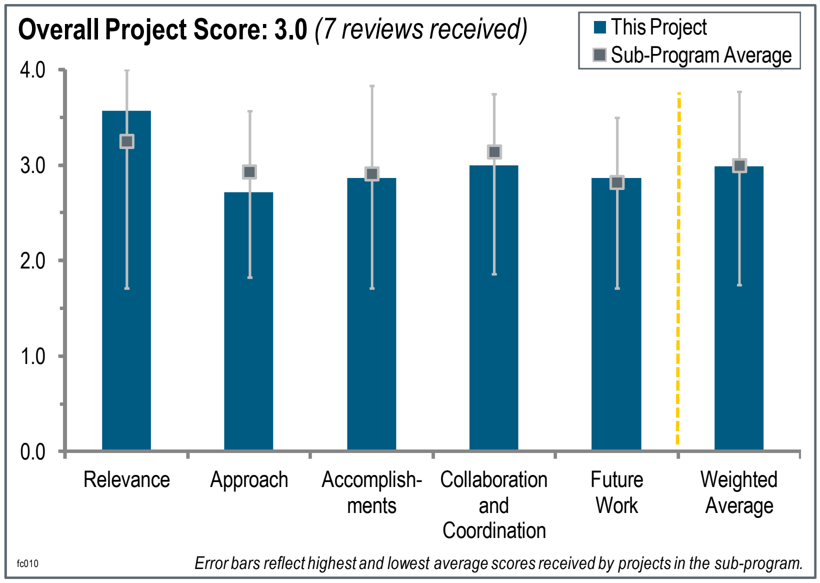
- This is a very nicely structured project and should continue as per the scheduled plan/scope.
- BNL should evaluate the MIT catalysts in MEAs.
- This project should have less of a focus on Au/Pd-containing cores and nanowire supports. More focus should be on hollow and refractory metal cores. BNL should continue MEA testing with an emphasis on durability and high-current density performance.
- BNL should focus exclusively on: (1) cheap cores, hollow and refractory, and (2) other catalysts that have demonstrated high mass activity on a precious metals basis. All other material developments should be deleted.
- The modeling activity should be deleted because the prospects are low that it will provide quality information toward predicting stability.

# Project # FC-010: The Science and Engineering of Durable Ultralow PGM Catalysts

Fernando Garzon; Los Alamos National Laboratory

## Brief Summary of Project:

The goals of this project are to: (1) develop durable high-mass-activity platinum group metal (PGM) cathode catalysts to enable lower-cost fuel cells; (2) elucidate the fundamental relationships between PGM catalyst shape, particle size, and activity to help design better catalysts; (3) optimize the cathode electrode layer to maximize the performance of PGM catalysts for improved fuel cell performance and lower cost; (4) understand the performance degradation mechanisms of high-mass-activity cathode catalysts to improve catalyst design; and (5) develop and test fuel cells using ultra-low loading, high-activity PGM catalysts.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- This project supports DOE's objectives to lower costs and increase durability of the cathode catalysts for polymer electrolyte membrane (PEM) fuel cells.
- The stated objectives will deliver a high-performing, durable catalyst that can be made to deliver these attributes in a practical fuel cell. There appears to be no clear path to meet that objective half way through the project.
- This project is directly relevant to the overall DOE objectives. Los Alamos National Laboratory (LANL) is working to generate a stable, lower-cost cell catalyst.
- Membrane electrode assembly (MEA) cathodes typically consume two-thirds of catalysts and are a good target for cost reduction for both quantity and material design.
- Understanding the durability of low-loaded PGM catalysts is essential to enable commercialization of fuel cell vehicles.
- The reduction of total platinum (Pt) loading in the fuel cell stack below 10 g is vital for fuel cell vehicle commercialization. This project is clearly aligned with this target and its development of durable, ultra-low Pt alloy cathode catalysts.

## Question 2: Approach to performing the work

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

- The options should be assessed against a metric, prioritized, and down-selected.
- The science within each topic or task within the project is fine, but there is no coordination between these tasks so that the overall effect is the simple dilution of resources and effort.
- The approach appears to be rational and it should be expected that the project will yield interesting and useful results.
- The basic approach of using modeling to complement the experimental work is a very valid one. In-depth analytical support is also a great asset to this project. Some of the catalyst ideas are quite novel and interesting,

such as metal nanotubes and Pt-Ce-carbon. However, others are well understood and are not novel, such as Vulcan v. Norit carbon supports and ethylene glycol studies.

- The approach is well designed, as it combines both theoretical and experimental components. If it is a modeling-driven approach, model validation should occur in parallel to model development to ensure that the modeling assumptions are correct. It would also help to correct modeling assumptions at the early stage.
- This project needs greater emphasis on model validation in parallel with good performance in electrochemical characterization. There may never be good performance, but the models could be validated. The researchers need some tests just to validate the model, even if the results are not good. There is not enough single-cell testing being performed, because not enough materials are getting through the initial screening.
- It is unclear how the current dispersed work streams will improve the understanding of ultralow PGM catalysts. The density functional theory (DFT) studies on nanotubes will only have value if those structures can be made and tested to validate the model. The microstructural modeling needs to be validated for standard electrodes first before using it to investigate ultralow PGM catalysts. It is not clear how the CeO mitigation addresses specific ultralow PGM catalysts issues. It is also not clear how the ethylene glycol method enables ultralow PGM catalysts. The nanoplatelet work is the one promising area of the project. The accelerated stress test (AST) work is being done elsewhere, and developing ASTs for ultralow PGM catalysts will not be fruitful until there is field data to validate failure mechanisms from these ASTs.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The conclusions are unclear conclusions. Only qualitative statements are given and hard data are missing.
- The accomplishments to date in modeling, nucleation growth studies, and synthesis and characterization are interesting. It is unclear if the use of yttrium (Y) actually reduces the cost of the catalyst.
- Significant progress has been made in advanced modeling, and modest progress has been made in overcoming barriers in catalyst synthesis.
- The modeling work has progressed well, but the experimental reduction to practice has lagged behind. There appears to be little integration of the modeling work into the catalyst work.
- The $Pt_3Sc$ and $Pt_3Y$ work has not really started.
- Overall, a lot of great work has been done on this project, all the way from modeling to analytical characterization to fuel cell testing.
- Perhaps in this particular presentation, too much emphasis was put on the modeling work and not enough was put on the actual experiments.
- The work done on Norit and Vulcan has already been done before, so it is unclear what the principal investigator is bringing that is new or exciting to the community.
- The mass activities for both the polypyrrole (PPY) and TKK benchmark appear too low.
- Pt-ceria-carbon catalysts are very unique with a lot of potential, and the analytical work thus far is quite positive. It will be exciting to see the future results.
- Many structural models are being generated, but not enough are being tested and initial results are limited. The project is 60% complete, but only limited testing has been performed to indicate progress toward objectives. The test results shown do not indicate any significant progress. Very limited cell and no stack testing has been performed.
- There is a smattering of results from the various parts of the project. All of the results seem reasonable, but it is not clear how they will, either independently or as a whole, lead to the development of an ultralow PGM catalysts with the exception of the PtY and PtSc alloy catalysts, which show some clear activity benefits. The PPY-Pt materials seem unlikely to meet activity targets.

### Question 4: Collaboration and coordination with other institutions

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

- This project has good collaboration among national laboratories, universities, and industry.
- The collaboration and coordination with other institutions appear appropriate for this type of project.
- There seems to be little coordination between the groups.

- All of the subcontractors appear to be making contributions to the project. It is not clear that there is a strong interaction among all of the partners.
- An exceptional team has been brought together with universities, national laboratories, and stack original equipment manufacturers. Collaboration between all of the partners is well described and coordination/interaction appears to be very high.
- This project is dominated by national laboratories and universities. It would be good to see more involvement with the catalyst industry if there is any chance of taking the success of this project beyond the academic curiosity.
- Collaboration within the project team appears to be on track, but only limited coordination is shown with non-team members. Catalyst and MEA manufacturers should be included somehow, despite proprietary information restrictions (maybe as peer reviewers).

### Question 5: Proposed future work

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

- The proposed future work appears logical.
- So far this seems to be a bunch of independent projects. The future plans do not include tying the work streams (i.e., materials and modeling work) together or down-selecting promising concepts.
- The future is focused on three areas of high potential: Pt/Ce/C, Pt/PPY, and Pt/Y, Sc with modeling support. More experimental work and less modeling is great. Additional fuel cell testing will be a great addition to the work thus far.
- Overall, the plans are built on achieved progress to date.
- The plans need to be more focused on model validation. Prior to extending the DFT model to catalyst coated nanotubes and nanowires, model predictions should be validated on uncoated nanotubes and nanowires. Prior to Pt/PPY fuel cell testing, the catalyst's activity needs to be properly evaluated by rotating disk electrode (RDE). Prior to moving to Pt/Y, Sc nanoplatelet research, the activity of Pt/Y, Sc nanoparticles needs to be properly evaluated by RDE. This project needs to be more focused on DOE targets with respect to mass activities.
- The PPY/Pt work is irrelevant and should be stopped. There should be more focus on tasks with the project so that the resources are not diluted too much.
- Decision points versus a schedule or budget are not addressed in the presentation. The greatest weakness of work to date is the minimal level of cell and stack testing. This is addressed at an increased level in the proposed future work. Verification of models needs further attention in future plans. The relation between DOE and Ballard Power Systems AST protocols should be related to actual test data, and one or the other selected for screening.

### Project strengths:

- The strengths of this project are the knowledge and dedication of the project members.
- This project has a strong consortium in principle.
- LANL has assembled and managed a great team.
- Some of the catalysts investigated are really interesting and the complementary modeling work is very useful.
- Future fuel cell testing will be a great addition.
- Using a theoretical approach as guidance for the synthesis of new catalysts and for building more efficient catalyst layers is a strength of this project.
- A strong team of national laboratories and universities has been assembled.
- This project's greatest strength is the development and verification of electrochemical and structural models with test data. Hopefully, there will be progress toward performance and durability targets.
- This project has highly competent researchers with outstanding facilities. The preliminary results on Pt/Y, Sc nanoplatelet catalysts are promising.

### Project weaknesses:

- This project has little coordination, and conclusions and down-selection seem to be missing.

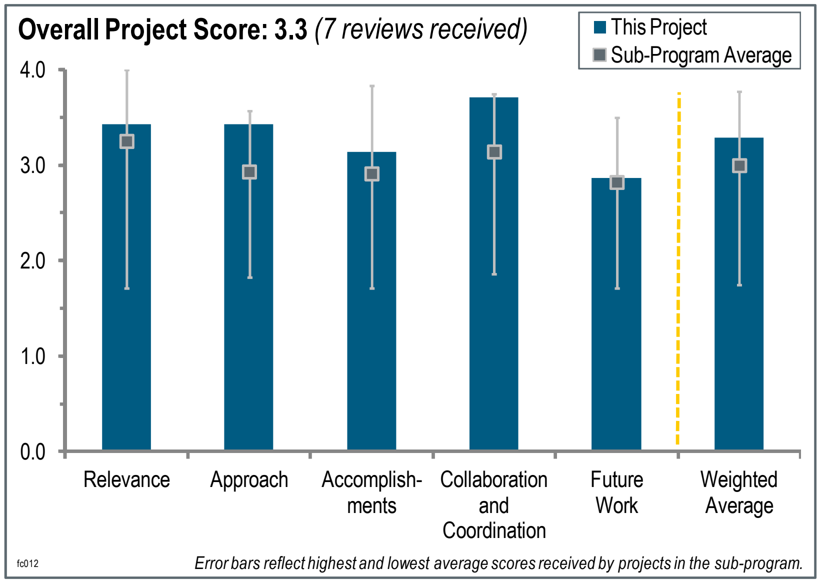
- The Vulcan/Norit work is not the best use of time. There is perhaps too much emphasis on modeling work.
- RDE activities are too low and need to be brought in line with literature values.
- There is a disconnect between modeling efforts and experimental work.
- There is limited involvement with industry. This means the project will have a limited impact on the industry if it was successful.
- The individual tasks are in silos, with no integration or down-selection planned.
- LANL has yet to show that any of the materials/structural designs can even meet the performance and durability levels of currently used cathode catalysts at costs comparable to current products, never mind at reduced costs. There is no discussion or analysis of costs for the tested materials/structural designs.
- There is a lack of clear indication of how the isolated projects tie together. Several tasks, such as ASTs, $CeO_2$ studies, and the Ballard electrode model, do not seem directly relevant to the development of ultralow PGM catalysts. The model validation plans are unclear.
- Model validation follows model development. It is unclear if the authors are planning to validate the model predictions that nanotubes are more susceptible to electrochemical dissolution than Pt (111). It is unclear if the researchers are planning to perform experiments on single crystals.
- Electron-beam techniques are not scalable for the synthesis of small-diameter nanotubes. It is unclear if there is a scalable approach for synthesis of small-diameter Pt nanotubes.
- With respect to RDE characterization of $Pt_3Y$ and $Pt_3Sc$ and bulk catalysts, the advantage of $Pt_3Y$ and $Pt_3Sc$ over Pt is not obvious, since the Pt standard has poor quality. Mass activities of PtY/C and PtSc/C catalysts are not shown, probably because they are low.
- With respect to RDE characterization of PPY Pt catalysts, the thin-film limit is not fulfilled, meaning that activities cannot be properly evaluated.

### Recommendations for additions/deletions to project scope:

- The researchers should continue the good work.
- The presentation slides were far too busy.
- This project is doing well. The only real comment is to continue to focus in on the new, novel high-potential catalysts shown on slide 26 along with more experimental work and more complementary modeling work.
- The project needs to be more focused with respect to catalysts research. The researchers should focus on three areas: nanotubes and nanoplatelets, alloys, and pyrolized PPY Pt catalysts. This will leave behind Pt-ceria catalysts and studies of Pt nucleation and growth on carbons.
- The researchers should stop work on PPY/Pt and seek to focus efforts on the most favorable approaches. The researchers should also integrate the modeling into the catalyst design. If no Pt nanotubes are being made, there seems to be no reason to do this work. It seems like this should be part of the work at the National Renewable Energy Laboratory (FC-007).
- There needs to be greater emphasis on testing at the cell and stack levels and feedback to the modelers and theoreticians. The involvement of cathode material and MEA manufacturers would help to keep the focus on cost and durability.

## Project # FC-012: Polymer Electrolyte Fuel Cell Lifetime Limitations: The Role of Electrocatalyst Degradation

Deborah Myers; Argonne National Laboratory




### Brief Summary of Project:

This project's objectives include: (1) understanding the role of cathode electrocatalyst degradation in the long-term loss of polymer electrolyte membrane (PEM) fuel cell performance; (2) establishing dominant catalyst and electrode degradation mechanisms; (3) identifying key properties of catalysts and catalyst supports that influence and determine their degradation rates; (4) quantifying the effect of cell operating conditions, load profiles, and the type of electrocatalyst on the performance degradation; and (5) determining operating conditions and catalyst types and structures that will mitigate performance loss and allow PEM fuel cell systems to achieve the U.S. Department of Energy (DOE) lifetime targets.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.4** for its relevance to DOE objectives.

- The topic and program are highly relevant to the DOE Hydrogen and Fuel Cells Program's objectives.
- This project is a good fundamental study on basic degradation mechanisms. The relevance is high, but it is limited to the catalyst.
- This project is very relevant to DOE's objectives. In order to meet DOE's durability targets, all decay mechanisms and rates from each of the components must be known.
- Understanding the effect of platinum (Pt) and Pt alloy properties on voltage decay is a critical issue that must be addressed to enable fuel cell electric vehicle commercialization.
- Argonne National Laboratory (ANL) seeks to understand in depth the nature of catalyst dissolution. It repeats in a very thorough and methodical way work that has been done already. The dependence of crystallite size is fairly well known. The pulse shape dependence has been published already.
- Degradation of catalyst activity is clearly a very relevant issue for DOE. This project contains a lot of work on different ways to degrade catalysts. However, it is not clear that all of them are relevant to real fuel cell conditions. For example, it is unclear why someone would use perchloric or sulfuric acid to examine acid leaching and dissolution. Triflic acid would be more relevant.
- ANL is making progress toward understanding the factors that govern the durability of oxygen reduction catalysts in fuel cells. Such knowledge is critical to the development and relevant testing of advanced catalysts to meet all of DOE's performance targets simultaneously.
- ANL is attempting to generate data to allow testing of a model of catalyst degradation that one would hope would have predictive value. This could be very valuable if it can be reliably and verifiably done.
- The comparisons of pure-Pt and Pt-alloy catalysts should be important in guiding how much emphasis should be placed on the development of alloy systems. It would be helpful to compare alloys prepared using different methods (e.g., codeposition versus sequential deposition) to check the generality of conclusions drawn from materials made by one preparative method, as appears to be the case here.

### Question 2: Approach to performing the work

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

- The approach is a little too limited.
- This is a good approach. It would have been good to see PtNi results on the non-carbon support done earlier.
- This project is a “tour de force” of organizing a large group of collaborators to produce a single, coherent view of predominant catalyst degradation mechanisms.
- ANL has done an outstanding job collecting and analyzing an abundance of data to highlight the key factors that impact voltage loss in dispersed platinum group metal (PGM) catalysts and membrane electrode assemblies (MEAs). The approach could benefit from a more thorough statistical analysis to identify better second and higher-order effects. There is even more that can be gained from the vast amount of data collected by taking advantage of the design of the experiments analysis. It is not clear how the modeling is fitting in.
- ANL accomplishes a lot of work and provides lots of data. There is an attempt to correlate with models, but it is not established very well, at least as it appears in this presentation. One is left with the impression that there is not a strong theoretical basis for the design of the experiments.
- The lead institution and partners have a very comprehensive approach and well-defined milestones. The project is well structured. There is a good balance between theory and experiment.
- Good work has been done to check correlations between electrochemical behavior in aqueous solutions and performance in real fuel cells.
- Nice efforts were made to separate Pt versus alloy effects from particle-size effects. The presentation could be a bit more clear on exactly how the different initial particle sizes were achieved, either by thermal annealing of a given small-particle precursor or by synthesis to differing initial particle sizes, as the results from these two sample-preparation strategies could be quite different.
- ANL has made good efforts to try to separate out the relative contributions of multiple effects: Pt dissolution/reprecipitation, particle coalescence, loss of alloying-element atoms, and corrosion of the support.
- A greater diversity of preparation methods should be employed for any given alloy composition. It is not clear if the conclusions on the effects of acid leaching, either before or after MEA preparation, would be the same for materials generated by different synthetic approaches.
- More detailed structural evaluations following durability testing could be informative.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- ANL has achieved an excellent level of accomplishment so far.
- ANL has generated a tremendous amount of data, but the progress is not so clear. After the presentation, it was still unclear whether this project is meeting DOE’s goals. The presenter went long and failed to make this point clear.
- This project has done very well on the first goal of elucidating predominant degradation mechanisms. It is not as strong in the second stated goal of mitigation strategies. The reviewer did not see a recommendation of running boundary conditions or whether there is a way to carry out the recommendations on operating limits based on this work.
- The key results involve the role of particle size and electrochemical surface area on durability and the fact that the relationship holds for both Pt and Pt alloy nanoparticles. It is not clear that the findings are universally true, as conflicting data sets exist, but the conclusions are merited, based on ANL’s data. It is not clear how the modeling work is driving the understanding, but that may be due to the lack of time during the talk to present the modeling work.
- The project goals are to understand the degradation of the cathode catalyst and its impact on performance. The project has done that from two points of view in a very in-depth way: at the catalyst level and at the voltage point of view, which are fairly well documented. What the researchers have not done is look at this project from the point of view of the acidity of ionomer, the type of ionomer, the temperature of operation, and the local current density.
- The team has made great progress toward the stated goals and has taken a nicely structured approach that has yielded many fundamental insights. The volume of work performed is truly impressive. The stated milestones

have been substantially met. The clarification of the relationship between ECA loss, mass activity, and particle size was particularly useful.

- Good progress has been made in measuring and modeling changes in particle size distributions for several different catalysts, and the process has been modeled.
- The output of the modeling effort should include not only the predicted evolution of the particle size distributions, but also the numerical values of the diffusion coefficients used for alloy systems and a description of how they were derived and the extent to which these coefficients agree with previous experimental data.
- The experimental work with the novel $Pt_3Y$ and $Pt_3Sc$ systems was very useful to fuel cell development in terms of showing the limited level of trust that can be placed in current calculational estimates of both activity and durability of oxygen reduction reaction (ORR) catalysts (and in experimental results obtained entirely through rotating disk electrodes).

### Question 4: Collaboration and coordination with other institutions

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

- This project has very good collaboration.
- The project has an excellent range of collaborators, but it is not very clear if they are all collaborating.
- There is phenomenal coordination and collaboration between the partners and their specialty/contribution. It is great to see Johnson-Matthey Fuel Cells Inc., make a catalyst control at the diameter needed to compare to alloys.
- This project has a solid coordination of organization within the project and clear interaction with other durability and catalyst projects through working groups. It is good to see the inclusion of ANL's PtNi alloys in future work, although the project is almost over. It would be interesting to know what impact Jeremy Meyers leaving University of Texas-Austin would have on his part of the project.
- The team is well structured and has demonstrated effective collaboration between national laboratories, industry, and university partners.
- The coordination between the team members appears to be quite good, including correlations drawn between experimental work and calculations. The project team appears to interact productively with other DOE ORR catalyst durability project teams. The project serves the fuel cell community in general through the principal investigator's chairing of the Durability Working Group.

### Question 5: Proposed future work

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

- The proposed future work is logical and in line with expectations.
- Mitigation strategies appear to be focused on carbon supports, which are covered with other DOE-sponsored programs. Most of the focus is on cathodes. It was unclear if ANL is doing anything on anodes.
- Future work on PtNi alloys, temperature dependence studies, and post-test diagnostics of the PtCo MEAs is justified. More statistical analysis of existing data should be added.
- There were no specific slides calling out future work. The reviewer is looking forward to the work on chlorides and 3M's NSTF and hopefully some core shell materials, as that work has not been published much.
- There were not many suggestions for where this project would go in the future besides responding to last year's reviewer comments.
- This project is approximately 83% done, so the amount of future work that can be done is limited. Plans for studying temperature effects on oxides, new work on PtNi, and completion of a cathode performance degradation model seem very ambitious.
- A bit more attention to possible changes in area-specific activity during analysis of the data might lead to insights useful for the design of new catalysts.
- It would be nice if ANL could make more progress with in operando XAS studies of catalyst degradation in working fuel cells to show scientists outside national laboratories whether such experiments are likely to be worth the trouble of doing.

## Project strengths:

- This team is very industrious with lots of data generated.
- This is a strong team with great analytical skills and equipment. The leadership is excellent.
- This project has a strong team. There is a good balance between theory and experiment. A large volume of work was performed and data was duly analyzed to provide key insights.
- This project carried out a degradation analysis through systematic and sophisticated analytical instrumentation. There was good selection of controls. The team is strong, as are the team's contributions.
- ANL has executed a solid plan and collected and analyzed a lot of useful data. It is a solid approach to address a critical issue. There is strong collaboration both within the project and with the fuel cell community.
- The quality of work is extremely high in documenting catalyst degradation at the catalyst level. It provides thorough documentation of what others have observed and forms a basis of prediction for modeling efforts in this narrow set of constructions.
- A very large amount of systematic work has been accomplished under this project, and several factors have been confirmed as major drivers of catalyst degradation.
- The level of relevance of studies concerning aqueous electrolytes to the operation of PEM fuel cells has been successfully tested.
- Initial particle size distributions were shown to exercise a strong control over subsequent changes in both surface area and mass activity.

## Project weaknesses:

- The project's mitigation strategies should be communicated as part of recommendations or areas for further research.
- There is a lack of statistical analysis and links between modeling and experimental work.
- The work is not held together by a strong hypothesis and is very empirical.
- The modeling component was not well explained. While a number of different types of catalysts have been examined, the question about how the observed results can be generalized will always remain and the team should try to address this issue.
- The dependence of many mechanisms on particle size and distribution is apparent, but other effects such as contaminants in the carbon or other material making up the ink solution are not explored. It is unclear why scandium was chosen as the other metal to evaluate; the results are underwhelming.
- Most of the work on alloy catalysts was done from one type of preparation of PtCo catalysts, and the results may not be fully representative of alloy catalysts in general.
- A bit more attention to possible changes in area-specific ORR activities might give a fuller picture of catalyst degradation, particularly with a more diverse set of starting materials.
- The project documents catalyst dissolution with outstanding science and powerful tools, but there is no new science here giving direction for what to do to moderate this. The researchers may be filing intellectual property, but there is no indication that they are. It would be desirable to see what deleterious effects come from alloy catalyst dissolution besides the loss of activity, namely performance losses at high current densities due to concentration gradients.
- The project focuses on one-cell temperature. An understanding of the activation energies of the process would be most helpful in determining the trade-offs between durability, performance, and balance of plant.

## Recommendations for additions/deletions to project scope:

- This project needs stronger theoretical justification.
- There is not much time left, but doing more thorough statistical analysis to look for second-order effects is recommended.
- The impact of dissolved metal ions from the catalyst in the catalyst layer ionomer, at high current densities, would be very interesting to study because it might be more important than the loss of activity in the catalyst in determining the overall MEA performance.
- This project should expand to look beyond chlorine, even though this is the most likely real-world contaminant. Other contaminants will be there such as sulfur and CO.

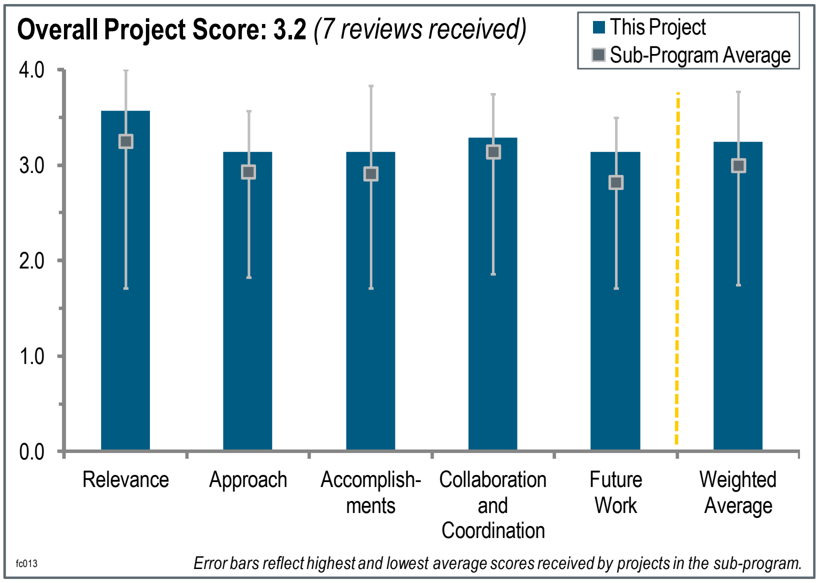
- Because the strength here is the analysis, ANL should collaborate with other DOE project teams that are "best in class" for stable support/alloy and apply the methodology developed here.
- In addition to using waveforms that comply with DOE durability protocols (cycling above or below approximately 1 V), perhaps upsets that typically occur in the stack, such as current reversal or the actual time spent at >1 V during start-up/stop of an automotive stack, can be employed as part of the degradation studies.
- The stated objective is cathodes, and clearly this is where most of the degradation action occurs, but perhaps anodes should also be examined. Even some small efforts to confirm there are no concerns would be useful.

# Project # FC-013: Durability Improvements through Degradation Mechanism Studies

Rod Borup; Los Alamos National Laboratory

## Brief Summary of Project:

The objectives of this project are to: (1) identify and quantify degradation mechanisms in fuel cells by developing advanced in situ and ex situ characterization techniques to evaluate individual fuel cell components, component interfaces, and component interactions; (2) understand the impact of the electrode structure on durability and performance; (3) develop models that relate components and operation to fuel cell durability, and make an integrated comprehensive model of fuel cell durability available to the public; and (4) develop methods to mitigate the degradation of components.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- This project is very relevant to the goals and objectives in the DOE Fuel Cell Technologies Program Multi-Year Research, Development, and Demonstration Plan.
- This project is fairly well aligned with DOE Hydrogen and Fuel Cells Program objectives, particularly in the area of durability.
- The project is relevant to DOE's objectives. Fuel cell component durability is a key area of interest where insights are needed.
- This is a fine beginning for the base case model of durability with a particular construct. The model might not have as much instructive value as hoped for, given the limited material set.
- This project is addressing the most relevant issues facing the fuel cell technical community. If this group is able to contribute to the durability solutions, this project will have been successful and will absolutely contribute to the overall DOE objectives.
- Durability is one of the key catalysts' properties. Improving durability is a very important segment of catalyst development. Knowing the degradation mechanism is the first step in the rational approach to improve durability.
- This is a well-executed project with a focus on the key parameters and interactions needed for high fuel cell durability without added costs. The project has great visibility across all of the DOE projects and Los Alamos National Laboratory (LANL) has used accelerated stress tests (ASTs) that are relevant to the key decay mechanisms.

### Question 2: Approach to performing the work

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

- The approach taken is comprehensive, though it appears that there are a large number of parameters/materials being studied and it is not very clear how all of it will be integrated.
- Basically, the approach is good. However, final changes in the material properties by AST are to be correlated with overpotentials and finally I-V performance using a MEA/CL model, which can predict I-V performance.

- The approach involving in situ and ex situ studies, varying catalysts and ionomers, determining components degradation, and looking at structural effects to identify the mechanism of degradation is quite appropriate for the durability problem.
- The approach taken by this team is comprehensive, coordinated, and well organized. All the major barriers are being addressed with well-defined milestones. Although all the major themes are being addressed very well, in the presentation it would have been helpful if a flowchart-like overview of the entire test plan had been shown. This would have been helpful in assessing which tests are being carried out time-wise and would allow comment on how findings from one set of tests might impact the test program in this comprehensive undertaking. Overall, the approach taken by this team in addressing the interrelationships of many variables is long overdue.
- This approach seems unnecessarily broad and because of that many slides have observations or theories that are left out there without further testing.
- The approach to carbon-supported catalysts is good; LANL should have been more focused on that and the overall impact on performance. Looking at platinum (Pt) blacks, 3M nanostructured thin films (NSTF), core shell, and alloy catalysts would be very interesting to see how they affect durability/performance as these are leached into the material. Investigating the impact of additives on performance, durability, and high current operation would have made for a more comprehensive model of durability. Changes in mechanical properties of the PEM MEA with relative humidity cycling seem neglected here.
- The approach overall is very good, with a couple areas for potential improvement:
    - Catalyst degradation: The anode loadings in MEAs are somewhat high. It would be good to see evidence that shows that this is not a factor in the overall performance loss measurements. The low cathode loading data are really interesting. It would be good to see more information on the microstructural changes in the electrode preparation. The lack of decay in the low-Pt loading samples seems anomalous with respect to the industry, so a deeper dive on this would be helpful.
    - Membrane degradation: LANL shows a very interesting effect of the cathode preparation on membrane durability and should consider the effects of this on Pt dissolution, with subsequent effects on membrane durability.
    - Carbon corrosion: It would be good to see the results of electrode degradation combining carbon corrosion and load cycling as seen in practical power plants.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has made good progress against its milestones and appears to be on track.
- The study on "low Pt-loaded MEA" is good. The study and modeling of the individual phenomena is going well. The quantification of material properties decayed by testing is necessary, and finally correlation with overpotentials/I-V should be revealed.
- This project has achieved great progress and insight on electrodes and electrode effects on membrane degradation. There should be more carbon corrosion mitigation combinations tested (i.e., combinations of stable carbon in gas diffusion layers [GDLs] and catalyst layers, PTFE content).
- The accomplishments to date are very interesting and there has been a large volume of work performed. Most of the milestones/targets have been achieved. One thing that did not come through was the work on the integrated modeling of all the degradation processes.
- The authors reported several interesting results that elucidate catalysts' degradation, including catalyst agglomeration as a function of loading and localized structure changes. It is not clear why no structural changes were observed in the microporous layer (MPL), even though $CO_2$ originates from it.
- Degradation of the ionomer interacting with Pt is an important observation.
- Some of the results confirmed previous findings, such as the role of increased hydrophilicity with oxidation and the role of dissolved Pt on hydrogen ($H_2$) and oxygen ($O_2$) crossover.
- Improved stability has been achieved with some catalysts, but no description of the procedure to obtain it was given.
- LANL has made excellent progress. New findings are leading to new durability perspectives and insight into this multidimensional problem. The team appears to be making progress on all fronts. The only serious weakness is the dependence on students to perform the work. As reported, one milestone was not accomplished

because of the loss of a student. It is also difficult to assess how other pertinent electrode elements are being addressed, if at all. For example, ionomer content and influence in the electrode layer, carbon purity (of the MPL and GDL and electrode support), and pre-treatment of the membrane can and have been shown to impact performance.

- There appears to be a tremendous amount of work being carried out in this project, but it is a mixture of studies that range from very relevant to an exceedingly thorough work of previously well-documented results. This reviewer will be going through the papers to understand the degree to which the review results were snapshots of scattered results or statistically significant results. LANL showed data for areas such as XPS shifts for fluorine binding energy without stating the hypothesis being tested until the backup slides. The open circuit voltage (OCV) data slides are ambiguous and perplexing. LANL stated that, for the aged MEA, unequal anode and cathode pressure data are unreliable due to $H_2$ outboard leaks, and the analysis is based on these data. Further, the observation that there was no change in OCV going from 10 to 35 psig with the fresh sample is a surprise.
- LANL should carry out pore size geometry measurements on tested catalyst-coated membranes to see how the electrode morphology is changing. It was unclear if this was a porosimetry measurement or BET.

### Question 4: Collaboration and coordination with other institutions

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

- Collaboration with partners appears to be going well.
- The project has shown good collaboration between partners.
- Very good collaboration is evident from the contributions of each collaborator.
- This is a well-coordinated collaboration with credible organizations and researchers.
- It seems like all of the groups are contributing proportional to their budget.
- The list of partners and collaborators is extensive. There is good coordination between the partners/collaborators, and this year's additional partners (Nuvera, Argonne National Laboratory [ANL], the University of Nancy, etc.) make sense.
- Great modeling progress has been made with Lawrence Berkeley National Laboratory; good characterization work was done with Oak Ridge National Laboratory, and nice start-up/shutdown (SU/SD) cell development was done with the University of Nancy. LANL should identify the ongoing gap of high-power durability issues at low loadings.

### Question 5: Proposed future work

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

- The proposed work is a logical extension of the project's accomplishments.
- The proposed work is clearly derived from open questions that remained unanswered in the reported studies.
- The proposed future work is in order. However, not much was mentioned about the integrated modeling component.
- Much more work is needed to complete the metal and carbon bipolar plate durability studies and the correct areas seem to be captured in the slides.
- This project's plan is OK. The temperature dependence of various degradation phenomena should be investigated to allow people to predict the MEA/cell lifetime for individual operating conditions by interpolation or extrapolation of the Arrhenius plot.
- The future plans presented are appropriate for this effort. It is concerning that there are still many tests that need to be completed within the next 18 months, and there is not much time considering that many tests may have a significant impact on the conclusions of the entire project.
- One significant outcome of this project is the integrated model for fuel cell durability; it will be a baseline to compare against.
- The studies comparing SU/SD cycling with potential cycling will be most interesting and hopefully show high correlation, which would greatly simplify SU/SD testing.

### Project strengths:

- The carbon-supported catalyst work is good.
- The theme, network, and collaboration are all strengths of this project.
- The breadth of the team and its experience bring a vast amount of expertise to the project.
- A strong team and strong collaboration are the strengths of this project. The treatment of the durability problem is quite comprehensive.
- This is an excellent team that is addressing key variables in a difficult system (MEA). The available testing resources are very good, as are the communication and coordination.
- LANL effectively uses ASTs to separate and identify the effects of different mechanisms. This project is a great use of MEA variants to show very interesting mitigations for durability issues (electrode and membrane), and a good use of characterization and modeling to register results.
- This is a very good team, which is addressing an important problem. LANL is using a sound approach and proper management with respect to meeting milestones. The researchers are attempting to have modeling in conjunction with experiments. The insights on localized structural changes in carbon were quite interesting.

### Project weaknesses:

- The quantification of material properties and the prediction of I-V by modeling should be improved.
- A large number of MEA variants have been chosen; however, it is unclear how all of the results can be integrated and generalized.
- It looks like there is some overlap with the ANL durability project; these redundancies should be removed where possible.
- This is a massive (technical) undertaking and researcher continuity is critical. The use of students is clearly understood; however, selected tests carried out by the students may not be completed on time.
- It appears that test result reproducibility was not reported.
- The integrated model LANL will produce does not appear to represent technology in current practice. All ionomers in use have fluorinated end groups and all manufacturers are using some peroxide mitigation strategies. The integrated model may be limited to a particular set of materials.

### Recommendations for additions/deletions to project scope:

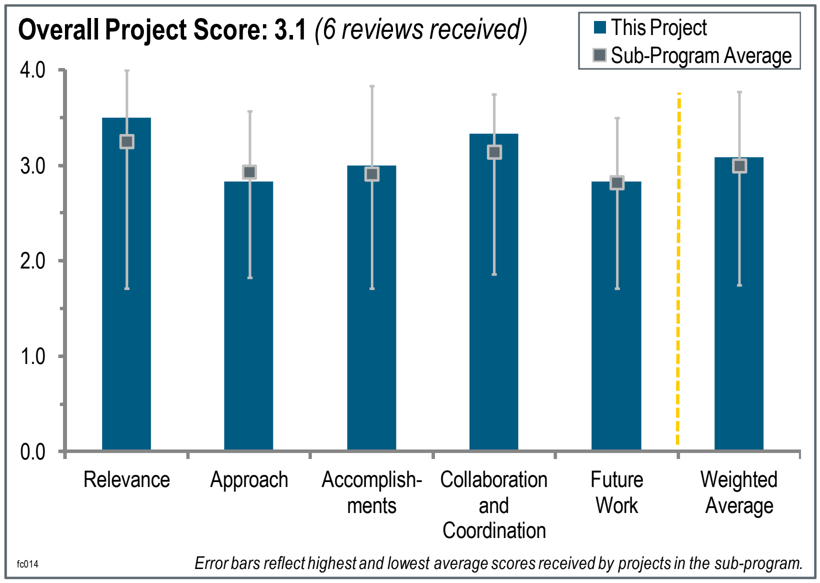
- The researchers should further explore ways to preclude Pt dissolution.
- At this stage, it is not advisable to add additional elements to the scope of the work plan. This project is already complicated enough.
- LANL should predict I-V performance based on the properties of fresh/decayed materials.
- No further studies on fluorinated end groups are necessary. Original equipment manufacturers use peroxide mitigating additives, and studies looking at the balance of these versus performance and durability might be nice.

## Project # FC-014: Durability of Low Pt Fuel Cells Operating at High Power Density

Scott Blanchet; Nuvera Fuel Cells

### Brief Summary of Project:

The objective of this project is to identify and model polymer electrolyte membrane (PEM) fuel cell durability factors associated with low-platinum (Pt) membrane electrode assemblies (MEAs) operating at high (>1 W/cm$^2$) power densities. The key deliverable of this project is a durability model that is experimentally validated over a range of stack technologies operating at high power.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- The project is very relevant, but component development is more critical than overall stack development because stack development does not contribute to companies using different stack configurations and systems.
- This program is addressing critical issues related to fuel cell viability. If successful, this group will provide additional evidence related to the durability questions plaguing the progress of attaining lifetime and performance targets. The objectives and activities in this effort align with the goals of the DOE Hydrogen and Fuel Cells Program (the Program).
- Maintaining high performance at high power densities using reduced platinum group metal content is a key enabler for PEM fuel cell automotive applications. The Nuvera design seems capable of very high power operation, so it is a good system to do this work.
- This project is well aligned with theProgram. The key to developing low-cost, durable fuel cell stacks is to be able to use models and accelerated stress tests (ASTs) to predict the voltage degradation under different operating regimes. This allows industry to conduct the relevant trade-offs to understand technical feasibility and the cost of each of the different system components (including stack).
- Reducing electrocatalyst is a good objective. The data reported indicates changes in the durability with changes in the catalyst loading. The data suggest reduced availability of oxygen ($O_2$) for the thinner catalyst layers; this is to be expected and should not be considered breakthrough technology, but rather a confirmation of the results from previous studies in the literature. The increased diffusion losses in the thicker cathode catalyst layer are consistent with previous works. The carbon corrosion with unmitigated start stops is expected. It was surprising to see the high reverse-current decay (RCD) not accelerating the degradation of high-loaded Pt electrodes. The objectives of the study were worthwhile; but they only provide confirmation of previous research and development.
- This project addresses DOE's cost and durability goals, and fully supports the Program objectives. Nuvera's approach to improve power density by operating at higher current densities offers cost advantages, but others have seen durability issues when operating in this regime with low-loaded catalysts. This work seeks to address the durability issues associated with operating at the higher current densities. This strategy is applicable to fuel cells outside the automotive arena as well. Automotive original equipment manufacturers (OEMs) have stressed heat rejection as an issue when operating at high current density; however, not all fuel cell applications have as stringent heat rejection requirements (Q/iTD) as autos. In addition, there are advanced thermal management techniques, which could allow for easing of the Q/iTD requirement that could make operating at higher current density more favorable.

### Question 2: Approach to performing the work

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

- The durability of catalysts has been evaluated by several groups using various drive cycles, and it has not been considered and utilized, or incorporated in this project.
- The approach taken is appropriate and addresses the durability and performance targets. The key durability issues are being investigated and, if successful, should provide guidance to resolving the lifetime limitations. It is not clear as to how the model will incorporate the different key variables being studied.
- The approach was very good, but there is nothing outstanding about it. If there was something new in the approach, it was not clear. The approach is sufficient to answer the objectives of the work.
- The program has done a good job in comparing performance across stacks, single cells, and subscale cells. There needs to be more about how the project is concluding that the main source of degradation in the low-loaded MEA's effects ionomer degradation. The use of electrochemical impedance spectroscopy to make quantitative determinations of losses within the electrodes can be highly suspect, and it needs to be confirmed with other measurements. The presentation needs to better highlight the way these conclusions are reached. The same must be said for the determination of the $O_2$ pressure at the electrode level. Also, the tests need to be run at the proper temperature in order to reject the heat at such high power densities; this factor needs to be included to see if increased degradation results.
- The project team has a balanced approach to the development of understanding of failure mechanisms in operating fuel cell stacks. The project team has done a good job of trying to link the impact of ASTs on voltage degradation performed at the single cell and multiple cell stack level. There appears to be good agreement between the different architectures investigated and between the different groups using those architectures (not a trivial task). The approach could be more relevant if the project team were to consider in more detail the impact of off-spec operating conditions on the fuel cell durability of low-cost MEA options. As an example, in an air-air start scenario, the voltages expected to be seen by the cathode far exceed 1.0 V (i.e., they are outside the stated AST protocol range). The team appears to have touched on this when trying to understand the impact of start-stop operation in NST3 (slide 15). However, it appears that the researchers have not been able to measure half-cell potentials to indicate the degree of stress in terms of voltage that differentiate mitigated and unmitigated. Also, the data shown is only for the high-loaded MEA case. One would hope that they have similar data for the low-loaded case.
- The approach is focused on using modeling and accelerated aging tests and drive-cycle tests to validate models and determine the effects of high current density operation on fuel cell durability. Testing and modeling both Nuvera's open flow-field design and land-channel architecture makes the work applicable to the whole fuel cell community. It is not clear that the ASTs being used will determine how operating at high current density affects aging, particularly for current densities of 2.5 $A/cm^2$ and higher. The discussion of aging or of ASTs operating above 2 $A/cm^2$ is limited. Tests where the cell is operated at the highest current densities for extended times (e.g., a soak at 2.5 or 3 $A/cm^2$ for 100 hours or more) are not described. The results from some cycling to high current density (lower V of 0.4 V) are shown, but it appears most of the testing has been limited to current densities below 2 $A/cm^2$. The testing to date appears to be more focused on how aging under normal conditions affects performance at high current density, rather than how operation at high current density affects aging (e.g., NST3 appears to only go to 2 $A/cm^2$ [from the performance curve on slide 10], and its effect is evaluated on performance at different current densities up to 3 $A/cm^2$).

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Some of the studies, such as on Pt loading, have been reported many times for the last decade and the results are not new or interesting.
- It is surprising that the main, salient result is the continued degradation at low loadings during the drive-cycle testing, yet no real attention is paid to how that will be mitigated in the future. This is the main focus of the program, so much more focus on this point is needed. Given the concerns about the assigning of electrode losses earlier, it is hard to see that fundamental progress on the low loading durability will be made.

- These researchers systematically answered questions regarding the stability and durability of the catalyst. It was surprising that the RCD did not accelerate degradation of high-loaded MEAs; this would suggest that much of the higher loading is not participating in the electrochemistry and that it may be unnecessary to have such a high catalyst loading.
- The progress to date has been good, and Nuvera has demonstrated consistency between single-cell open flow field and stack beginning of life performance, temperature and pressure gradients, and oxygen pressure at the catalyst. This project has benchmarked open flow field and land-channel architectures, and found similar aging and limitations for comparisons above 1 $A/cm^2$ due to the pressure differences in land-channel versus open flow field. The researchers should focus more on how normal aging affects operation at high current density.
- The low-cost MEA design appears to be on the path toward meeting DOE cost targets based on initial performance. However, the voltage degradation observed appears to indicate that there is still a significant hurdle in terms of achieving realistic lifetime expectations (i.e., 5,000 hours). This in itself is an important result. The key deliverable from this project is a validated model and the team appears to be on track. The low- and higher-cost MEA technologies studied have provided good data to which voltage degradation models can be developed from. At present, the correlations between model data and AST data (slide 6) are in good agreement for only a couple of cases. This would generally seem to suggest that there is still some more refinement required.
- There results presented are very good. It would have been helpful to understand what thermodynamic studies were carried out and what they yielded in relation to the evaluation. It also appears the data on slide 6 regarding Pt dissolution was force fit. The data looks quite linear but with a Y-axis offset. Many other questions are raised based on the waveform used, but they were not discussed in the presentation. Furthermore, it is not clear whether the statements on the progress slides are observations or conclusions. The reproducibility of the data should have been reported. More data on all that is known on the MEAs would have been beneficial. If the MEAs are proprietary, it is unclear how one will know what to correlate with what on a fundamental basis.

### Question 4: Collaboration and coordination with other institutions

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

- Sufficient collaboration exists in this project.
- The partners seem to be engaged based on the data presented.
- The collaborative efforts are well balanced and appropriate.
- The program has established a highly qualified collaboration team.
- Collaborations within the project are well coordinated and appear to be working smoothly. Integration of the modeling, characterization, and testing efforts is excellent.
- It seems that the benchmarking done with Los Alamos National Laboratory and the post-test collaboration with Oak Ridge National Laboratory have been well executed, although these are the easiest. It seems that the modeling work with Argonne National Laboratory has only been applied to Pt dissolution, which is not the key phenomenon for the degradation. With 70% of the project complete, the model needs to have progressed to the point where quantitative reconciliation with the high-power, low-loading decay is occurring.

### Question 5: Proposed future work

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

- This work is consistent with the scope and project objectives.
- The future work does not clearly highlight the need for the model development to include the right mechanisms for the key decay phenomena, nor does it outline a credible plan for experimental validation of the model.
- How the future work will increase durability of fuel cell systems is not addressed. The proposed future work appears to be more basic science than direct benefit to commercial fuel cell development.
- There is some question as to whether there is satisfactory data available to begin building the Pt dissolution model. The researchers should decide what additional experiments need to be performed and what other electrode factors need to be understood, such as percent Pt on the carbon, ionomer content and influence, and micro-layer characteristics (i.e., pore size distribution and $d_{10}$ data).

### Project strengths:

- This project has a strong team and good systematic analysis of the data.
- The high-power-density stacks are operated with realistic loadings. There is good benchmarking across experimental platforms.
- This project has good collaboration partners, consistent experimental data sets, and a balanced approach of in situ and ex situ measurement correlations.
- This project has an excellent and very experienced team. The team is not resource limited, nor is it without the knowledge of degradation issues.
- This project has been very responsive to DOE and Fuel Cell Technical Team concerns about heat rejection and Q/iTD requirements, and it has begun testing at higher temperatures.

### Project weaknesses:

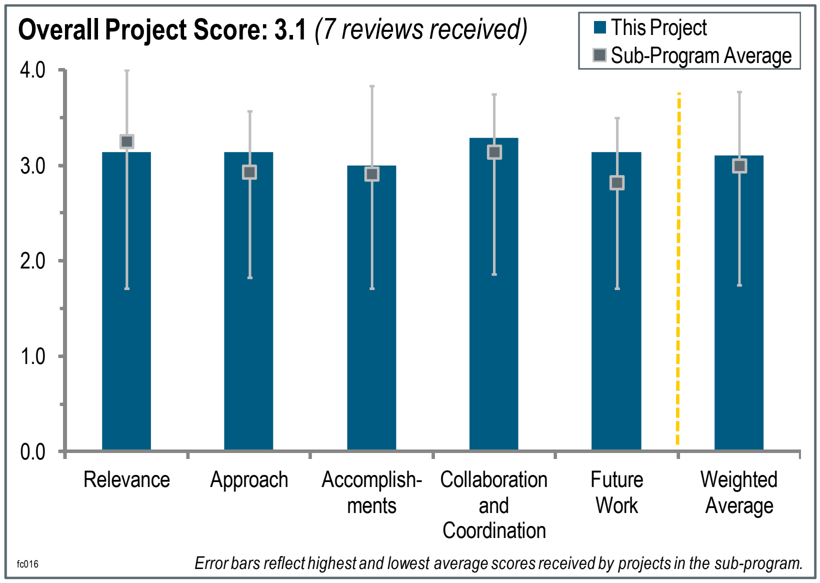
- There is a lack of focus on reducing high power decay with low electrocatalyst loadings, which is the stated purpose of the program. There is a lack of progress on model development and proper diagnostics for model validation.
- This project is repeating the results of many others. It would appear the researchers thought the increased catalyst loading would provide different results than other studies with higher loadings, and that the low-loaded results would yield different data than previous studies on low-loaded results. It is unclear why the researchers and DOE thought this.
- This project does not have enough half-cell voltage mappings of duty cycles. This would help to develop understandings of effective stress load on MEAs and would help develop more effective ASTs. There was no focus on recovery techniques for degraded MEAs or other modes of operation that would help maximize performance from degraded MEAs.
- It is hard to understand the level of detail that will be in the model results. It is unclear how the system characteristics (e.g., thermal gradients in the cell, for one) will be taken into account. It would be good to know about the specifics of the MEA. The potential weakness is embedded in the possible lack of detail.
- It is not clear how much of the testing to date has been at high current conditions versus aging under normal conditions to determine how it affects performance at high current density (e.g., NST3 appears to only go to 2 $A/cm^2$, and its effect is evaluated on performance at different current densities up to 3 $A/cm^2$).

### Recommendations for additions/deletions to project scope:

- If it makes sense, incorporate operational conditions that the MEA will encounter in a stack.
- The researchers should have the models more accurately address commercial fuel cell systems.
- The researchers should look more closely at linking off-spec operating conditions from duty cycle to voltage degradation.
- More focus on testing at high-current-density conditions would be beneficial, even if OEMs think this is beyond the current densities they could use. Determining the degradation modes under higher-current-density operation would be beneficial to the fuel cell community at large.

## Project # FC-016: Accelerated Testing Validation

Rangachary Mukundan; Los Alamos National Laboratory




### Brief Summary of Project:

Accelerated stress tests (ASTs) are important because they allow for faster evaluation of new materials and provide standardized tests to benchmark existing materials. The objectives of this project are to: (1) correlate the fuel cell component lifetimes measured in an AST to the real-world behavior of that component; (2) validate the existing ASTs for catalyst layers and membranes; and (3) develop new ASTs for gas diffusion layers (GDLs), bipolar plates, and interfaces.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.1** for its relevance to DOE objectives.

- The correlation of ASTs to real-world experience is valuable information for fuel cell developers.
- This project is relevant to the DOE Hydrogen and Fuel Cells Program. Validating ASTs is important to the industry.
- The project goal is well aligned with the DOE goal of improving the durability of polymer electrolyte membrane (PEM) fuel cells. Newly developed ASTs for GDLs, bipolar plates, and interfaces will provide researchers with a very useful standardized instrument to evaluate new and existing fuel cell materials.
- This is a very important project with good, solid work. Despite only evaluating post-mortem data from bus stacks, the general principles learned may be validly applied elsewhere. It is relevant to the key issue of improving durability.
- Overall, this project is relevant to meeting DOE's durability barriers. However, a significant portion of the project depends on data from buses operated by Ballard using older membrane electrode assembly (MEA) technology for field correlation. The level of mitigation for start-stop within the Ballard program is unclear, and a small number of unmitigated starts can have a significant impact on the catalyst and support durability, which could render the field comparison irrelevant. Further, the ASTs developed for the GDLs seem irrelevant because this failure mode is uncommon.
- This project makes an early attempt at correlating changes occurring in MEAs in a field application (buses) with changes induced by laboratory ASTs. As such, it provides a start toward a critical need in the development and validation of fuel cells for transportation applications.
- The total database from the field was limited to a few used stacks from buses, which apparently used platinum (Pt) loadings higher than those that could be considered for any application in larger numbers. While buses are a good early market application for fuel cells, the modest number of field samples obtained and the limited level of control over the duty cycles in the field have made this project appear a bit premature. For example, it does not appear that there were any tests of different carbon supports in the buses, so all of the support-variation data from the laboratory could not be checked with field data.
- The importance of understanding the durability and life of fuel cell systems makes this one of the most relevant programs supported by DOE. This is an extremely ambitious undertaking.
- The 40,000 hour stationary durability target may be difficult to meet with PEM. The 20,000 hour status was obtained under either laboratory or commercial conditions. If under laboratory conditions, it is not a reasonable durability claim.

- The AST's lack of correlation to "real-world" data needs better definition. It is unclear what "real world" means. Maybe Los Alamos National Laboratory (LANL) should take on the task of defining "real-world" conditions for the multiple fuel cell applications.

### Question 2: Approach to performing the work

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

- The approach is well designed and focused on establishing correlations between real-world fuel cell data and the results of accelerated tests to develop reasonable accelerated tests.
- The approach to correlate field ASTs and material property changes cannot be improved upon. The principal investigator has met the milestones required.
- LANL is taking on catalyst, membrane, and GDL issues for a well-rounded approach. It would be useful to see a correlation to more than the limited set of bus data. Light-duty vehicle (LDV) data would be more relevant due to the larger potential market and the very different drive cycle experienced by a vehicle rather than a bus.
- The approach seems sound. It might be useful to understand the sensitivity of the degradation mechanisms to the operating parameters rather than trying to find a single AST.
- The approach is well organized. There was no discussion on the operating history of the bus or a correlation with specific events. The impact of an air/air shutdown was not explained. The operating temperature spikes and the length of time that the fuel cells were at a peak temperature were not explained. The current approach gives an average temperature dependence, but in many cases this unusual event has a greater impact on durability. It is unclear if the anodes or cathodes were ever reactant-starved. All of these performance conditions exist and need to be documented for correlation with ASTs.
- The approach does not include pore size distribution, porosity, or mean pore size of GDL or microporous layer. It is unclear if the researchers think these values are unimportant.
- While it is a very good idea to try to validate ASTs by comparing the results with data from actual applications, the quality of the field data available to this project and the level of information that can be shared about the materials used in the field tests were inadequate for this project to really deliver what its title promises.
- Two different MEA types were used in the bus stacks. While the P5 stacks actually ran in buses, the HD6 stack was a bench test run under a presumed bus operation schedule. One therefore cannot really know whether the observed differences between P5 and HD6 behavior were due to the different materials used or the different protocols. For the P5 stacks, only averaged values of stressors are available, compromising the utility of comparing P5 bus results to results from ASTs. Because the differences between the materials for P5 and HD6 are not fully shared, the fuel cell community cannot draw useful information from this comparison.
- LANL plans to bridge the significant gap between the duty cycles of buses and those of the larger market of LDVs through small-scale laboratory fuel cell tests incorporating protocols intended to mimic light-duty behavior. While this work will be of some interest, it does not permit the "validation" of protocols listed in the project title. Until automotive original equipment manufacturers (OEMs) release samples and service histories from their vehicle fleets, no one will be able to do better on validation under a DOE project.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Significant progress has been made toward the evaluation of ASTs for catalysts, carbon, and membrane degradation. There is significant progress toward establishing correlations between field and laboratory tests.
- A significant amount of work has been done on catalysts and membrane ASTs. The rate of progress on automotive-related drive cycles and the timing for related failure analysis is unclear.
- LANL has developed good correlations between field data and catalyst ASTs, but it lacks good correlations for membranes and GDLs. Most fuel cell failures occur due to the mechanical breakdown of the membrane (crossover). It would be valuable to spend more time correlating membrane breakdown in the field to an AST. This would help screen new membrane materials more rapidly in the laboratory and avoid the cost of putting faulty materials in the field.
- The accomplishments/progress have produced some notable results that may not have been anticipated, and so they are even more valuable. Quantifying the Pt in the membrane ratios for the field versus AST is one such

result. The second is that there can be significant Pt particle growth as well as carbon corrosion during just the high-voltage hold (maybe without cycling). It is unclear if the mechanism for this sintering is understood. Because this is more of a diagnostic-type project and not a solution-finding project, it is difficult to rate this a 4, but it is a very good, solid project.
- The accomplishments are very helpful and will increase the knowledge base for fuel cell durability.
- It is unclear why high-surface-area carbon is being tested. It is unclear if there was something new in this carbon. Over the past 10 years, high-surface-area carbon has been evaluated and reported in the literature. It is unclear if a new AST will repeat experiments that compare high and low SA carbons.
- In slide 8, the researchers made no comment on the order of magnitude decrease in the number of air/air starts per hour.
- Results on electrochemical surface area (ECSA) and Pt-particle size appear to be a repeat of the last 10 years' work; it was unclear what was new.
- The AST underestimation of Pt in the membrane may mean that the test is invalid or that field tests were operated at harsher conditions.
- For the graphitization data, the initial size of the Pt particles on graphitized carbon compared to Pt particle size on higher-surface-area carbon was not given; it would be interesting to know if this is important.
- Position-in-cell-resolved characterization of bus MEAs is a useful contribution, but the presentation of the data on the "AST/Field correlation-electro-catalyst" slide was confusing, in that increases in ECSA are discussed but increases in crystallite size are what were shown in charts (i.e., inverted polarity of the effect).
- In terms of carbon corrosion, as measured by electrode layer thinning, a correlation of bus and 1.2 V hold data is encouraging, but it is likely to be highly dependent on the exact protocols used in the buses.
- It appears that post mortem characterization (failure analysis) of the bus-stack MEAs is still not complete, even though the researchers are 70% into the project. It would seem that this analysis should have been completed quite early in the project to provide a baseline for characterization of the post-AST MEAs.
- For the Pt in the membrane, it should have come as no surprise that the redeposited Pt lined up near the cathode for the hydrogen/nitrogen cycling AST, while for the bus it is more centered in the membrane; these contrasts have been previously observed and published.

### Question 4: Collaboration and coordination with other institutions

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

- A good team has been put together.
- There has been good coordination and collaboration between the partners.
- The collaboration within the project appears to be working well.
- Efforts should be taken to reach out to automotive OEMs to gather any relevant field data.
- This project has well-coordinated collaboration between the national laboratories and industry.
- The collaboration seems reasonable, but it would be good to see a broader range of field data for the correlation. The team should focus on making partnerships with organizations that can provide that data.
- The coordination with partners in this project is apparently limited by the willingness of the partners to provide materials and information on testing for evaluation. It could always be improved upon.

### Question 5: Proposed future work

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

- Plans are based on the past progress and are focused on filling the gaps between the results of field and laboratory tests.
- The proposed work builds on past work and expands to meet some of the critiques from last year.
- Completion of the automotive drive-cycle durability test and any related failure analysis is critical.
- The future work should yield data for a correlation between bus data and laboratory data. Failure analysis of bus stacks should quantify performance and durability losses with operation conditions and operation events. This did not appear to be mentioned in the presentation.

- The team is focused on improving the understanding of the relationship between ASTs and field data for GDLs and membranes. This is very important work to focus on.
- The researchers need to finally complete the failure analysis on the old bus stacks. Auto drive-cycle testing will be done with small-scale cells and serves as a patch to the project to make up for the limited field samples available. Much of the total impact of the project will ride on what can be accomplished in such tests.
- It is not clear that enough different membranes are being tested to allow a meaningful recalibration of the combined mechanical/chemical cycling AST, nor is it clear that this project is the right place for such a recalibration to be done.

### Project strengths:

- The researchers have significant experience at LANL with studying durability issues.
- The ability to run tests and correlate the observed losses using models is a strength of this project.
- This project has a strong team and good knowledge of laboratory accelerated testing procedures.
- LANL is a taking a holistic approach to the problem, including the carbon support, catalyst, membrane, and GDL.
- This project combines extensive experimental effort with modeling. LANL has carried out very careful analysis of different components to make a valid comparison of field and laboratory samples.
- This is one of the few public-sector analyses used in vehicular MEAs, even if they are limited. This project combines thoughtful experimentation and data analysis.

### Project weaknesses:

- This project lacks other field data, such as from LDVs.
- This project lacks field data with relevant mitigation strategies for start-stop and chemically mitigated membranes.
- The durability tests at different catalyst loadings overlap with Dr. Borup's project. It is unclear how analysis of the particle size distribution after durability tests was carried out and if catalyst-coated membrane (CCM) cross-sections were analyzed.
- This project lacks full (100%) collaboration with the partners supplying the materials and conditions. This project may be providing data on MEAs that are now old technology. As such, it is important to try to wring-out generally valid principles.
- This project has inadequate field databases with high-loaded electrodes, which will not be carried further into fuel cell development projects, and have only one type of MEA actually used in buses (with another on a bench running a different bus-operation-like protocol). Without a more extensive publicly released field database, true validation of ASTs is still impossible.
- LANL is also unable to disclose publicly the nature of the materials used in the field tests.
- This project is limited in its attempt to correlate laboratory results and commercial performance results (i.e., the ability to catalog all operational events in commercial operations and to assign performance or durability losses in a quantitative manner to operational events.) The overall scope of the objectives may be greater than the available funding.

### Recommendations for additions/deletions to project scope:

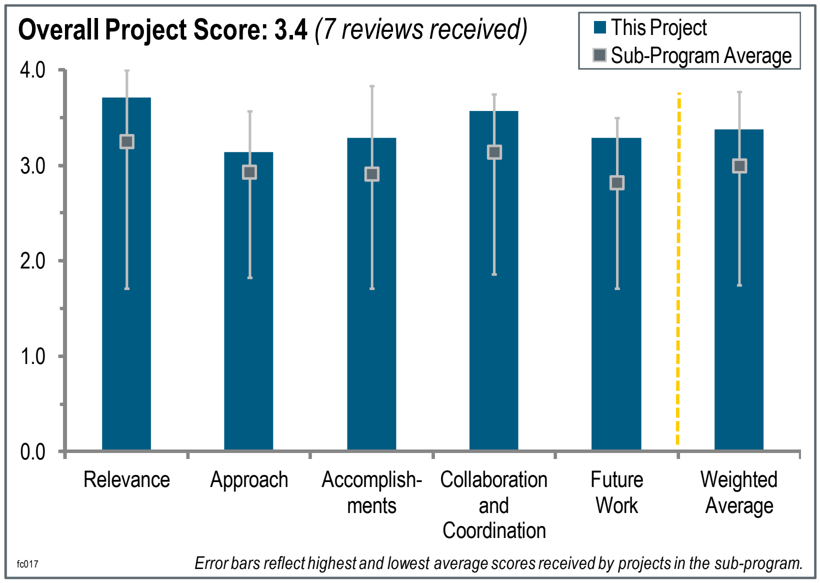
- This project should add LDV field data.
- The researchers should check if there are any relevant data from the National Renewable Energy Laboratory from the Learning Demonstration project to look at the number of voltage, relative humidity, and start-stop cycles.
- It does not appear that the team is focusing on stationary fuel cells. This should be removed from the scope of the project.
- More careful HRTEM analysis on CCM cross-sections, both for field and laboratory samples, would be helpful. More careful analysis of GDL microstructure, including analysis of hydrophilic and hydrophobic pores, pore size distribution, etc. would help to better validate AST protocols for GDLs.
- It seems like the researchers are collecting the data that seemed to be missing from the presentation, and that they are reporting what they believe are the critical parameters. When an AST is developed, it is unclear whether it will be generalized to all fuel cell systems or specific to the ones (Ballard?) included in this study.

## 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 for fuel cells and use it to assess design-point, part-load, and dynamic performance of automotive and stationary fuel cell systems. Specific objectives include: (1) supporting the U.S. Department of Energy (DOE) in 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 the DOE projects on high-volume manufacturing cost estimation.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- This project is very relevant and is one of the most important studies to drive the identification of targets for fuel cell applications.
- This project is highly relevant because it forms the technical basis of DOE's cost model work, and as such it is fundamental to managing the cost aspects of the program.
- This project supports DOE R&D with an independent analysis to assess design-point, part-load, and dynamic performance of automotive fuel cell systems. Stationary systems are not integrated, or were not presented.
- This project provides essential intelligence that informs the research, development, and demonstration plans by providing a comprehensive, fact-based, and data-rich analysis of the actual performance of fuel cell materials and components and how they relate to the overall performance of a fuel cell system.
- The project objectives do support the DOE Hydrogen and Fuel Cells Program (the Program) in a critical manner, but the focus of the principal investigator (PI) recently is too narrow, both with respect to the system options considered and studies undertaken.
- This project is important to DOE for target setting and as input into the high-volume cost estimates by other contractors. The project continues to evolve as more information is learned from industry about simplified designs (such as removing components, etc.).

### Question 2: Approach to performing the work

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

- The approach is solid and has remained the same for many years.
- The general approach to this project is sound. More extensive validation would be good, considering the use the model is put to.
- The approach followed is effective and contributes to overcoming barriers. Argonne National Laboratory (ANL) aims to provide a validated versatile system design and analysis tools adapted to handle issues of current interest. Validation on real systems should be investigated in the future.
- The approach is to validate the model against data collected through collaborations with industry and academia. The tests run on a 50 $cm^2$ cell to simulate stack data should be replicated with a more realistic hardware to

really prove performances. It is strongly recommended that other stack manufacturers are engaged to validate stack data and accuracy.
- The approach to the work is superlative. While this project, at its core, involves the creation of a model system (slide 6) and modeling the characteristics and performance of the materials and subsystems of which this model is composed (passim), the project is exemplary in its integration of actual performance data from a wide range of industry sources to validate and exercise the model in useful ways.
- The critical barriers are cost and durability. However, the PI is presenting minor system improvements (e.g., hydrogen [$H_2$] purge schedule) instead of studies that can have a major impact on cost or durability. The PI needs to include decay to help the Program understand how this impacts system and vehicle performance, to better understand what different types of decay mean to system design and cost. It would be interesting to know how adding complexity to the membrane electrode assembly (MEA) to mitigate start/stop decay with oxygen evolution reaction catalysts/eliminating carbon supports (such as 3M is exploring in multiple other DOE-supported projects) compares to system-level mitigations (such as those UTC Power has developed and utilized to obtain 12,000 hour durability on fuel cell-powered buses with conventional MEAs).

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The work is good, but the focus is not on the high-level, broadly applicable studies that would have the most impact.
- ANL is highly productive and provides outstanding value to the Program. The technical accomplishments (analytical results) summarized on slide 6 do not do justice to the myriad analytical results found on slides 7–20 and in the Technical Backup slides.
- The artificial neural network and physical models are a good way to capture the complexities of real systems without a model that is too complicated for the purposes of these studies.
- The calculated optimum purge schedule should be validated against existing fuel cell electric vehicle (FCEV) systems, because the optimal system may not always be able to be implemented in a real-world system for a variety of reasons.
- The model has been exercised to identify trade-offs. Studies suggest that low platinum (Pt) loading has an optimal value, but the data are biased by collaboration with only one stack supplier. More validation of the model on real systems is recommended. ANL should engage system manufacturers. It is clear that the model is capable of representing a huge variety of system conditions; however, it is not clear what the optimal system design is because the studies have not been combined.
- Several good accomplishments were achieved this year. Some—the neural network, for example—are not of the same value as others. To be trusted as generally as is required, this network really needs more data to learn from, and of course it cannot know when it is treating a system that it is improperly set up to handle.
- The nitrogen (N) model is useful, but it may need calibration at more H/N ratios in the feed.
- The studies to determine optimum Pt loading provide an interesting result and also offer an opportunity to validate the model by comparing with experiments. Model predictions on the optimum purge schedule could also be compared with experiment.
- The accomplishments and progress were significant this year and have been well presented. Comments regarding specific components include the following:
  - Model precision: It has been demonstrated that model and experimental precision are comparable. This is an important point for the model validation.
  - Stack: Collaboration with 3M to validate the model for nanostructured thin-film (NSTF) MEAs and stacks was increased.
  - Fuel management: Collaboration with 3M to validate the model for the anode subsystem and to propose a stack purge strategy was increased. It would have interesting to integrate the temperature evolutions during power variations into the $N_2$ dilution and purge strategy.
  - Water management: The performance of planar humidifiers using Gore's sandwich membrane structure was integrated. The researchers have proposed some improvements.
  - Dynamic performance and drive-cycle simulations: The researchers presented some interesting data on optimum Pt loading, optimum system efficiency, and optimum fuel cell system operating parameters regarding cost. Some surprising results are that the optimum Pt loading (0.25 g/kW) is higher than DOE's

targets and the optimum system efficiency (45%) is lower than DOE's targets. For the last point, the heat exchanger area constraint in a car should be taken into account in the model.

### Question 4: Collaboration and coordination with other institutions

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

- Interactions with 3M (stack) and Gore (humidifier) are fairly clear (they provided input data for the model). Honeywell's involvement is explicit.
- The collaboration with component suppliers and dissemination teams appears to be very good and relevant to achieving the project goals. Enhancing the collaboration with other component suppliers would be appreciated to not develop too specific of a model.
- By necessity, this project is highly collaborative with major suppliers of fuel cell materials and subsystems. Furthermore, the well-considered models produced by this project feed into other efforts to determine the actual costs of production for high-volume goods.
- This project has numerous and appropriate connections and collaborations that lead to value for DOE. In addition to the mentioned collaborations, ANL is also working with others on vehicle-level simulations and bringing the value in this project to those projects.
- This project does have sufficient partners to do what it is doing, but not enough to study more broadly applicable issues (e.g., different system designs, different system components, different hybrid vehicles, and durability studies).
- There is a strong level of interaction with several industry partners on particular technology improvements. The project would benefit from a wider cross-section of industrial collaborators, perhaps outside of the U.S. DRIVE Partnership community.

### Question 5: Proposed future work

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

- ANL has good plans that will continue to support key needs in the program. A bit more model development on balance of plant (BOP) would help.
- This project looks like more of the same instead of doing what is recommended here, which would have a broader impact than the intended plans.
- More validation on real systems should be pursued. The effect of durability on system trade-offs should be studied thoroughly, including taking the degradation of stack and BOP into account.
- The PI has a firm grasp on what future work will serve the Program well, including his enthusiasm for adding durability attributes into the models and his extemporaneous answer to another reviewer's question.
- A GCTool model of fuel cell systems for forklift applications may not be necessary, because forklifts are now commercial products.
- Establishing closer collaborations with original equipment manufacturers (OEMs), including those outside of U.S. DRIVE, would be beneficial.
- The researchers should identify other suppliers in addition to 3M and Gore for inclusion of novel cost-saving technologies as these technologies are developed.
- The future work proposed is in accordance with the general goals of the project. Validating, calibrating, and documenting the stack model is important because it is for the validation of air, fuel, thermal, and water management models. To do that, closer collaborations with the OEMs and component suppliers need to be established.
- Supporting SA, Inc. in conducting its high-volume manufacturing cost projections and collaborating in life-cycle cost studies are appropriate to ensure global system optimization for cost, performance, and durability.
- Incorporating fork-lift applications is fine.

### Project strengths:

- This project has a strong analytical foundation.
- ANL conducts very valuable modeling work, which is validated by tests.

- This project has a long, documented history, excellent team, fine models to base off of, and good responses to suggestions for improvement.
- The open system model is useful for helping to assess different design and system operation options, including cost impact. This project has just enough collaborations to establish at least one system configuration.
- This project is highly focused on DOE's needs for having an accurate and current fuel cell model available to answer fuel cell system-related questions.
- ANL provides valuable and necessary input to the cost modeling of others.
- Results are clearly communicated and suboptimization problems (humidification, purge, etc.) usually provide interesting insights.
- This project provides essential intelligence to the Program and provides great value for its budget. The richness of the data and the careful analytics are exemplary for a modeling project. This work demonstrates a superlative level of technical competence.
- The main strength of this project is the development of an independent analysis to assess design-point, part-load, and dynamic performance of fuel cell systems. This model takes into account many critical technical issues and appears helpful to guide the cost analysis investigated in other projects.
- The collaboration with different OEMs and component suppliers is good.

### Project weaknesses:

- ANL lacks access to the newest confidential information from component makers.
- It is a challenge for ANL to keep up with the latest designs being considered by the OEMs, which of course all have their own unique design.
- Assumptions on the stack and system are dependent on specific system and BOP design. No verification of the model against real applications has been pursued.
- Integration of stationary systems should start now.
- The experimental validation of fuel cell systems or systems integrated in cars is not presented.
- As collaboration with 3M has been increased, the MEA and stack modeling may become too specific and not applicable to other suppliers.
- This project is not focused on studies that can have a major impact on the key Program barriers: cost and durability.
- This project includes a very limited component set to date (i.e., NSTF only, externally humidified systems).
- ANL needs more partners to include more component and system-design options.
- There is too much focus on subtle system operating strategies (e.g., $H_2$ recycling purge strategy), and not enough on higher-level and more broadly applicable studies such as optimal fuel cell power rating.

### Recommendations for additions/deletions to project scope:

- ANL should broaden the set of collaborators beyond the U.S. Council for Automotive Research and existing suppliers.
- It is not necessary to include forklift fuel cell systems in future studies, unless the rationale is clearly laid out.
- The researchers should investigate fuel cell systems at higher temperatures, such as 95°C–120°C for automotive systems.
- The model should be validated through comparison with fuel cell systems or vehicle data.
- The modeling of stationary systems should be investigated.
- Collaboration with other component suppliers should be enlarged so as not to become too specific.
- ANL should conduct and disseminate more studies on complete vehicle optimization, specifically optimum fuel cell power rating versus battery rating. Although there were some slides in the backup this year, it is not clear what is being done here—for example, what the battery cost assumptions are and whether projected future battery pack costs are included. Some collaborations between the DOE Vehicle Technologies Program and this project seem warranted to obtain good battery assumptions. For validation (which the PI claimed would be challenging), ANL should collaborate with the projects examining FCEV field data (i.e., the National Renewable Energy Laboratory).
- ANL should analyze how a system will start up from low ambient temperatures, but not necessarily sub-freezing (using the low-temperature performance shown in the backup slides for NSTF). For example, it was

unclear how long it will be before an FCEV will have acceptable power output when started from 30°C. ANL should show the percentage of rated power availability during various start-up operating profiles.

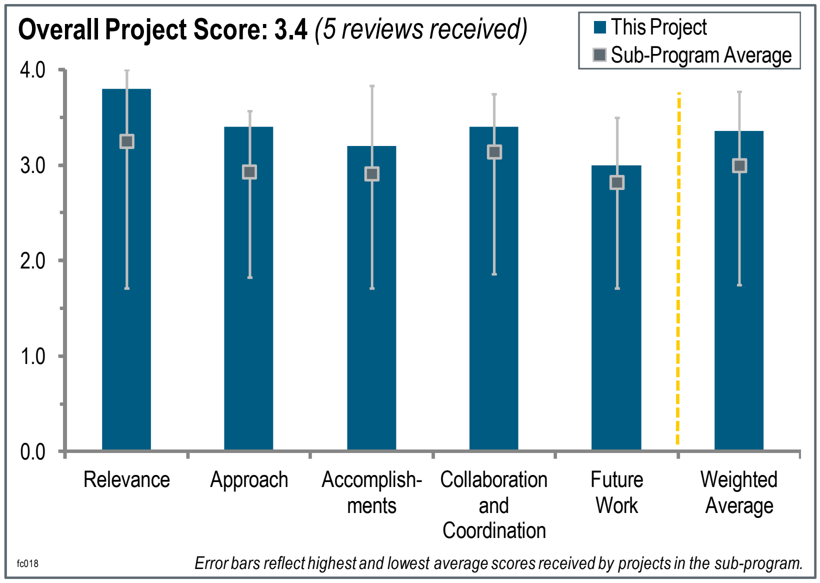
- ANL should analyze MEAs other than those with NSTF. For example, conventional MEAs (Pt/C) with low loadings (0.1 $mg/cm^2$ on the cathode) now offer performance on par with NSTF, but the performance variation with operating conditions is very different (e.g., temperature sensitivity).

## Project # FC-018: Manufacturing Cost Analysis of Fuel Cell Systems and Transportation Fuel Cell System Cost Assessment

Brian James; Strategic Analysis, Inc.

### Brief Summary of Project:

The goal of this project is to conduct a process-based cost analysis of stationary, light-duty automotive, and bus fuel cell power systems. The cost analysis can be used to assess the practicality of proposed technologies, determine key cost drivers, and identify the most fruitful research paths to cost reduction, thus providing insight for directing research and development (R&D) priorities for industry as well as for U.S. Department of Energy (DOE) targets.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The cost analysis is an important tool for the deployment of fuel cell technology.
- It is imperative for the assessment of technology advancing and target settings in the DOE Hydrogen and Fuel Cells Program (the Program).
- This is absolutely relevant for the DOE objectives because the analysis sheds light on the state of the technology.
- This project provides a valuable output, which satisfies the Program's need to show the drop in fuel cell system cost as the technology gets closer to market. Inclusion of other applications is useful to provide insight into R&D in other parts of the Program.

### Question 2: Approach to performing the work

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

- It is good to leverage widely collected data, including suppliers' information.
- The cost analysis addresses all relevant aspects of the technology.
- The approach is thorough and well thought out. This project has good interactions with industry and Argonne National Laboratory (ANL) to provide coordinated outputs. There is significant value generated from the key parameter sensitivity analysis.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- There is very little missing in the analysis.
- This project has a great methodology of analysis. The system designs need to be touched up.
- For the transportation fuel cell area, not enough accomplishment can be seen. Updated technical accomplishments should be incorporated into the cost analysis, such as various materials and process selection of bipolar plate technology. For membrane electrode assemblies, platinum alloy dispersed on carbon (state of

the art) should be analyzed as well as nanostructured thin-film options as a reference. It is unclear why air-compressor cost is so sensitive for the entire system cost. It is basically off-the-shelf technology.
- This project has shown rapid progress on the stationary fuel cell system analysis. It is useful to know that reformer balance of plant, rather than the reactor itself, is the key cost driver. This is the type of value and insight that is typical of this project. The automotive design seems to have stabilized. However, the researchers should prepare to make substantial changes/improvements in the next few years as additional pre-commercial and commercial fuel cell electric vehicles are introduced and innovations are revealed. It is very useful to know that the knee of the curve occurs at 50,000 units per year. As a result of this analysis (if validated by original equipment manufacturers [OEMs]), there should be a concentrated push to enable the market to reach this cost tipping point (incentives, etc.). Federal hybrid electric vehicle incentives from 2006 were sustained to a level of approximately four times this production volume per manufacturer. The tornado chart results were very interesting.

### Question 4: Collaboration and coordination with other institutions

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

- Strong industry and laboratory collaborations continue to make this project successful.
- Relevant partners and components have been addressed in order to obtain realistic cost projections.
- It is good that this project incorporated ANL system models for a performance baseline. It is good to communicate with OEMs.
- There should be more opportunity to communicate with other suppliers to increase technical options to be used in the analysis.
- The principal investigator (PI) has done an excellent job communicating with OEMs.

### Question 5: Proposed future work

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

- The project should keep watching the U.S. DRIVE Partnership target and assumptions to update the transportation fuel cell cost model.
- Exploring the differences between light-duty and bus fuel cell systems will provide additional insight and perhaps lead to some ideas about how to leverage both of them to reach the 50,000 production volume goal. Medium-duty trucks should be included in the analysis at the same time as the fuel cell buses.
- The assumed performance of the fuel cell technologies should be stated (beginning and end-of-life performance). There is significant uncertainty about the lifetime of the low-cost metallic bipolar plate. If the low-temperature polymer electrolyte membrane (PEM) fuel cell has to use graphite-based bipolar plates, the impact on cost should be included in the risk analysis. The higher value of excess heat for high-temperature PEM and solid oxide fuel cells should somehow be included in the analysis.

### Project strengths:

- The methodology is very good.
- This project has excellent solid investigations on cost and very sound results giving valuable guidance.
- The systematic methodology of the cost model and the accumulated database are strengths of this project.
- Clear, methodical analysis leads to a high confidence in the results from this project, which is made even more robust by the added sensitivity analysis. This project gives very interesting insights into what the major cost drivers are for the fuel cell systems analyzed. These results are useful for helping guide where DOE should put future investments focused on cost reduction.

### Project weaknesses:

- Some process modeling would benefit the analysis.
- With more than one application included in the DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) presentation, it appears to be hard to cover all of the work in one presentation. Perhaps break this up

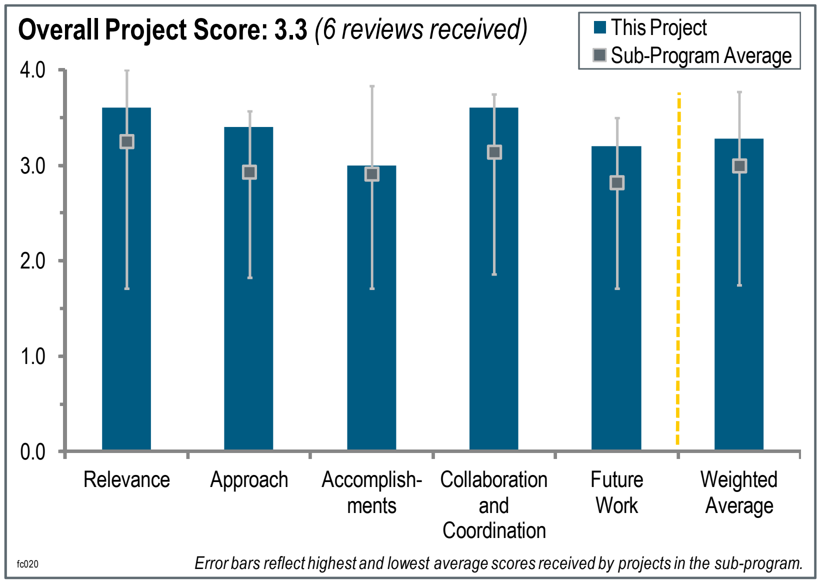
into two (or more) AMR presentations for fiscal year 2013. No reviewer comments from last year appeared to be addressed in the back of the presentation, perhaps because of the shift in companies.

### Recommendations for additions/deletions to project scope:

- It is recommended that the researchers look at medium-duty trucks as an interesting application in parallel with fuel cell buses.
- This project should add an industrial review for each system type. This way, one can align the project with performance projections and the best practices of industry. In addition, this project should add metrics to show how this system cost correlates to other similar systems (on a cost/lb basis). For example, vehicles today tend to cost approximately $12/lb of final product. This type of gross comparison will show if the PI's estimates are in the ballpark of manufacturing cost estimates. It would also be very helpful to know how much steel, plastic, copper etc. is in each system, and what the raw material cost is relative to the system cost.

## Project # FC-020: Characterization of Fuel Cell Materials

Karren More; Oak Ridge National Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) identify, develop, and optimize high-resolution imaging and compositional/chemical analysis and preparation techniques for the µm-to-Å scale characterization of the material constituents comprising fuel cells; (2) understand the fundamental relationships between the material constituents within fuel cell membrane electrode assemblies; (3) integrate microstructural characterization within other U.S. Department of Energy (DOE) projects; (4) apply advanced analytical and imaging techniques for the evaluation of microstructural and microchemical changes to elucidate microstructure-related degradation mechanisms contributing to fuel cell performance loss; and (5) make capabilities and expertise available to fuel cell researchers outside of Oak Ridge National Laboratory (ORNL).

### Question 1: Relevance to overall DOE objectives

This project was rated **3.6** for its relevance to DOE objectives.

- This project has a continuing history of providing insight into fuel cell functions using the newest techniques of microscopy.
- This project is an imperative analytical tool for major technical problems of automotive fuel cell materials and their materials research.
- This project is key to providing an improved understanding of the performance of catalyst materials.
- This is a valuable, core project in the DOE Hydrogen and Fuel Cells Program (the Program). It provides broad access to electron microscopy for projects studying fuel cell materials, in particular the structure and distribution of catalyst particles.
- This proposed effort focuses on using advanced electron microscopy tools for understanding the structure morphology of membranes and catalysts for fuel cell applications. A very wide group of collaborators are included in this effort and represents a concerted and focused attempt to understand some specific phenomenological effects, including degradation. It is very appropriate for the overall DOE effort in the area of fuel cell development for automotive use.

### Question 2: Approach to performing the work

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

- It is good to develop the analytical method for ionomer/polymer electrolyte membrane (PEM) degradation. This area should be emphasized. For catalyst work, it is questionable whether it is worthwhile to keep working on platinum (Pt) nanoparticle analysis. It would be good to shift the project focus to novel catalyst structures, such as nanostructured thin film (NSTF), core shell, etc.
- This project provides a high level of "value added" because it goes beyond simple access to electron microscopes. The expertise made available at ORNL, particularly in the area of sample preparation, creates an efficient and productive resource. Having a common electron-microscopy resource for many research projects

capitalizes on the advantages that specialization offers, "leveraging" the knowledge gained in one research project for the benefit of many.
- In general, the applications are done at the state-of-the-art level and are needed at that level (good use of the high-end resources). Of course, there are some questions this method can and cannot attack, but it is useful and powerful for the right projects. The balance of what is done could be improved upon: in situ PEM analysis is probably the most important thing the researchers could do, and development of this technique could be a higher priority.
- The approach is well thought out and appropriately designed for the slated objectives. This year's effort is specifically tailored to understand the dissolution and migration of Pt particles as a function of various operating conditions. It must, however, be recognized that the measurements are very straightforward and that there is no complicated combination of tools and techniques in this effort.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The usefulness of the results is hard to judge because very few results were presented.
- In accordance with the stated objective, the accomplishments are good and on target.
- Slides 5–18 demonstrate that outstanding progress has been made this year in areas of interest to the U.S. DRIVE Partnership's Fuel Cell Technical Team (FCTT) and industrial collaborators.
- This project has made good progress for ionomer degradation analysis, and a good start on atom probe tomography to characterize NSTF catalysts.
- This project has developed valuable insight into how Pt moves in accelerated stress tests (ASTs), which is important because if mechanisms are different, then the AST may not be general or dependable.

### Question 4: Collaboration and coordination with other institutions

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

- This project is working and adding value to many of the best projects in the DOE portfolio. There are many active and valuable collaborations, not simply occasional conversations.
- The collaboration with partners seems good. However, it was not possible to evaluate the success of the partnerships with all the collaborators, most likely due to the time constraints of this presentation. As indicated in slide 19, the collaborations appear to be widespread across the community and progressing well.
- This project is inherently collaborative because it provides an expert technical resource to non-ORNL collaborators. The presentation demonstrated an impressive list of university, industry, and government partners (slides 2, 19, and the technical results slides).

### Question 5: Proposed future work

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

- A better translation of the investigations into applying the results should be attempted.
- It is suggested that the researchers focus on the development of new analytical tools (e.g., atom probe tomography). It is also good to focus on ionomer analysis.
- This project has valuable and appropriate plans with good prioritization of the tasks. It would be good to see more in situ fuel cell work.
- The proposed future work (slide 20) demonstrates informed planning to increase the relevance of the project. In particular, the plan is to reestablish a capability for in situ liquid transmission electron microscopy (TEM) after consultation with the FCTT.
- The proposed future work is in good measure with the previous results. The attempts to understand the coarsening of the particles and migrations in the surrounding structures, such as the membrane, are in line with what is required for durability studies. Also, the stated objective of understanding ionomer interactions with catalysts is important. Some further details would have been helpful in understanding how these measurements would further advance knowledge from the current state of the art.

## Project strengths:

- This project has good collaboration with many parties interested in this work.
- This project is providing a strong capability of electro-scanning analysis.
- This project has a long history of excellent work, the best quality tools, and is inventing technique over time to push what can be done.
- This project provides technical competence and relevant experience in the field of electron microscopy for the benefit of a broad range of projects within the Program.
- This project reflects the development of a unique user facility at ORNL for performing cutting-edge electron microscopy on interfacial structures used in PEM fuel cells. In this context, a wide network of collaborators has been established across the community of academia, industry, and national laboratories.

## Project weaknesses:

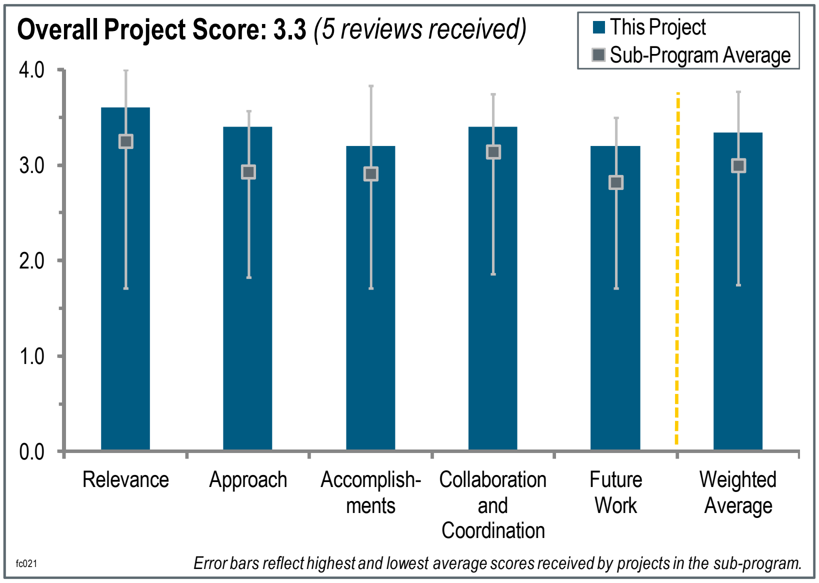
- The principal weakness is that most of the results are “expected.” This effort therefore does not significantly push the envelope of science. This is a problem with all technique-driven programs, which often get drawn into a technique looking for a problem rather than the reverse.
- The objectives of this project include, “make capabilities and expertise available to fuel cell researchers outside of ORNL.” This was on slide 3 in all capital letters. However, in the response to the 2011 reviewer comments, the researchers seemed to backtrack on this, saying, “We do not perform ‘the community’s’ routine microscopy.” Rather, their primary goal is to listen and provide answers. Further, they are also tasked with identifying and developing proper techniques. It is unclear what their balance between research and service is supposed to be, or whether or not they are at the intended ratio, but their project manager can easily review this and make adjustments as necessary.

## Recommendations for additions/deletions to project scope:

- Development of the in situ liquid TEM is of great importance, considering the nature of the interface being investigated in this program. This should be emphasized in the future.

# Project # FC-021: Neutron Imaging Study of the Water Transport in Operating Fuel Cells

Muhammad Arif; National Institute of Standards and Technology




## Brief Summary of Project:

In this project, the National Institute of Standards and Technology (NIST) will develop and employ an effective, neutron-imaging-based, non-destructive diagnostic tool to characterize water transport in polymer electrolyte membrane fuel cells. NIST will: (1) provide a research and testing infrastructure to enable the fuel cell and hydrogen ($H_2$) storage industries to design, test, and optimize prototypes for commercial-grade fuel cells and $H_2$ storage devices; (2) make research data available for beneficial use by the fuel cell community; (3) provide a secure facility for proprietary research by the industry; (4) transfer data interpretation and analysis algorithm techniques to industry to enable entities to use research information more effectively and independently; and (5) continually develop methods and technology to accommodate rapidly changing industry/academia needs.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- This project addresses important issues.
- It is probably impossible to achieve a water management project goal without good neutron imaging in real time of real fuel cell hardware. This facility provides a unique capability to DOE and industry researchers.
- The development of this technique for use with fuel development is interesting. If the technique helps understand water transport within a cell, it may meet the requirements of DOE. However, it is unclear how this expensive equipment can be used in cell design for most companies.
- The development of advanced diagnostic tools, such as those at NIST's neutron imaging facility, and making them universally available to researchers and fuel cell developers, provides a critical need to those developing technology to meet DOE's fuel cell targets.
- Neutron imaging is a powerful tool for probing into an operating fuel cell. It can provide valuable information as to the location and movement of water in an $H_2$/air fuel cell. The NIST facility is well equipped and the NIST personnel are well qualified to carry out the proposed research.

## Question 2: Approach to performing the work

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

- This project provides valuable analytical service.
- The approach appears sound, but the end use is unclear. The usage costs are also unclear.
- This project fully addresses barriers by allowing imaging in steady state and, most importantly, during transients. Other good aspects include the environmental chamber for freeze testing; the continually improving resolution; and the integrated approach, which allows both fuel cell components and stacks to be tested.
- NIST recently completed an upgrade of their facility, which includes higher-resolution imaging capability. With the development of these higher-resolution capabilities, the error and uncertainty analysis becomes even more

important, and it would be nice to see more attention to those areas. The new cold neutron capabilities should prove valuable for freeze start/tolerance studies.
- The NIST approach was primarily focused on building hardware components for the imaging of fuel cells and fuel cell components. It was not clear if the NIST researchers also contributed to the interpretation of fuel cell imaging experiments and the planning of future fuel cell experiments.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The accomplishments are interesting; however, the end use was not clearly evident.
- NIST provided an interesting expansion of tools and important insights for fuel cell developers.
- Limited data were collected at NIST's facility over the past year as the facility was upgraded. It will take some time before one can assess whether or not these upgrades will provide value. Still, many users continue to use NIST's facility as part of the their fuel cell research and development effort, including several that were highlighted in the DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) presentation, so clearly the facility continues to provide a great value to fuel cell development in the United States.
- The new hardware installed included a full-scale fuel cell test stand. With 0.10 micron resolution, a 25-fold increase in resolution was achieved. The goal of 1 micron resolution is still to be achieved, but the path to 2 micron resolution was achieved. New capabilities of imaging with cold neutrons are being designed and a new standardized high-resolution test cell fixture was built.
- A majority of the slides dealt with the hardware and test cells that are available or that were developed/constructed at NIST. For example, results were shown to demonstrate that the prior 250 micron resolution has been improved to 25 microns (a 10-fold enhancement in resolution), with an eventual goal of a 2 micron spatial resolution. While such work is important, how neutron imaging is helping researchers better understand fuel cell operation and the role of water during fuel cell power generation is of the most interest. In this regard, seven slides were shown of fuel cell data (of a total of 24 slides in the entire presentation), including a Los Alamos National Laboratory (LANL) study of non-precious metal catalysts, a University of South Carolina (USC) study on transient membrane hydration and conductivity, and a University of Tennessee (UT) heavy water/light water contrast radiography study. More explanation of how the neutron imaging results were improving fuel cell operation or improving understanding of fuel cell operation would have been valuable. It was unclear how important the fuel cell data shown at the AMR were.

### Question 4: Collaboration and coordination with other institutions

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

- The list of collaborators is impressive.
- This project has excellent collaborations with industry, national laboratories, and universities that are hard to criticize.
- The nature of this project is to develop advanced tools to meet customers' needs. NIST has clearly been successful in this area, as evidenced by the numerous projects that have effectively been carried out at the facility.
- There were excellent collaborations with other institutions; the NIST facility relies on other researchers who need NIST's neutron imaging capabilities. It was a bit disappointing to see that there was not a closer connection between the collection of data at NIST and the interpretation of the results at LANL, USC, and UT. It was not clear if there were face-to-face discussions of the neutron imaging results by NIST and these outside collaborators. It was unclear how closely the NIST researchers worked with their outside collaborators, and who will choose the next round of experiments—the outside collaborators (in which case the NIST people are just operating equipment) or a collaborative team of in-house and outside researchers.

### Question 5: Proposed future work

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

- The proposed follow-on work is interesting and appears to be in keeping with the project.
- NIST is continuing to develop a world-class facility; 1 micron resolution, when achieved, will be a world record.
- The focus on the higher (1–2 µ) resolution could help provide significant learning about transport issues within thin electrode and membrane layers. To date, several studies conducted at NIST have had to use components that are much thicker that those used in actual systems. However, these high-resolution studies will need significant time to run and will require substantial uncertainty analysis to determine accurately the component interfaces, whose roughness is often greater that the proposed high-resolution detector. The planned efforts to increase the field of view while maintaining resolution are certainly warranted.
- The AMR presentation lists future work, but it does not connect such future work to the needs of fuel cell researchers. For example, one future task is to measure the water content in an operating fuel cell membrane electrode assembly with a resolution of at least 5 µ. It is unclear why is this important, under what fuel cell operating conditions such data will be collected, what questions will be addressed in such experiments, how such experiments will advance the understanding of fuel cells, and how it will ultimately help us reach DOE's technical targets. It is unclear if the 5 µ resolution is a NIST scientific goal, or if such a resolution will give fuel cell researchers the information they require to answer fundamental questions regarding the performance of fuel cell components and the methods of fuel cell operation.

### Project strengths:

- The strengths are the knowledge and dedication of the project members.
- Lots of great work was still accomplished, despite the reactor being off-line for much of the year.
- The expertise of the neutron imaging people at NIST is unquestioned.
- Interesting data have been collected during the past year and numerous collaborations have been developed/cultivated.
- This project has a very strong collaborative effort with the fuel cell community and dedication to meet customers' needs. NIST has a unique, world-class facility that allows researchers to measure water profiles in situ that they would not be able to do elsewhere. The solid team at NIST continually strives to increase its value and build its customer base.

### Project weaknesses:

- The weaknesses are the lack of apparent direct and cost-effective applications to cell and system design.
- The type of analysis NIST and its users do requires significant error analysis to support many of the conclusions that are generated from the experiments that are done there. It was not obvious from the presentation how much effort NIST has spent on these analyses. It is very important that the users of NIST's facility fully understand the limits of its capabilities.
- The connection between the imaging results and improving the fuel cell operation/improving the composition/structure/performance of fuel cell components is tenuous. It is unclear if this is an academic/scientific exercise or if the neutron imaging work is critical to fuel cell development. This aspect was not adequately addressed. More/better interpretation of imaging results should be included in next year's AMR slides. Collaborators should be present in the audience during the presentation to answer questions regarding the rationale for performing the imaging experiments and the interpretation of the imaging results.

### Recommendations for additions/deletions to project scope:

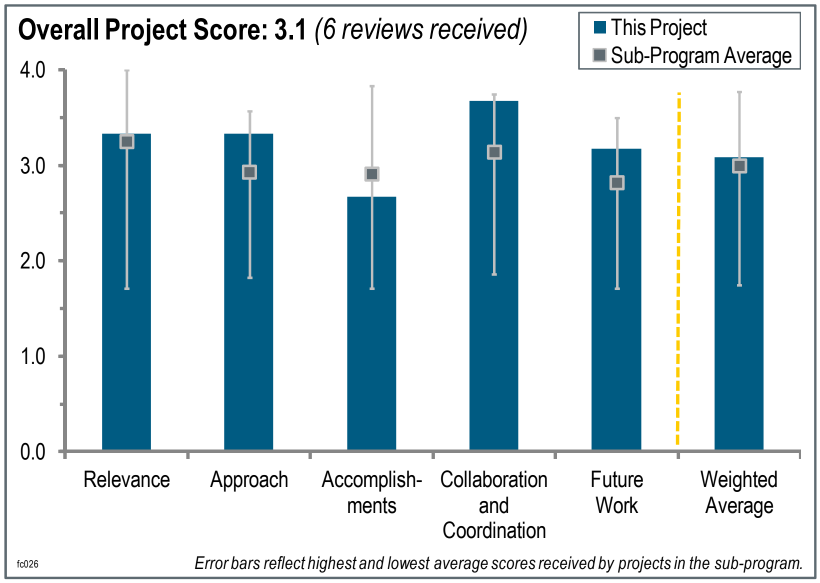
- The project scope seems appropriate, with the one exception of the additional effort spent on error and uncertainty analysis and educating the facility users on the limits of the techniques.
- As presented, the end use is unclear. Clarification on how these data are to be applied and the availability of this device to other manufacturers would greatly enhance the acceptance of this project.

## Project # FC-026: Fuel-Cell Fundamentals at Low and Subzero Temperatures

Adam Weber; Lawrence Berkeley National Laboratory

### Brief Summary of Project:

The objectives of this project are to: (1) build a fundamental understanding of transport phenomena as well as water and thermal management at low and subzero temperatures and (2) elucidate the associated degradation mechanisms due to subzero operation. Improved understanding of transport phenomena, water and thermal management, and durability at subzero temperatures will enable mitigation strategies to be developed to deal with degradation, thus allowing for the U.S. Department of Energy (DOE) targets to be met with regard to cold start, survivability, performance, and cost.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.3** for its relevance to DOE objectives.

- This project fully supports the water management and degradation (due to freeze) objectives of the DOE Hydrogen and Fuel Cells Program (the Program).
- This project is improving fundamental understanding of issues affecting not just cold starts, but also performance in polymer electrolyte membrane (PEM) fuel cells.
- Understanding operation and water management at low temperatures where flooding may occur is important, especially for thin electrode structures such as nanostructured thin films (NSTFs).
- PEM fuel cell start-up under cold or freezing conditions is very important for automotive applications. It is important to characterize, understand, and mitigate any damage that may occur with multiple cold/freezing start-ups.
- Understanding subzero start-up was a critical factor for fuel cell electric vehicle commercialization; however, it appears a number of original equipment manufacturers (OEMs) have solved this matter down to -20°C. It is unclear how relevant this project is today, especially with its very narrow focus of only on 3M materials.
- This effort is very well aligned with the goals and objectives of the automotive fuel cell effort of the Program. Understanding the freeze thaw effect of a membrane electrode assembly (MEA) is crucial to enabling this technology for applications. Because the interface is complex—comprising a proton conducting membrane; a gas diffusion media; and an interface composed of porous conducting support, ion conducting polymer, and catalysts—it is imperative to have a holistic approach as indicated in this effort where advanced tools and methods, electrochemical measurements, and computations are combined to provide specific indicators on the effect of freezing conditions in this three-phase interface.

### Question 2: Approach to performing the work

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

- Expanding the analysis to examine dispersed electrodes in addition to the NSTF has broadened the potential impact.
- The approach shown on slide 5 is outstanding and is a very good balance of experimental, modeling, and state-of-the-art analytical support. Focusing only on an NSTF catalyst, however, is the major negative of this project.

- This project has a very strong synergistic use of modeling and experiments. The model used is multiscale and includes all of the physical phenomena that are important. The use of experimental data for critical mechanisms related to cold operation and failure is critical. The researchers made a good choice of baseline cell assemblies.
- The top-level approach of addressing freeze and low-temperature operation through a combination of modeling and experiments is the appropriate way to address these complex issues. A quad serpentine cell is probably not the best choice for the baseline cell, because this design is not ideal in terms of water management.
- The approach of using advanced tools and methods, such as GISAX, capillary pressure, DSC, and electrochemical measurements (such as standard polarization and high-frequency resistance, with detailed computation) is comprehensive and very well designed to understand this complex three-phase interface system. The principal investigator has made very good progress in implementing this strategy, as manifested in the results shown.
- Multiple approaches are being employed to characterize and model cold or frozen water effects in simple PEM fuel cell configurations. One wonders if it is possible to employ some additional approaches to better determine where the water/ice is in the PEM fuel cell and to determine its amount in the different areas. For example, it might be possible to introduce a fluorescent dye into the water, if this will not complicate the fuel cell functions.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.7** for its accomplishments and progress.

- Very good progress has been made, as indicated by comparison with the results shown in 2011. Strong and close collaboration between various parties involved in this project is well indicated.
- The overall progress appears good, but there does not seem to be a clear progression to an identification of the key cold start “choke points” in the PEM fuel cell system yet. The inclined sessile drop measurement of hydrophobic and hydrophilic surfaces is good.
- The model validation, and subsequent application, to understand the implications of the measured properties is key. Experimental characterization, particularly catalyst water uptake, is filling knowledge gaps, but the lack of modeling results to demonstrate the sensitivity of measured properties is a concern.
- Good progress was made during the year, but the one criticism is that the team has left a lot to do in the last year to conclude the project. The excellent durability of NSTF in freezing conditions was demonstrated. The study of water uptake in catalyst layers and membranes was very nice.
- The accomplishments to-date are more or less in line with the well-established approach and work plan. What is not clear is why the performance of the 3M MEAs is so poor at lower temperatures (40°C). While subzero start-up is the focus of this project, it appears these MEAs are too poor for even a room-temperature start-up. It is unclear whether this a material issue or a hardware issue; either way, it needs to be addressed. The analytical work done on MEAs post freeze-cycling is very interesting.
- Good progress has been made in component characterization and determining the fundamental properties of the materials. The catalyst layer freeze durability results are interesting. More details regarding the conditions of freeze/thaw cycles would be appreciated. Durability is expected to be highly dependent on the condition of the catalyst layer prior to freezing. The difficulty with obtaining reproducible results between laboratories/cell-testing sites has slowed progress. Model convergence has also delayed achieving milestones, and convergence issues are a concern. This should be given top priority moving forward.

### Question 4: Collaboration and coordination with other institutions

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

- This project has excellent collaboration with national laboratories, universities, component manufacturers, and OEMs.
- This project has an excellent set of collaborations designed to cover the range of key programmatic needs.
- This is an outstanding team that has been organized, and the work from each partner is clearly illustrated.
- The teamwork and coordination seem strong; it would be helpful to more clearly identify which members are responsible for the individual tasks and accomplishments.

- Collaboration between the partners is good. A collaboration with an OEM or fuel cell manufacturer experienced with water management and freeze issues with conventional plate and dispersed platinum (Pt) catalysts may be beneficial to insure testing is being done under relevant conditions for freeze and restart.
- Outstanding collaborations are indicated among Los Alamos National Laboratory (LANL), 3M, United Technologies Research Center (UTRC), and Pennsylvania State University. LANL data on fuel cell durability is tempered with 3M supplied materials and tools, and methods such as DSC from Pennsylvania State University. Modeling and parametric analysis is closely coordinated between UTRC and Lawrence Berkeley National Laboratory.

### Question 5: Proposed future work

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

- The future work looks reasonable.
- Testing of NSTF with alternative anode gas diffusion layers (GDLs) is important because 3M has seen substantial benefits with different anode GDLs.
- The proposed future work is in line with the progress made. This proposed effort is well in line for making significant contributions to knowledge on freeze tolerances in such PEM interfaces.
- It is great to see that the NSTF with the PtNi alloy will be tested. A very comprehensive component characterization will be achieved, especially as a function of water content. The development of a transient model will be vital.
- The future work plan is acceptable, provided the following two criteria are well understood; otherwise the output of this project will not have so much value to the fuel cell community. The researchers should work to understand why the 3M MEA is so poor at 40°C–55°C, and implement some more benchmark data with traditional Pt/C electrodes.
- There have been a number of studies of water leaving GDLs, both ex situ and in operational cells. It is not clear what the experimental studies will add, though a thorough analysis of the available information and improved model formulation could be valuable.

### Project strengths:

- The project features a good multidisciplinary and collaborative approach.
- The project has assembled and excellent team, and has excellent project management and capabilities.
- A strength of the project is its multidisciplinary, multilength-scale combined theoretical and experimental approach.
- The project is well organized, and the team is thoroughly investigating fundamental properties and physical processes.
- The materials properties studies (catalyst water uptake, DSC of freezing in catalyst layer, droplet adhesion force) are excellent.
- This project is well designed to answer the complex question of durability mechanism under freeze conditions. Recognizing the role of freezing in altering the nature of a three-phase interface, as in the case of a conventional PEM fuel cell, this effort combines advanced tools and methods such as SAXS, DSC, and HFR with hierarchical modeling of transport and kinetics. Good progress has been made in implementing this strategy with good collaborative effort.

### Project weaknesses:

- This project has no weaknesses.
- It would be nice to see a larger variety of MEA configurations.
- This project focuses too much on 3M NSTF electrodes. The fuel cell data is too low and needs to be understood.
- This project has some model convergence issues, which is a serious concern and must be solved for the project to be successful. The modeling is crucial to understanding the experimental results, and without a robust, validated model there will be little advancement.

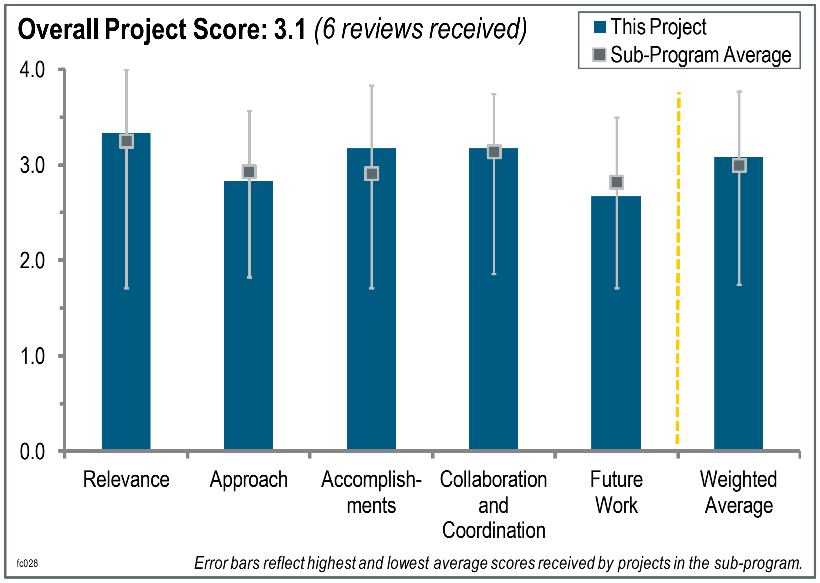
- The principal weakness of this effort is that it does not adequately deal with freeze cycle transients, which would manifest itself in a stack. Most of the effort is designed for single-cell-level work; however, there is a complete set of issues that are stack level, which this proposed effort does not address.

### Recommendations for additions/deletions to project scope:

- This project should expand to Pt/C, at least to benchmark and try to improve cooler IV performance.
- The researchers should try to find some better ways to track the water/ice locations and its content in the PEM fuel cell in a cold-start.
- DSC work does not provide the same type of information as IR thermography. There may be other analytical methods that would provide data regarding pore structure and filling with ice or water, such as X-ray tomography to see water/ice interface or advancing ice surface.
- While a close collaborative effort is manifest in this work, the role of 3M seems to be simply related to being a source of electrode materials; its contribution in terms of validating some of the results generated in this work would be more beneficial to the overall program.

## Project # FC-028: Transport Studies Enabling Efficiency Optimization of Cost-Competitive Fuel Cell Stacks

Amedeo Conti; Nuvera Fuel Cells




### Brief Summary of Project:

The objective of this project is to optimize the efficiency of a stack technology for fuel cells by using a combination of high current density with low platinum (Pt) loadings to meet the U.S. Department of Energy (DOE) 2015 cost targets. A model capable of predicting high-current-density operation in different architectures is the central deliverable of the project. The high-temperature operation has been explored in both single-cell and full-format stack testing to address requirements.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The project is focused on achieving fuel cell cost by optimizing design and operating envelopes.
- The project is very relevant, but raising the efficiency of a specific stack may not help other systems that are likely to become commercial.
- The program target cost and performance values for a polymer electrolyte membrane stack are relevant to DOE's objective. The targeted values are focused on national goals.
- The project addresses barriers and technical targets that are critical to the Hydrogen and Fuel Cells Program. When successfully executed, this project could have a large impact in the understanding of water transport within fuel cell stacks.
- A validated multiphase model that is sensitive to controllable material and design parameters is critically needed. The main question is if enough information will be released to enable other developers to exercise the same optimization of stack efficiency.
- This project is well aligned with the DOE Hydrogen and Fuel Cells Program. The key to developing low-cost fuel cell stacks is to be able to use models to predict the performance achievable under different operating regimes. This allows industry to conduct the relevant trade-offs to understand the technical feasibility and cost of each of the different system components (including stacks).

### Question 2: Approach to performing the work

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

- This is a sound approach with a clear path forward that employs the strengths of the participating partners.
- The approach is conventional, a combination of design, modeling, fabrication, and testing. The basic thesis was to address target (goal) parameters by increasing current density levels to high values.
- The temperatures and pressures used should be reevaluated, keeping in mind the practical values for radiator size and heat rejection.
- The project team's approach of developing models based on single-cell data and validation at the stack level is sound. Good mapping of operational parameters was carried out for the higher-cost membrane electrode assembly (MEA) case to form the baseline against which the lower-cost MEA could be compared.

- Modeling and comparing open flow field and land/channel architectures establishes the flexibility of the model. Focusing on cost and optimizing designs and operating conditions to meet cost is a unique approach and could reveal weaknesses in the approach of focusing on performance first and cost later.
- The program approach seems comprehensive. However, it is not constrained by the heat rejection target. It seems that this target is regarded as a “nice to have,” and the optimization has continued without this as a constraint. This renders the technology useful for only niche applications. DOE must focus on major market penetration.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The basic target, performance based on a metric of current for each milligram of Pt catalyst, was demonstrated. There was no convincing evidence that the cost target was addressed or achieved.
- Good progress has been made on the project, and it is clear that the MEA technology developed and showcased in this work has the potential to meet the DOE performance targets. What is less clear is whether the technology has the capability to meet the durability targets. This is not achieved through stable operation at one operating point for a number of hours. It should be assessed effectively with a meaningful accelerated stress test (AST).
- Several new techniques were developed to experimentally determine key parameters important for fuel cell modeling that did not show any agreement in the literature. The presented new techniques show that the team is creative, understands its research topic, and is making exceptional progress. Experimental results indicate improvement in material development to control water management in the stack.
- Performance and modeling targets of the project have been met at low loadings. The projected cost target of $15/kW can be met with low platinum group metal (PGM) loading and high current density in a four-cell, full-format stack. Durability data should be presented, especially at higher temperatures. Q/iTD findings should be reported.
- The model predictions are very impressive. It seems the model can predict open flow field and channel performance very accurately. If this model is this robust under many other operating conditions, it is state of the art. The lack of published results about this model begs many questions, though. A full disclosure of the model in a peer-reviewed forum is strongly encouraged.

### Question 4: Collaboration and coordination with other institutions

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

- The project partners appear to be engaged and contributing well to the technical output.
- The research team is adequate for the scope of the project. Input from the auto industry is received through the U.S. DRIVE Partnership’s Fuel Cell Technical Team.
- This is a good team with industry and university partners. There is a clear separation of tasks between partners that requires a significant amount of communication and team work to accomplish the project goals. The progress shows that this collaboration was executed successfully.
- The collaboration seemed to involve vendors and suppliers. It was impossible to discern the roles of each partner, other than it appeared that the university partner was involved in testing and modeling, and that the MEA partner supplied fuel cell hardware.

### Question 5: Proposed future work

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

- More details about the material optimization should be disclosed. It is unclear where the magical improvements in efficiency were coming from.
- The team needs to address how the model will be made publicly available for other groups to gain access to and use. The future work on materials optimization should be preceded by AST evaluation of the lower-cost MEA technology.

- The future work section is in line with the proposed approach. It was unclear whether the model would be expanded to a full stack model. More demonstration is needed that show the project made progress in improving the addressed technical targets. No information was shared with respect to a cold start-up time.
- There are five months left. Modeling future work will exercise and validate the model over a broader envelope of temperature, PGM loading, and cell architecture. The model will be published. Experimental work will focus on improved MEAs.
- The project is 90% complete and the future work was essentially to close out the project, show final stack test results, and publish the report. One cannot tell if the results presented have value for future commercial fuel cell hardware.

### Project strengths:

- The team is competent.
- The perspective received from a "cost first" approach is valuable.
- This project has good linkage between single-cell and stack data, and it has solid input from collaborators.
- This is a comprehensive project that includes fundamental aspects as well as a development aspect with the target to understand and improve water management.
- This project has strong collaboration with suppliers and universities, and it has demonstrated a clear improvement in Nuvera's product performance.

### Project weaknesses:

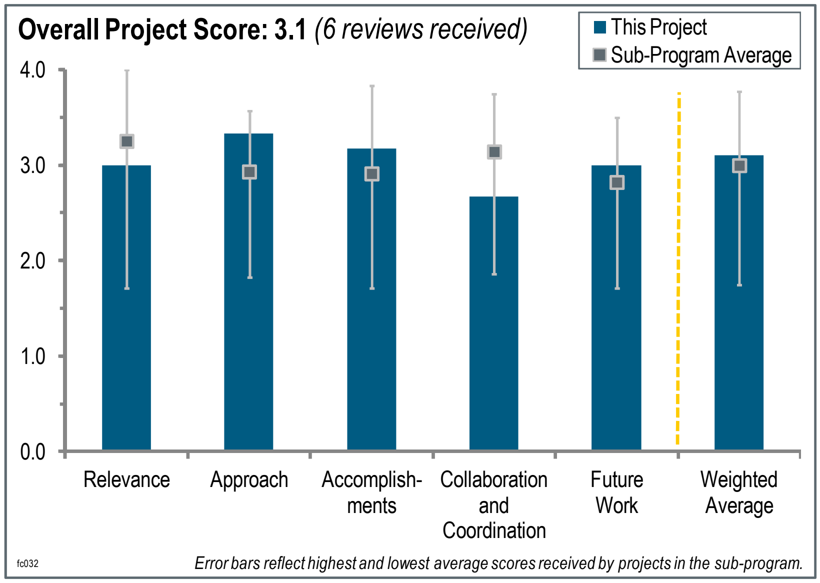
- This project considers performance in isolation of durability.
- It is not clear how the DOE-funded technology improvements will transcend to the broader fuel cell development community.
- The data was not really convincing. There was concern (expressed in previous DOE Hydrogen and Fuel Cells Program Annual Merit Review comments) that the proposed operating temperature was too low. Therefore, operating temperatures were increased, and it was apparent that it was the intent to go to even higher temperatures. It is true that the balance of plant, especially the thermal management components, get large and costly when operating at low-temperature (room temperature) stack. However, there is reason not to get too hot, because then, for automotive applications, considerable energy loss occurs because the stack must be heated, and start-up energy eats away at the anticipated fuel economy. Temperatures in excess of 85°C usually are not necessary. Even though there was considerable concern expressed about temperature, there was no description of thermal management or any estimates of the cost of that hardware. It would have been appropriate to present information that the earlier review team was correct or not so correct by showing estimated designs and costs for thermal management subsystems for a range of operating temperatures. There was no suggestion of experimental error in measurements. Some data suggested very limited durability, and indeed there is a calculation showing that the stack operates best when the water balance is at net zero (the ratio of so-called back diffusion and electro osmotic drag fluxes is one). This is usually true, as others have shown. However, it usually has proven difficult to park the stack at that point, even for a short time. That concept has not been shown to be useful.

### Recommendations for additions/deletions to project scope:

- More details about the material optimization should be disclosed.
- Use of state-of-the-art electrocatalysts that will also help raise the efficiency is suggested.
- The team should demonstrate how start-up/shutdown behavior can be improved with the generated results.
- The team should conduct limited work on ASTs leveraging other project information or capabilities prior to committing funds to further design optimization.
- There is still some time left on the project. One would like to see a series of replicated experiments, even polarization curves. Data at one fixed temperature is fine; there needs to be some limited measurement of durability. Even a run of operation for 24 hours could address those concerns.

# Project # FC-032: Development of a Low Cost 3-10kW Tubular SOFC Power System

Norman Bessette; Acumentrics Corporation




## Brief Summary of Project:

The objectives of this project are to: (1) improve cell power and stability of the cell building block within solid oxide fuel cells (SOFCs), (2) reduce the cost for cell manufacturing by improving processing yield and productivity while decreasing material consumption, (3) increase stack and system efficiency, (4) meet system efficiency and stability goals through prototype testing, and (5) integrate to remote power and micro combined heat and power (micro-CHP) platforms to allow short- and longer-term market penetrations.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- This project is advancing fuel cell technology without relying on a hydrogen ($H_2$) infrastructure.
- Certain project aspects such as cost reduction and micro-CHP and other applications align with the DOE Hydrogen and Fuel Cells Program and fully support DOE's research, development, and demonstration objectives.
- This work supports DOE's goal to integrate the fuel cell technology into remote power and micro-CHP platforms to allow short- and longer-term market penetrations.
- The project is addressing several DOE goals, including cell and stack power density, cell and system cost reduction, system efficiency, and system durability and lifetime.
- This project has been structured to be responsive to the DOE Office of Energy Efficiency's (EERE's) Fuel Cell Technologies Program (FCT Program) goals and objectives. Originally it was in the Solid State Energy Conversion Alliance program (a government-industry partnership supported by DOE's Office of Fossil Energy), and more recently it moved to the FCT Program, where it has been underway for four years and is approximately 95% complete.
- This program seeks to reduce costs in distributed SOFC power plants. It seems only tangentially related to EERE missions, because it only appears to address very niche markets that seem more appropriate for the military. Some of the progress could be brought to bear on larger SOFCs, although the tubular technology is generally not regarded to be cost effective at larger scales, so the alignment in this respect is not great.

## Question 2: Approach to performing the work

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

- This project is focused on key barriers, such as cost reduction and performance stability.
- This project is addressing the appropriate critical barriers of power density, durability, systems integration, and cost.
- The technology development barriers have been clearly identified, and actions have been taken on each.
- The approach taken in this project is to focus on cell performance improvements and improving manufacturability at the cell level, then addressing issues of stack assembly and cost reductions.

- The program has approached cost reduction in credible ways by exploring high power densities, reducing the number of components, improving Takt time, and improving life-cycle costs through durability improvements.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has made good progress toward the objectives of cost reduction and performance stability.
- This project made significant progress early on increasing power density for tubular geometry (although it is still less than planar geometries), and since then it has increased durability (10,000 hour test), worked on systems integration and development of recuperator and cooling, and minimized process steps to reduce cost.
- Significant accomplishments have been achieved during the past year. Current density has been increased to meet project goals with stable, long-term operation. Significant reductions in cell processing times have been achieved, balance-of-plant (BOP) parasitic power losses and the number of fuel cell modules were reduced, and commercial units have been placed in the field (nine months in the field with more than 90% reliability).
- It looks like the project team has made great strides in improving manufacturing cost and quality for the fuel cell stacks, and it has started work on BOP costs. Field demonstration units look like they will deliver the performance needed, but the main question is on durability.
- Researchers were able to maintain voltage stability while increasing current density from 150–250 to 350mA/cm$^2$ over the life of the program. There is now a fully automated cathode and barrier layer spraying in the manufacturing process. The team also developed a tri-sinter process for tubes, and thermal insulation cost was reduced by 65% with an 85% potential reduction number of parts cut in half.
- The progress on durability seems good, with demonstrated stability at constant power hold, thermal cycling, and variable ambient temperature conditions. However, no life-cycle targets were provided, so it is hard to understand where they are with respect to requirements. The progress on cost reduction is even harder to assess because no information is provided on what the target cost is, nor how the demonstrated cost reduction activities are correctly pareto-ed to address it. The presenter did refer to having cost models, but this was not included in the presentation. If this information is business sensitive, the researchers should use pie/bar charts showing relative magnitudes but omitting values. Related to this is the reduction in Takt time for processing the tubes. While a substantial fraction was reduced, the processing time is still measured in days. It seems that very large production volumes would require huge capital investments, so understanding whether this is feasible is critical.

### Question 4: Collaboration and coordination with other institutions

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

- This project collaborates with Ariston Thermo Group (Ariston).
- There are some collaborators, but it is not clear from the presentation what their roles are.
- The project is working with eight strategic partners. They are making units for the military, and some 36 units have been fielded. The 91% availability is good.
- There is an Italian partner (Ariston) that is obtaining a European Certification (CE) marking for a micro-CHP system.
- There does not seem to be any participation by partners, though the overall score should not be negatively impacted because of this.
- Acumentrics claims a number of strategic partners, but it is not clear that they played any significant role in the present project except possibly as a sponsor or customer. The project is heavily focused toward bringing a product to market, and it seems reasonable to maximize the in-house effort as much as possible.

### Question 5: Proposed future work

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

- The proposed future work for this project is appropriate.
- The project ends in fiscal year 2012, but the proposed future work seems like a logical extension of the work to date.

- The proposed work focuses on durability, cost reduction, and transfer to markets indicative of a maturing technology.
- The project is essentially complete and the remaining effort will support micro-CHP development, which appears to be reasonable.
- Without understanding the magnitude of the gaps, it is hard to assess how the articulated future work will address these.
- The future work is consistent with program goals: complete RP-20 micro-CHP integration and continue cost reductions on each product platform.

### Project strengths:

- This project has made good durability progress.
- This project has been making steady progress over time.
- This project is developing a maturing fuel cell technology that does not rely on $H_2$ infrastructure.
- This project has strong in-house technical, engineering, and manufacturing capabilities.
- This project is focused on key technical barriers, the development of systems and products, and prototype demonstration.
- It looks like Acumentrics has made great strides in reducing the manufacturing costs of its stacks and that it has a plan of attack for BOP components. It is very promising that the combined heat and power system is on track to get a CE mark.

### Project weaknesses:

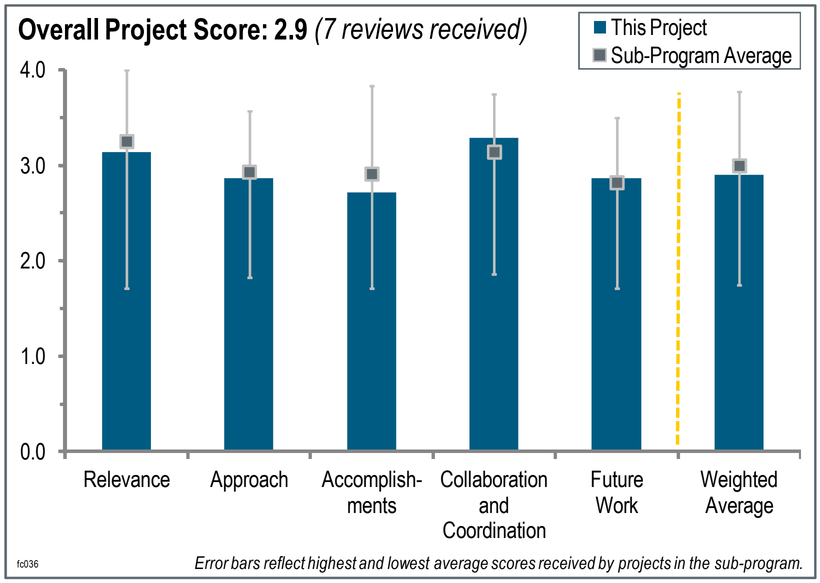
- This project's metrics/targets are not well defined.
- This project could use further power density improvement.
- The rate of progress seems appropriate, but it might have been faster.
- This project needs to better articulate its requirements and how current/future measures go toward meeting them.
- The required durability for a home appliance has not yet been fully demonstrated, adding to the risk of demonstrating the combined heat and power system.

### Recommendations for additions/deletions to project scope:

- This project appears to be near completion. Remote natural gas applications at the wellhead seem to be a good emerging market. The natural gas composition at the wellhead needs to be investigated for compatibility with fuel cells.

## Project # FC-036: Dimensionally Stable High Performance Membranes

Cortney Mittelsteadt; Giner Electrochemical Systems, LLC





### Brief Summary of Project:

This project identifies pathways to dimensionally stable membrane (DSM) support fabrication. Initially, the project investigated various approaches to identify a scalable and cost-effective route to fabrication. Currently, the project is pursuing three fabrication routes: (1) ultraviolet curing of polymers between a mold and a backing substrate, (2) mechanical deformation via mold pillars, and (3) precipitation of polymers on a mold using a non-solvent.


### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.1** for its relevance to DOE objectives.

- Durable, high-performance, and low-cost membranes are critical to achieving the overall the DOE Hydrogen and Fuel Cells Program targets.
- This project addresses key barriers in the Program related to durability and the cost of membranes.
- Developing low-cost, dimensionally stable polymer electrolyte membranes (PEMs) is critical to the Program and is also beneficial to hydrogen ($H_2$) production via electrolysis.
- DOE's objectives include fuel cell cost and fuel cell durability. If successful, this project could impact goals posted for those objectives.
- This project addresses the durability and cost of room temperature to medium temperature PEM fuel cells. This also applies to high-temperature PEM fuel cells. With modifications, it could also apply to solid oxide fuel cells.
- This project aims to increase membrane durability by adding new support layers with micro-fabricated openings. This directly relates to increasing durability. The project also aims to address cost barriers, but direct evidence of cost reduction was not presented.
- Development of new membrane mechanical supports should be a relatively low priority for the Program. Adequate mechanical supports have already been developed by several stakeholders. At this point, improving ionomer properties should be the highest priority in membrane research and development, but this project lacks an ionomer development component (understandably so, given the relatively small size of this project).

### Question 2: Approach to performing the work

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

- This project is using an innovative approach that enables low tortuosity and high-temperature stability (120°C).
- This is a nice approach to manufacturing fuel cell membranes with increased mechanical durability.
- Using a strong web and optimizing its geometry to contain an ionomer could be an enabling technology for using polymers that have good conductivity but no strength/crossover resistance.
- This multipronged approach is beneficial and increases the likelihood of success. The mechanical deformation process should work for a wide variety of materials and should be easier with thinner supports. The materials selection appears limited for the ultraviolet (UV) micro replication approach and could involve a substantial amount of work to develop a UV curable thiolene with appropriate mechanical properties.

- The project comes down to building a supported membrane to achieve a strong but thin ion exchange separator component. In fact, almost all useful fuel cell membranes are supported, so the project, which focuses on supported membranes, brings no novelty. This issue then comes to lower cost and higher performance. The approach described were tasks in manufacturing engineering that strive to identify relative costs for three conventional manufacturing approaches.
- The 2DSM approach is inherently limited by the hole size. Fabricating supports with extremely small holes is difficult. The 8 micron holes indicated as the lower limit of hole size are still large enough that a significant quantity of ionomer will not be active (i.e., the ionomer layer on the parts of the support away from the holes will not carry any significant current density and is essentially wasted). The inhomogeneous distribution of the support within the membrane (support-free layers above and below the support) is also a concern in terms of the creation of internal stresses during relative humidity cycling. The approach to support fabrication, which involves the parallel development of three possible fabrication techniques, is good, though more details about the mechanical deformation procedure would be appreciated.
- State-of-the-art membranes already meet durability targets. This project does not show a significant ability to reduce costs over current membrane supports. This project therefore does little to enhance membrane technology over what is already available. Although developing new supports is interesting, there seems to be little likelihood that these supports will be utilized in future membrane electrode assemblies (MEAs). None of the techniques presented has high porosity (more than 50%), which will make reaching conductivity targets more challenging. Also, the in-plane dimension of the nonconductive support layer is on the same magnitude as the through-plane thickness. This may lead to non-uniform catalyst utilization, which may decrease catalyst effectiveness and reduce MEA durability. Routes to mass production are not presented (and are not easily envisioned) for micro-replication and mechanical deformation. It is not certain if any of these techniques can be reduced to practice for less than $\$5/m^{-2}$.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.7** for its accomplishments and progress.

- This project has made good progress on all three parallel methods.
- Some progress was apparent, but it is obvious that the early results are not fully convincing that promise has been demonstrated. Indeed, this micro-fabrication remains a tough task.
- Even though the approach is nice, the lack of progress and accomplishments was disappointing. It is not clear if this was related to funding or the process to manufacture the membranes.
- Membrane supports were fabricated on a bench top level and possible interesting candidate techniques were identified. It is hard to say at this point in the process whether or not these materials will be able to meet target goals once incorporated into ionically conducting membranes.
- A number of ways of achieving the same result have been demonstrated in this project, indicating the approach is valid. The approach did not get rated a 4 because the most favored approach, mechanical deformation, was minimally discussed, because it is proprietary, and so it is difficult to appreciate the validity of this approach.
- The project is progressing well in terms of developing viable fabrication pathways for each of the three techniques. The mechanical deformation technique seems to be lagging behind the others though, with support porosity still far too low. The lack of a schedule with milestones makes it difficult to assess the actual progress versus what was planned. The projected costs are of some concern. The lowest projected cost for the supports is $\$20/m^2$, which is equal to the cost target for the entire membrane; however, the principal investigator (PI) indicated that further reduction would occur with volume.
- Three potential pathways for DSM-support fabrication were developed and all were found to be viable and have the potential for scale-up. The supports were thicker than optimal for PEM fuel cell use. The three methods appear to have been demonstrated with fairly thick (approximately 10 microns) supports. They should be demonstrated with thinner supports (approximately 5 microns and possibly less), which would be used in approximately 15 micron or less membranes. This may prove to be more difficult for some of the approaches. The mechanical deformation samples shown had square holes, which may concentrate stresses at the corners. It is not clear if this would cause problems or if the mechanical deformation technique can provide round holes. The phase-inversion casting process results in porous supports. It was mentioned that thermal treatment can reduce this porosity to about 25%, but the temperature needed and the effect of the porosity on the mechanical

properties of the support was not fully discussed. Current thiolene's mechanical properties were not substantially better than Nafion.

### Question 4: Collaboration and coordination with other institutions

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

- This project has very productive collaborations with industry and academia.
- The partners and degree of collaboration are appropriate.
- The collaboration seems to be mainly with vendors.
- The work was well distributed between partners and the collaboration shows synergy.
- This project had a great set of partners that brought a lot of expertise from industry and academia with a nice role.
- It appears the collaboration between the project partners is working well. Collaboration with a PEM ionomer/membrane supplier could prove beneficial in the next step.
- Giner, University of Massachusetts, Impattern Technologies, and Colorado Photopolymer are an effective team. More active participation by an end user might be helpful, although it appears General Motors, Ford, and others are aware of this work and have been giving inputs.

### Question 5: Proposed future work

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

- This effort is well focused on achieving the program targets.
- This project is continuing to improve preparations in reasonable ways. Through-plane conductivity work is laudable and should be correlated to all preparations; correlation of the crossover of reactants for all preparations would be interesting.
- The future work path is not clear, and one cannot tell how these supports will be optimized or when ionomer impregnation will be incorporated. The path to reach DOE's goals is not specifically identified.
- The need to down-select to a process and optimize will be critical in the future work, which is believed to be in the last year. MEA fabrication and fuel cell qualification will be key to validating the DSM and should have started before now.
- The future work described sounds appropriate. Given the pace thus far, and recognizing the remaining problems with some of the fabrication techniques, actually achieving all the described tasks remaining is doubtful.
- There is still much to do before the project is complete. Some of the candidate specimens are just now being prepared. These test pieces are the reinforcement and will need to be built into useful membranes. The loaded ionomer parts will then need testing. No credible pathway forward was apparent in the presentation.
- The three methods appear to have been demonstrated with fairly thick (approximately10 micron) supports. They should be demonstrated with thinner supports (approximately 5 microns and possibly less), which would be used in approximately 15 micron or less membranes. The ability of the support to reduce X-Y swelling and chemical and mechanical durability and performance of the membranes should be demonstrated at these thinner thicknesses as well.

### Project strengths:

- This project has a strong team and good results.
- This project addresses cost issues for proven 2DSM technology.
- The techniques used for support development allow for a wider variety of materials to be used as supports.
- The membrane and electrode assembly is an important cost issue. This activity works to lower cost.
- This project provides a good membrane that is both dimensionally stable and has the potential to be lower in cost. The proposed manufacturing process seems to be very scalable.
- Pursuing three processes in parallel increases the chances of achieving both the performance/durability targets as well as the cost target.
- Giner Electrochemical Systems is leveraging the expertise gained from work on an earlier DOE project. The relative simplicity of the 2DSM is a strength in terms of ease of fabrication and support/ionomer integration.

### Project weaknesses:

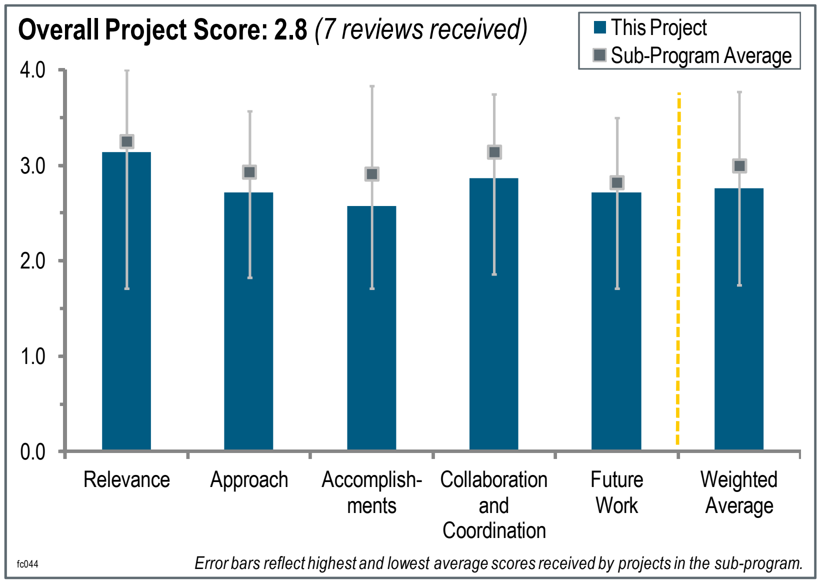
- This project has no weaknesses.
- Weaknesses include the low porosity of supports, high cost compared to current available supports, and support geometry that may not maximize durability.
- The inactive regions of membrane ionomer due to shadowing by the support and the possible mechanical problems due to a lack of support distribution throughout the membrane are weaknesses of this project.
- It seems that not much progress has been made since last year. Also, converting these DSM materials into a complete MEA for fuel cell qualification would have been good to start before the last year. Also, more details on the potential cost of such systems would have been nice to see.
- There should have been early work on the reinforcement-ionomer merger. There are many issues with adhesion, mechanical and thermal stability, chemical incompatibility, etc. that are potentially challenging. Much effort has been spent on getting the strengthening component; however, that is just a piece of the target.

### Recommendations for additions/deletions to project scope:

- Performance evaluation data is very important to have before completion of project.
- Possible additions include modeling the current distribution through the pores of the membrane and how this will affect electrode utilization, and load cycling durability of MEAs made with new supports.
- The three methods appear to have been demonstrated with fairly thick (approximately 10 micron) supports. They should be demonstrated with thinner supports (approximately 5 micron and possibly less), which would be used in approximately 15 micron or less membranes. The ability of the support to reduce X-Y swelling should be demonstrated at these thinner thicknesses as well.
- It is too late in the project to change the scope, but for future work the PI may want to consider developing a current density distribution model, which could be used to design a support structure that minimizes effective membrane resistance while maximizing mechanical strength. Such a structure would have to be much more streamlined than the current 2DSM structures.
- The project needs to consider a basic "test" of the concept. It makes sense to pay for a few laser-drilled polymeric test pieces to be made and then to load those specimens with ionomer. (Yes, this test specimen will cost "too much," but it is not the car part.) The researchers should boil that ionomer-plastic component in water for a few hours, and see if things stay together, then test conductivity. This need not be as thin as the projected component. It makes no sense to move forward with a polymer that cannot work. There also should be some tests on polymer stability and the effects of constituents released from the polymer (the reinforcement), as well as the effects of those constituents on fuel cell durability. Nafion, in its commercial applications, is sold as a reinforced composite using Teflon, which is not a cheap material and not very strong. It can be assumed that Teflon was chosen because of its inertness and that Teflon "debris" does not foul the electrochemical cell. There should be some suspicion about thiolene as a fuel cell component. Certainly sulfate is OK, such as sulfonic acid. However, reduced sulfur compounds tend to poison electrochemical processes and low-cost, commercial polymers are probably not going to be useful in fuel cell components.

## Project # FC-044: Engineered Nano-scale Ceramic Supports for PEM Fuel Cells

Eric Brosha; Los Alamos National Laboratory




**Brief Summary of Project:**
The objective of this project is to develop a ceramic alternative to carbon material supports for a polymer electrolyte membrane (PEM) fuel cell cathode. Ceramic supports require enhanced resistance to corrosion and platinum (Pt) coalescence; can preserve positive attributes of carbon such as cost, surface area, and conductivity; and are compatible with present membrane electrode assembly (MEA) architecture and preparation methods. Goals for the ceramics include possessing the required surface area; fostering high Pt utilization; and exhibiting enhanced Pt-support interaction, adequate electronic conductivity, and corrosion resistance.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.1** for its relevance to DOE objectives.

- This project aligns well with DOE's objectives.
- This project supports all three critical DOE objectives: durability, cost, and performance.
- This project is very relevant because it address carbon support corrosion/oxidation issues. These are major outstanding issues with the durability of fuel cells with respect to voltage degradation. While there are system mitigation strategies that can be put in place, in the longer term, a stable material is required to reduce overall system cost.
- The stated goal is to get rid of carbon because, "in order to make durable ceramic for fuel cells we need to get rid of all carbon." This statement is not necessarily true, and perhaps prevention of carbon corrosion is a better objective. Getting rid of carbon would be great, but it is unlikely that any material will meet carbon's cost and electrical/electrochemical properties.
- This project clearly addresses the durability of dispersed platinum group metal (PGM) catalysts by replacing carbon with an oxide. Whether such a material substitution can address the cost and performance barriers is highly speculative, because the very nature of the enhanced conductivity properties that such oxides must meet will make it harder for nanoparticles to perform as well on a conductive support. The limitations of specific activity and Pt dissolution under high voltage cycling of particles with small radii of curvature will still be present.
- Corrosion-resistant supports for fuel cell catalysts may not be essential for adequate durability because system controls may adequately mitigate stressful conditions, such as start-stop and hydrogen ($H_2$) starvation. However, corrosion-resistant supports would simplify the requirements on control systems and balance of plant, and could lessen the durability/reliability impacts of control-system malfunctions. Therefore this work, while not necessarily required for the primary path to affordable, durable fuel cells, could contribute to the development of more robust systems. Non-carbon supports are sufficiently difficult to develop that some work to develop them should continue, even if they are not deemed to be the primary path to mass-produced fuel cells. If carbon supports come up short despite all efforts to mitigate the severe stressors, it would be good to have non-carbon supports available as "plan B."

### Question 2: Approach to performing the work

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

- The technical approach used in this project is adequate and well defined.
- It is necessary to show that material (ceramic support) screening for corrosion resistance and electrical conductivity may be based on the state-of-the-art carbon support.
- To be successful, Los Alamos National Laboratory (LANL) will have to develop a carbon-free support that still has a large enough surface area. The synthesis will also have to be scalable and the supports will need to fit into an existing MEA.
- It is unclear why proceeding with C-$Mo_2C$ was a go decision.
- With $Mo_2N$, it may always be a trade-off between having no free carbon and having a high surface area. Both are needed.
- The down-selected approaches have many promising properties among them. There is still some concern that some approaches still involve the use of carbon to get either conductivity ("black" $TiO_2$ phase) or durability (carbides). This appears to have been born out of the results from the initial accelerated stress test (AST) measurements.
- Although the work presented is good, there was insufficient information to establish whether a structured approach is being used. Theory and modeling are used to support the experimental approach, but it is not clear how much they are guiding the work.
- The correlation of the high-level targets into specific support requirements has not been made clear. For example, it was not clear if there was a minimum conductivity required for the support, or if there is a stability that should translate into meeting the targets. It is not clear if a gated process has been used for down-selection of support materials. The characterization is stated as ongoing, but limited information on, for example, support conductivity has been shown.
- One of the stated goals is to have a support material with comparable conductivity to carbon, which is metallic. Unfortunately the nanoscale oxides and carbides selected, such as the oxygen-deficient titanates and MoC, are prone to hydration and possibly oxidation in the fuel cell cathode environment. The basic question is whether a metallic interface can be preserved between the Pt catalyst and the support, and between the supports in the fuel cell environment. From the electrochemistry, it appears that LANL has very highly resistant electrodes. Thus, there should be much more thought about how to maintain interparticle metallic conductivity and more of an emphasis on surface, rather than bulk, properties.
- The overall primary approach toward generating conductive supports with good corrosion resistance and adequate surface area through templated synthesis (polymer-assisted deposition [PAD]) is, in theory, a good one, because most other syntheses of conductive support candidates do generate quite low surface areas.
- The project has properly placed increased emphasis on electrochemical evaluation of materials this year, but it still needs to improve its methodologies, or at least its reporting metrics. Instead of reporting "Kinetic currents at 0.8 V," LANL should use the standard metric of A/mg PGM at 900 mV RHE. Rotating disk electrode (RDE) PGM loadings should be adjusted to give between 0.5 $mA/cm^2$ and 1/2 of the limiting current at 900 mV.
- More emphasis needs to be placed on direct measurements of conductivity after exposure to relevant conditions. It was unclear if $Ti_4O_7$ maintains adequate conductivity after exposure to oxygen reduction reaction (ORR) conditions. It is unclear if nitrides and carbides stay in that form, corrode, or turn into insulating oxides, and if the surfaces of nitride or carbide particles get passivated by an oxide that decreases conductivity to an unacceptable level.
- It seems doubtful that the theory is advanced enough yet to be able to predict adequately whether electronic conductivity will be maintained through the surfaces of these supports when they are in contact with acidic electrolyte over the relevant potential range. This is a central issue in the development of non-carbon supports.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- Good progress has been made so far toward the project and DOE's goals.
- It is necessary to identify the reason for poor fuel cell performance.
- Very good progress has been made. A significant amount of high-quality results have been generated.

- Overall, good progress has been made in electrochemical evaluation by RDE and MEA, even if the results are not that good. At least LANL is showing conclusively that their supports do not work.
- LANL has established improved stability for supports when compared to carbon supports. LANL has also determined that residual carbon is an issue resulting in reduced stability.
- LANL has attempted incorporation into MEAs and MEA testing, although performance is poor due to integration issues.
- The surface area stability of the AST of the zero-carbon-containing oxide ($Mo_2N/Pt$) shows excessive (60%) Pt electrochemical surface area lost after 10,000 cycles. This may be a general concern because Pt dissolution mechanisms from repeated oxidation and reduction are not so support-dependent and will remain a concern with any dispersed nanoparticles.
- The difficulty of making dispersion ink with very different types of catalyst particles should not be underestimated and may prevent the ability to deduce solid conclusions about the performance or durability of the catalyst particles in MEAs. The best fuel cell performance shown is still an order of magnitude away from state-of the-art Pt alloy/C performance (0.2 vs. 2 $A/cm^2$ at 0.6 V under 30 psig $H_2$/air). This is a huge gap to overcome just by electrode processing.
- This is a very difficult area in which to work, and one must expect most of even very well chosen systems to fail, but one might have expected a bit more systematic work to address questions of conductivity and durability.
- LANL has shown that it is difficult, and perhaps impossible, to get carbon-free $Mo_2N$ through polymer-assisted deposition, and it has shown that the significant carbon content contributes to a lack of durability under severe carbon-corrosion tests. This is an accomplishment, even though the outcome is not positive.
- It seems somewhat late in the project to just now be starting attempts to synthesize carbon-free $Mo_2N$ by converting $MoO_3$, though this is a good idea.
- More corrosion data should be generated and shown, and the issue of loss of conductivity due to the formation of thin layers of insulating oxides on the surface of the conductive particles when exposed to realistic ORR conditions needs to be addressed.
- To maintain conductivity of $TiO_2$ under ORR conditions, one probably needs metallic dopants rather than just growing an oxygen-deficient oxide. Doping was mentioned in the presentation, but it appears that no data on doped titania were presented. Last year's DOE Hydrogen and Fuel Cells Program Annual Merit Review presentation for this project improperly listed $NbO_2$ as a conductive oxide, but Nb can be an effective dopant in $TiO_2$.

### Question 4: Collaboration and coordination with other institutions

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

- LANL has limited collaboration.
- LANL has good collaboration with partners.
- LANL's activities are well coordinated among the team members.
- Collaboration with an existing company in the fuel cell area could give ideas for a path forward.
- Collaboration between the laboratories in the project appears adequate. No-go materials are coming from different laboratories.
- The computation work seems to be concentrating on Pt/oxide interactions rather than the more critical surface conductivity issues, but the latter may still be intractable.

### Question 5: Proposed future work

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

- An XPS evaluation of the surfaces of these ceramics would be very beneficial.
- The planned work on characterization and fuel cell and durability testing is appropriate.
- The proposed future work is adequate and laid in accordance with the project objectives.
- It is suggested that LANL focus on fuel cell performance before going to the durability assessment.
- Some attempt should be made to characterize surface properties with transmission electron microscopy and XPS, although neither is an in situ method.

- The future work planned appears consistent with the prior years' work, but it is not encouraging that it will overcome the very poor performance and substandard durability that the new materials have shown to date.
- The continued emphasis on totally carbon-free supports seems justified, as when carbon is present it is hard to tell whether Pt supported on the non-carbon phase is electrochemically active.
- The planned attempts to make fine-grained $MoO_3$ and convert it to the nitride seem wise.
- The return to $TiO_2$ work is appropriate, but major emphasis should be placed on metallic doping to maintain conductivity.
- Increased emphasis is needed on measuring electron conductivity after exposure to ORR conditions.
- Activity and durability should be measured in MEAs, where conductivity issues become more challenging.

### Project strengths:

- Good institutions and personnel are involved in this project.
- A bold approach to change how fuel cell MEAs are built is a strength of the project.
- LANL identified promising supports with techniques used to produce high surface area.
- LANL has a very good team. Each team member brings relevant expertise to the project.
- This is an innovative idea, especially if a non-carbon-containing support with high surface area can be obtained.
- The dedication to get the carbon out rather than using carbon additions to patch up inadequate electron conductivity is a strength of this project.

### Project weaknesses:

- Having to add carbon to try and generate the conductivity lost is a weakness of this project.
- Poor performance may require a Pt loading increase in catalyst layers.
- LANL did not show a clear screening or modeling link to support properties/characterization.
- The original concept of using oxide particles as supports for nanoparticles to improve Pt surface area stability is a weakness of this project.
- There is not enough emphasis on controlling and characterizing the interparticle resistances, which are likely to govern the resistances of the electrodes.
- The future of such support is unclear.
- The high cost of the PAD process is also a weakness.
- LANL needs to improve the volume and quality of electrochemical testing. The planned testing in MEAs as well as RDEs is critical; conductivity issues are harder to deal with in MEAs.

### Recommendations for additions/deletions to project scope:

- LANL should incorporate surface analysis of the materials using XPS.
- LANL should include comparisons to most stable commercially available carbon supports.
- LANL should provide structured and gated approaches to selecting and developing supports.
- The electrical conductivity of the electrocatalyst should be measured and included as one of the parameters that can be correlated to the performance of the MEAs.
- The researchers should add direct testing of conductivity in support powders after exposure to relevant ORR conditions.
- LANL should try to initiate a calculational effort to address bulk and surface conductivity issues and not just Pt-binding energies.
- It is not clear that LANL will be able to demonstrate any significant path to overcoming the major barriers addressed by the project. However, the argument can be made that this body of work is justified to answer those very questions about the utility, or lack thereof, of metal oxide supports for dispersed nanoparticle electrocatalysts. It was unclear if going back to the titania supports would really improve the large performance gaps.
- LANL needs to make metallic surfaces, put Pt on them, and then characterize them. LANL could develop a bulk metallic oxide electrode and decorate it with Pt. Some sort of coating to control the oxidation of the oxide/carbide surfaces could be used. The oxide/carbide surfaces need to be characterized.

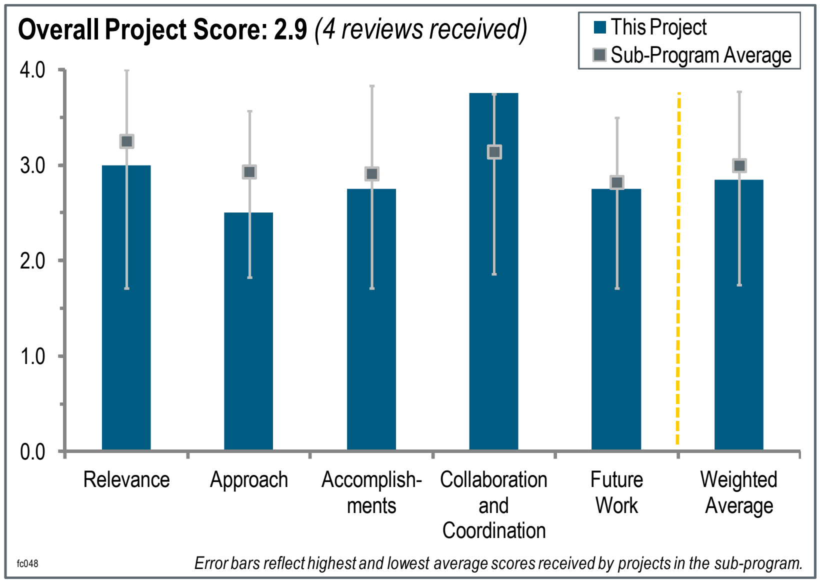
- LANL should put a very thin layer of their oxides or carbides on carbon. Then they would have all of the benefits of carbon (cost and high conductivity), but eliminate the Pt-C contact, which is the trigger for the carbon degradation.
- LANL's results show that carbon is a pretty handy thing to have in an MEA.

## Project # FC-048: Effect of System Contaminants on PEM Fuel Cell Performance and Durability

Huyen Dinh; National Renewable Energy Laboratory


### Brief Summary of Project:

Core project objectives are to: (1) identify fundamental classes of contamination in polymer electrolyte membrane fuel cells, (2) develop and validate test methods, (3) identify the severity of contaminants, (4) identify the impact of operating conditions, (5) identify poisoning mechanisms, (6) develop models/predictive capability, and (7) provide guidance on future material selection. Successful completion will increase performance and durability of fuel cells by limiting contamination-related losses and decreasing overall fuel cell system costs by lowering the balance-of-plant (BOP) material costs.


### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The project seems relevant to DOE objectives and targets, but there is some question as to the relevance of those to hydrogen ($H_2$) technology.
- This project will allow the manufacturer to select the material of construction (primarily for ancillaries), knowing the impact on performance/durability.
- This project tackles the very important issue of contaminants that are introduced by either the components used for the stack or the BOP or by the cathodic air flow. The potential $H_2$ impurities are not considered.
- Over the course of the years of fabricating fuel cell electric vehicles, developers have commonly run into failure modes that stem from the release of materials within the system, whether it is from elastomers or metal cations from the BOP, or the release of similar contaminants from bipolar plates and seals. A project that seeks to address contamination from within the system is relevant to development. What compromises the probability that the end results of this project will be relevant to any individual developer is the low probability that an individual contaminant studied will be the same that actually affects a developer. Developers "pick their poisons" by which material sets are down-selected for the various systems and stack components. The premise of this project creates quite a challenge to actually find system-induced contamination that will be experienced universally by developers.

### Question 2: Approach to performing the work

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

- The approach is based on the material screening of commercially available materials and those that are available as improved non-commercial qualities from different manufacturers.
- There is some question as to whether the leak tests are truly representative. While it is known that there needs to be some acceleration to get appreciable leak amounts, it is not known what the acceleration factors truly are, and thus this needs to be addressed.

- The specific concentrations of contaminants and model compounds will depend on fuel cell design, so the results will not be directly applicable. The trend in material classification, however, will hold, and the results will help stack designers to choose appropriate materials.
- Ideally, the approach should include studies at low platinum (Pt) loadings that reflect what the status of the technology will be near commercialization. The cathode loading used in this project is 0.4 mg $Pt/cm^2$, which is much higher than program targets. It is difficult to see how the project will eventually draw conclusions regarding the different contaminants tested. Voltage loss is measured versus total organic carbon (TOC) and concentration, but it is unclear how the project intends to derive conclusions from these data. There would have to be some comparison between an accelerated concentration and a realistic concentration to see what the acceleration factor is, and whether unacceptable degradation would occur during vehicle lifetime. It is unclear (1) how the realistic concentration is determined, and (2) whether the accelerated concentration level causes degradation modes that would not be experienced at the lower realistic concentration.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project still needs to develop correlations between ex situ analyses and in situ performance losses, but the project is well on target at this time.
- The project is on target and many tests for many materials have been accomplished. There is a question as to the importance of the dosing concentrations on performance and whether the concentrations are too high.
- Species being released from polymers have been determined; select species were investigated in depth in parametric studies; studies that include a variety of operating parameters are ongoing; and different levels of degradation have been identified and materials have been classified.
- In terms of a gross number of experiments (660), it is evident that the project has done a considerable amount of work. The problem with the project is what has been reported, which does not provide direct conclusions regarding any particular contaminant. Acceleration levels are not reported; all that is reported is voltage loss versus a given TOC and concentration. Some questions remain for each contaminant. It is unclear what the mechanism of degradation is, if the degradation is recoverable at a lower concentration, if contaminants crossover to the anode, and what the mechanism of recovery is, if one exists. It might help to see the Zytel voltage loss plotted versus TOC and concentration. Silicone degradation trends have already been acknowledged in the fuel cell research community.

### Question 4: Collaboration and coordination with other institutions

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

- This project has no issues with collaboration.
- The project is large, but it seems well coordinated with a good team. BOP material suppliers, however, are one stakeholder group missing from the team.
- The project consortium is well tailored and consists of academia as well as industry. A wide selection of samples of different manufacturers has been acquired and tested; hence the project is well interconnected with industry.
- The collaboration between the National Renewable Energy Laboratory and General Motors (GM) appears to be strong. GM has helped considerably in the survey of possible contaminants and in developing the criteria by which contaminants are selected for study. The contributions of the other organizations (3M, University of Hawaii, Los Alamos National Laboratory, University of South Carolina, and Colorado School of Mines) are described broadly in one slide, but it is not apparent exactly how their individual contributions advance the project.

### Question 5: Proposed future work

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

- The future work is sound.

- There is a lot of good proposed work, which may be a bit overambitious. It should be better prioritized toward more examination of durability and identification of critical parameters. It is not clear that good correlations exist for the proposed contaminants and screening tests. It would be good to focus on some ex situ parameter estimations, such as membrane conductivity and rotating disk electrode testing with the different model contaminants instead of full cell testing initially.
- The project plan does not address the combination of contaminants/model compounds. The testing matrix would become unmanageable, though. It might be interesting, based on material selection recommendations, to check the effect of a combination of contaminants; one could imagine that some material selection could have a negative impact (more than just additive) on the fuel cell performance.
- What is described for future work is fair, and hopefully the project has the statistical prowess to be able to do all of what is described. Considerable data mining will be needed to draw conclusions about the mechanisms involved for each contaminant, and how the effects of each contaminant may vary with concentration and operating conditions. The project has 18 months remaining from the time the slides were due. The question exists as to whether in that time the project will both be able to determine mechanisms for degradation and then be able to enter them into a model that predicts the effects of contamination. It would be interesting to know if there is a model that has been validated for non-contaminated performance, which can then be modified to account for contamination.

### Project strengths:

- The screening used is a quick route to good results.
- This project has a strong team with the appropriate tools/techniques, and it should be successful.
- The capability to test both ex situ and in situ with the various expected contaminants is a strength of this project, along with a good team.
- This project has demonstrated the ability to perform an incredible number of experiments in a short period of time. The project has an extensive knowledge base about possible contamination and about which contaminants are likely to exist through fuel cell system operation. The project has interactions with numerous stack manufacturers: GM, Nuvera, and Ballard. The project has been able to establish experimental methods to treat both liquid- and gas-phase contaminants.

### Project weaknesses:

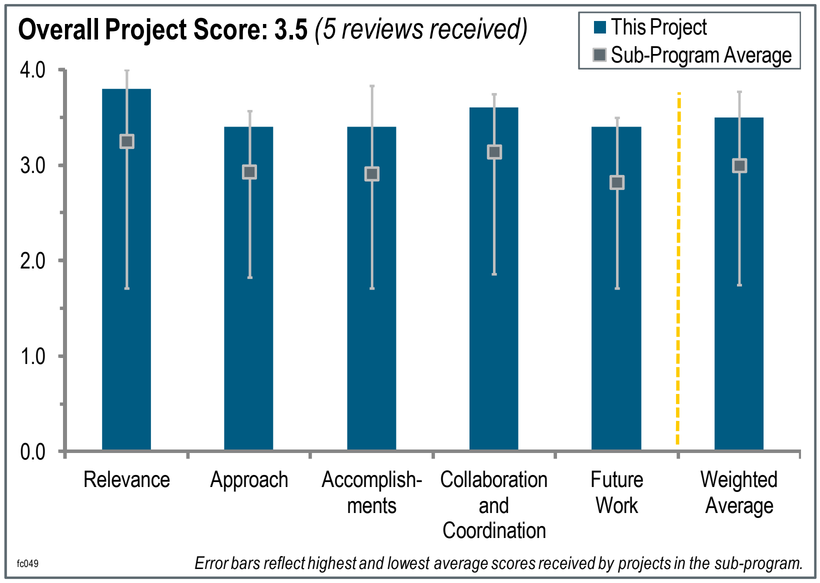
- Very high levels of contaminants are used in this project.
- It is not clear if clear trends or validated simple tests exist and will be generated in this project. It is also unclear as to the output of the screening back to the manufacturers.
- The fact that each contaminant is studied individually may be a weakness, but one needs to start somewhere in understanding the impact of material selection and their contaminants on fuel cell performance.
- The project has had difficulty drawing specific conclusions about contaminants and providing some degree of direction as to how the gathered data might eventually be used by manufacturers to drive component selection. The project needs to identify contamination mechanisms that reach beyond what is already understood (e.g., silicone constituents cause membranes to get brittle and greater amounts of cations reduce membrane conductivity).

### Recommendations for additions/deletions to project scope:

- This project investigates lower concentrations of impurities, which are more likely to be present, and at what level critical contaminants are likely to occur.
- Something needs to be done to address acceleration factors and what concentrations are to be expected within the cell. Something like a generation amount versus a removal amount is required to see if various contaminants accumulate in the different fuel cell materials.
- If a baseline model does not exist, it may be worth considering whether developing a predictive model is within the scope of the project. The project should strive to observe trends among contaminants that feature the same functional groups. Some of the prior presentations from this project seemed to indicate that this direction was being followed, but this year’s presentation did not make that seem clear enough. It would be good to add some indication of how contaminants might be selected for long-term durability testing. If time does not permit, perhaps long-term durability testing should be removed from the project.

## Project # FC-049: Development of Micro-Structural Mitigation Strategies for PEM Fuel Cells: Morphological Simulations and Experimental Approaches

Silvia Wessel; Ballard




### Brief Summary of Project:

The objectives of this project are to: (1) identify and verify catalyst degradation mechanisms, including (a) platinum (Pt) dissolution, transport/plating, carbon-support oxidation and corrosion, and ionomeric thinning and conductivity loss, and (b) mechanism coupling, feedback, and acceleration; (2) correlate catalyst performance and structural changes through catalyst layer and gas diffusion layer (GDL) properties; (3) develop kinetic and material models for aging with a macro-level unit cell degradation model, micro-scale catalyst layer degradation model, and molecular dynamics degradation model of the Pt/carbon/ionomer interface; and (4) develop durability windows of operational conditions, component structural morphologies, and compositions.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The project goals are relevant to DOE's objectives.
- Durability is the most important issue with getting cars on the road. It also paves the way to further reduce Pt loading, and hence serves cost.
- It is necessary to move toward commercial stacks if the United States intends to compete in the fuel cell arena. Ballard knows well the cost, performance, and durability requirements and the project targets national goals.
- This is an important modeling effort aimed at understanding in very fine detail the reaction and decay mechanisms in a polymer electrolyte membrane (PEM) fuel cell.
- Studies of the durability of PEM fuel cell systems and materials are critical for defining the electrode properties and operating conditions that will enable these systems to meet the operating lifetime target of 5,000 hours for the automotive application.

### Question 2: Approach to performing the work

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

- The approach used is very good. There is a balance of theory and experiment.
- This project is directly tackling the issues of cell durability and performance by using very detailed analysis methods, which appear to meet experimental results.
- Ballard brings a new design and evaluation technology with the intent of demonstrating a new fuel cell stack. There is considerable analysis, with lots of technical variables. The team's approach is "conventional" (design, build, test), but that is the correct pathway.
- The portions of the project that are performed at Ballard, specifically the model development and sensitivity analysis, appear to have a sound approach and have made substantial progress in the past year. The overall approach of integrating multiple characterization techniques of the various electrode components does not appear to reach the objectives or add value to the approach.

- Because the materials are almost defined in PEM technology owing to longstanding research and development, the issue of microstructural mitigation is key for further advancement. The detailed approaches address the most important issues of catalyst degradation mechanisms and the correlation of microstructural changes and performance, as well as developing kinetic models and material models for aging. Operating conditions are considered in order to identify operating windows other than just keeping developing materials against electrochemical barriers. The targets are precisely defined and ambitious. The project combines cutting-edge approaches, such as molecular dynamic modeling, microstructural modeling of the GDL, and experimental verification in order to identify and understand the mechanisms for degradation, create design curves for degradation, and find operating windows in which the degradation is tolerable or negligible.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The accomplishments so far are promising. However, it is unclear to what extent this model will be available for other fuel cell developers to use once it is complete, or whether it will be so specific to Ballard technology that it will be of lesser value to others.
- The project began in 2010, and it is far along in its progress. There is emphasis on electric contacts in modeling, which is an important stack issue. This project is still evolving, building on testing results, and it shows good probability that new designs will improve performance and durability.
- A very complex molecular dynamic model of the Pt/C/ionomer interface has been completed and the one-dimensional membrane electrode assembly (MEA) model has been completed and verified. The experimental investigations have been completed, except for the interface characterization and the property changes of aged GDLs and catalyst layers. The overall progress is in line with the project plan.
- The progress shown has been very good and key milestones have been met. Model-based simulations have been experimentally validated. The results on carbon corrosion and Pt dissolution seem to be quite similar to other projects. One issue is the question of model discrimination and parameter estimation. It is not quite clear how many adjustable parameters there are in the model, and how the other parameters have been estimated.
- The project has made significant and valuable progress in developing and validating the microstructural and unit cell performance model. Significant progress was also made in the evaluation of the effects of Pt loading on initial performance and degradation rates, electrode composition, materials properties, and operational conditions on durability. It was not clear from the presentation what accomplishments have been made by the project partners in the past year and what characterization has been performed beyond electrochemical surface area, cell performance, crystallite size, and Pt in the membrane. For example, it is unclear if the University of New Mexico performed the XPS studies of carbon-corrosion mechanisms.

### Question 4: Collaboration and coordination with other institutions

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

- The team is quite strong and the presentation was integrated with contributions from the various partners.
- This project has a strong industrial lead provided by Ballard with very specific research contributions. All project partners are renowned or at least well known in their field.
- Collaborations are very strong and bring in many key institutions in the industry. This is reflected in the quality and depth of the work.
- It may just be the way the material was presented or the limited time for the presentation, but there does not seem to be significant contributions from the project partners.
- There are strong partners in the team, and Ballard seems to have its role well characterized. Ballard seemed focused on durability and appreciates that durability is much related to stack operation and maintaining the stack in safe operating parameter space during the entire stack lifetime. Certainly, other successful fuel cell stack manufacturers have also appreciated that a few minutes of operation under adverse conditions may turn out to cause performance degradation that might be observed after a few years of sensible operation.

### Question 5: Proposed future work

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

- The proposed future work is appropriate.
- The future work is sound with the project goals and appropriate to achieve the goals.
- The project needs to deliver a stack with demonstrated performance, and it looks like that goal will be achieved.
- The proposed future work shown in the presentation is vague, especially in terms of what future experimental investigations will be performed.
- The future work will provide important insights into addressing performance and durability barriers. However, the sole focus on Pt dissolution and carbon degradation misses the fact that most fuel cell systems in practice fail for reasons other than degradation, such as mechanical crossover failure of the membrane. It is a rare fuel cell that meets its end of life solely due to degradation of performance. Perhaps it is beyond the scope of this project, but it would be good to see some work done to address the actual reasons that fuel cells fail in practice.

### Project strengths:

- This project has a very robust analysis.
- This project has a strong consortium that is working toward resolving the critical issues of a real application.
- The emphasis on combining insights from theory and experiments and efforts taken toward model validation is a strength of this project.
- The team is excellent. Ballard demonstrated they know the problems and the proposed sensible solutions to address those problems.
- The project strengths are the careful and extensive experimental studies by Ballard and model development and validation at Ballard.

### Project weaknesses:

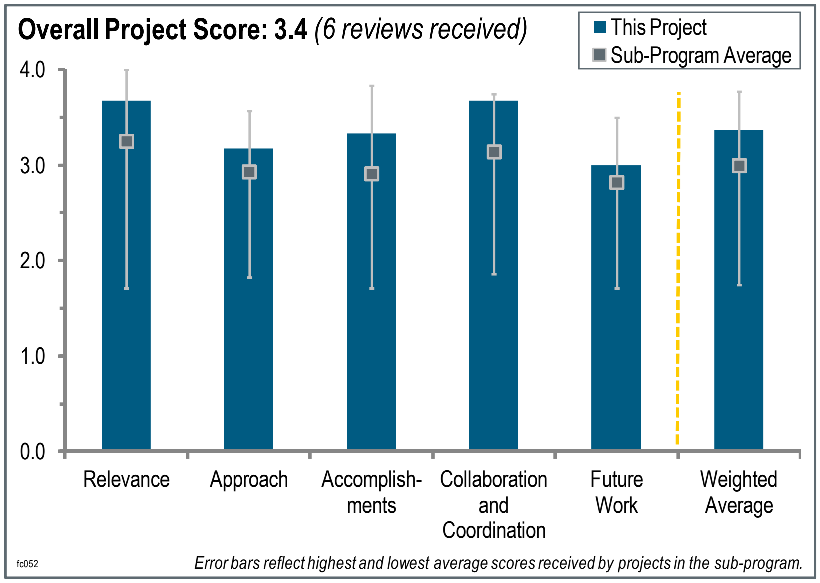
- This project has no notable weaknesses.
- This project is missing key failure modes experienced in practice.
- The lack of evident contributions from project partners and the lack of post-mortem materials characterization are weaknesses of this project.
- Not a weakness as such, but it is important to ensure that overlap with other national laboratory projects (Los Alamos National Laboratory, Argonne National Laboratory) is avoided. It seems like 2–3 projects have similar subsets of partners with different lead institutions. Some input on parameter estimation and sensitivity analysis should be provided.
- There are not really any weaknesses. However, Ballard is a leading stack producer and has sold many stacks within the global marketplace. Because those stacks are proprietary, both in design and performance, it is hard to judge the actual value of this improved design. This makes an informed analysis of the value of this activity difficult.

### Recommendations for additions/deletions to project scope:

- The researchers should add other failure mechanisms, such as crossover, poisoning, and membrane humidity issues.
- The researchers should add the effect of temperature on carbon-corrosion mechanisms to the project.
- The concept of a "durability window" is excellent (much like the "engine map" of an internal combustion engine).

## Project # FC-052: Technical Assistance to Developers

Tommy Rockward; Los Alamos National Laboratory




### Brief Summary of Project:

Los Alamos National Laboratory (LANL) provides technical assistance to fuel cell component and system developers as directed by the U.S. Department of Energy (DOE). This project includes testing of materials and participation in the further development and validation of single-cell test protocols. This project also covers technical assistance to Working Group 12, the U.S. Council for Automotive Research (USCAR), and the USCAR/DOE U.S. DRIVE Partnership's Fuel Cell Technical Team (FCTT).

### Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- This project serves as a vital link in technology transfer and technical assistance.
- This project is relevant "by definition" because the DOE Hydrogen and Fuel Cells Program (the Program) gives LANL direction. The portfolio described this year is more relevant than last year's efforts.
- This project has historically been well aligned. Because the work is not defined in advance, it is impossible to be sure it will remain so, but it seems highly likely. By offering DOE an independent judgment of methods and material function, this project is critical to the goals of the Program.
- This project is useful for generating the reliable benchmarking of fuel cell components at the domestic and international levels, and for providing technical assistance to the fuel cell and component developers to overcome technical barriers and achieve the goals set by DOE more quickly.
- The sharing of technical expertise developed by the national laboratories with support from DOE is an important and necessary activity for moving fuel cell technology forward. This project provides a great service to industry.
- The relevance of the developer support project was much improved versus last year. This particular year, the project focused more on tasks that addressed more essential questions related to fuel cell performance and durability. Some examples of the more fundamental efforts include: (1) analysis of in-plane currents during start-up/shutdown, (2) discovering improved mass transport with nanotube-incorporating microporous layers (MPLs), and (3) studying the durability of TreadStone plates. The study of plate surface energy versus performance serves as an example of efforts that overlap with work that has been done within the community. Consensus has already been reached that highly hydrophilic plates would be advantageous.

### Question 2: Approach to performing the work

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

- The description of how work flows in and the nature of the output was good.
- The project integrates very well with other DOE efforts and the U.S. DRIVE FCTT.
- The technical approach used in this project is adequate. LANL intends to use the standardized fuel cell test procedures to evaluate a variety of stack components objectively to address the main commercialization barriers: durability and performance.

- The approach is a little hard to classify except to say the tests done on the membrane electrode assembly (MEA) or plate level are done at small scale. This is a good and fast approach, though it can miss stack effects. In some past cases, the technology used in the rest of the system was not state of the art, and LANL could miss impacts on the newer systems if they differ from the older ones in response. This issue can only be addressed by providing LANL extra funds to update their equipment, and, if need be, for compelling the makers of novel state-of-the-art components (catalysts, membranes, etc.) to supply LANL with research samples.
- Because the project is dependent upon what developers bring to the project, the approach would not be fairly analyzed based on the particular efforts of the past year. In this project, the evaluation of the approach depends upon how discerning LANL is in understanding whether the tasks suggested from the outside are worthy subjects of investigation or just efforts that duplicate what has already been done. This past year, the project showed some improvement in this regard. There remain some questions regarding the hardware used for the investigators' work. It is worthwhile to contemplate whether the use of quad-serpentine flow fields is still part of the right approach in work that addresses components that may be used in automotive contexts.
- The approach builds on the expertise developed at LANL and other institutions, which is of great benefit to the emerging fuel cell technology. The study of a hydrophobic treatment of bipolar plates appeared questionable. UTC Power has used hydrophilic bipolar plates for more than 10 years. Early work at LANL suggested that thin film transport of water, rather than water "bubbles," was the best method for moving water from the system. The wicking techniques developed in the 1980s by Englehard supported hydrophilic bipolar plate concepts. The researchers should be aware of past results in open literature.
- The start-up/shutdown protocols are in the patent literature. It was unclear if this study attempted to repeat that work and if so, why.
- Testing metal bipolar plates is important. Gottesfeld at LANL conducted some similar corrosion studies that were not referenced in this work. Anode plate corrosion at the outlet is interesting, but the researchers did not say what local potential (electrolyte potential) was needed to develop this type of corrosion.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- A significant amount of high-quality results has been generated in a timely manner.
- Some interesting work was conducted this year, particularly the start-stop studies and the bipolar plate work.
- A good amount of work was completed in many different areas. The understanding gained in the two plate projects was very helpful in understanding corrosion and water management. The other work was extremely helpful to the principal investigators (PIs) involved.
- The correlation between Ballard and Nancy Université data was not explained. For the MPL material studies, characterization of the material and evaluation of performance versus either a previously tested baseline material or a DOE-relevant material, as well as communication of quantified properties, could be improved.
- LANL showed another example of how defects in MPLs enable greater water flux from the cathode catalyst layer by incorporating carbon nanotubes in the MPLs. It would be interesting to see images of the MPLs, but this work serendipitously adds to a growing understanding of water transport. The in-plane currents shown from the start-up/shutdown experiments show that the inlet of the cell contains more in-plane current than the outlet upon start-up. This result could suggest cell designs that could mitigate degradation.
- The study of hydrophilic plate treatments showed higher performance with greater hydrophilicity, which agrees with other studies. The project used a quad-serpentine plate design with small outlets, which may have exaggerated this conclusion. The study of the TreadStone plates showed, under somewhat accelerated circumstances (older membrane that likely has high HF release), where contamination may come from (wetter parts of the cell) and with what metals (iron [Fe], titanium [Ti]).
- The researchers have systematically demonstrated expertise in evaluating several polymer electrolyte membrane (PEM) fuel cell components. The application of the techniques developed by LANL and others has advanced the understanding of the chemical and physical properties of PEM fuel cells. There was no discussion of hydride formation on Ti-coated plates. It appears the researchers did not consider the formation of hydrides (hydride formation has been observed in electrolysis systems).
- It is not clear why the hydrophobic treatment of graphite bipolar plates was undertaken, unless it was requested by an outside source.

- The segmented plate study provides insight into the corrosion mechanisms occurring during start-up and shutdown.

### Question 4: Collaboration and coordination with other institutions

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

- The project activities are well coordinated among the team members.
- Evidence is good for strong collaboration with DOE and with the other companies.
- LANL has a large number of critical people with which to collaborate, a very helpful group that supports success in other DOE contracts.
- The team has a good breadth of organizations, including international organizations. Information sharing and communication with other DOE activities is excellent.
- The project is built around collaboration between national laboratories, academia, and industry. This is a great example of cooperative research and development to resolve technical problems.
- The project is entirely dependent upon collaborations, so by necessity the collaborations are close and wide-ranging. The list of collaborators reveals that there could be some improvement in incorporating stack developers. Ballard is the only stack developer listed.

### Question 5: Proposed future work

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

- LANL did not show a future work slide.
- The proposed future work is adequate and laid in accordance with the project objectives.
- As described, the future work is appropriate for now, because it seems to be dependent on future guidance.
- This is not really a fair aspect to grade this project on, because the future is not entirely in the researchers' control; rather, they attack the problems DOE directs them to address.
- As future work is determined by DOE, it is difficult to determine the scope of planned projects. An indication of trending topics for assistance or specific problem areas with which LANL may be best able to assist along with better communication of LANL's capabilities, with DOE agreement, may assist in gauging future plans for this project.

### Project strengths:

- LANL has good interaction with the industry.
- The expertise and cooperative efforts between universities, LANL, and industry are strengths of this project.
- LANL demonstrates an impressive analytical capability, particularly with regard to the elemental mapping capabilities and protocol development.
- LANL is well equipped to do a variety of tests in support of the major DOE research thrusts in fuel cells. LANL is flexible, skilled, and dedicated.
- This project has an excellent team, approach, and results. This is a great idea to have one of the most credible teams in the fuel cell industry available to help developers meet the developmental targets for their fuel cell component technologies.
- LANL can collaborate with a wide variety of developers, suppliers, and universities. LANL has shown the ability to work on catalyst layers, gas diffusion layers (GDLs), metal plates, and many other components. A high level of testing throughput exists at the PI's location. The PI has deep experience in fuel cells.

### Project weaknesses:

- There may be room for a little more autonomy on LANL's part.
- Some indication of DOE guidelines for selected projects would be beneficial.
- This project has insufficient funding to expand capabilities and have more developers involved in the project.
- Some of the evaluations appear to be unaware of previous literature, patents, or industry results.

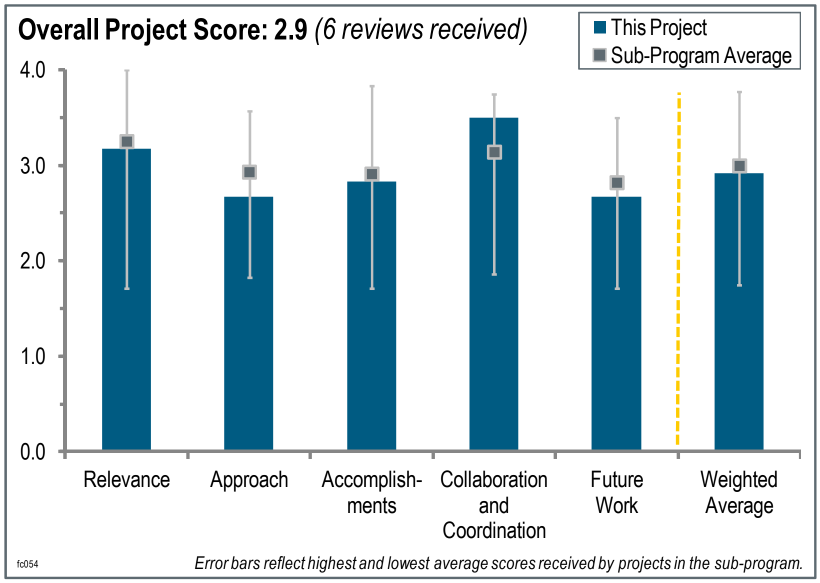
- LANL often uses cell formats that are outdated (e.g., 50 $cm^2$ quad-serpentine flow fields). The project is at the mercy of collaborators to find quality problems to tackle.
- There is always the potential for the equipment (and especially the parts of the system that are not being tested) to fall out of date, and it is key that the researchers' work remain funded at a level where they can continually update systems to reflect the state of the art in fuel cell design and a wide range of MEAs, GDLs, balance of plant, etc. as may be needed to answer some question of interest to the Program.

### Recommendations for additions/deletions to project scope:

- DOE needs to provide more funding for this project.
- This project should be continued and expanded to include other projects funded by the Program.
- LANL should outline themes or topic areas where they see opportunities to discuss with the FCTT. Then, LANL could work along those lines for a longer period of time with less direction being necessary.
- Because the next tasks for the project were not shown, it is difficult to provide commentary here. That said, LANL needs to stay aware of recent publications so as to avoid accidentally duplicating results that are well known or well understood.

## Project # FC-054: Transport in PEMFC Stacks

Cortney Mittelsteadt; Giner Electrochemical Systems, LLC




### Brief Summary of Project:

The objective of this project is to improve understanding of the correlation between material properties and model equations for polymer electrolyte membrane (PEM) fuel cell stacks. The project will: (1) supply model-relevant transport numbers, (2) stress the model by developing different materials with different transport properties, (3) determine sensitivity of fuel cell performance to different factors, and (4) guide future research.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- Water transport and the interaction with thermal management are important factors in meeting performance goals.
- This project relates directly to DOE's needs for understanding and increasing mass transfer effects within fuel cell components.
- This project is directly approaching several barriers with the intent to reach DOE's targets for cost and stack efficiency. Both of these targets are essential in moving PEM fuel cells toward commercialization.
- This project addresses several of the identified DOE technology barriers with a focus on stack performance, including cold start-up and stack power density and efficiency.
- While transport (water transport in particular) is critical to PEM fuel cell stack performance, this project is not particularly innovative; very similar research was already performed in past DOE Office of Energy Efficiency and Renewable Energy projects. Thus, the project's relevance to the overall objectives of the DOE Fuel Cell Technologies Program appears limited. It is also not clear what the main goal of this research is and how its ultimate success versus failure can be defined.
- This project addresses fundamental characteristics that are essential to every fuel cell model, particularly those associated with the membrane. These include the water diffusivity and electro-osmotic drag coefficient. At this time, many fuel cell models are attempting to address condensed water. This project also seeks to do this by understanding how the tortuosity and porosity of the gas diffusion media affect water flux across the gas diffusion layer (GDL). Developing a model is necessary for understanding how to improve power density while not compromising low-temperature performance. Greater power density lowers stack active area, which is the most powerful component toward reducing the overall cost of a fuel cell stack.

### Question 2: Approach to performing the work

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

- The approach for this project includes developing models, improving inputs for models, using the models to study sensitivity of fuel cell performance to various factors and, finally, using model results to guide research activities.

- The membrane characterization is providing valuable information. The intention to address effective GDL transport properties is good; the presented and planned work looks to result in only a characterization study with some potential flaws.
- This project approach represents a good, though not very novel, combination of experiments and modeling. At the present stage, this project should be focusing more on fuel cell stacks and less on routine testing of components in single cells.
- The approach is sound because it combines cost analysis, material development, and in situ testing with the development of a model that will be made public. It remains a little unclear to what extent the published model will be useful for third parties, because the details of the developed materials and the stack architecture will not be published.
- The approach seems to have both modeling and experimental components, but it is unclear how much each one relates or feeds into each other within this project. It is unclear how new model development is occurring and if there is transfer besides just the material functional properties. The method development is good, although pretreatment conditions should be noted for the membranes. It was not clear what the main driver for the plates is.
- The project's approaches for measuring water diffusivity through the membrane and the electro-osmotic drag coefficient are good and have been noted in the past for their novelty and the need that they fill to address these properties for more contemporary membranes. The methodology behind the porous media aspect of the model is missing. What is described is how the MacMullin number is calculated. However, this leaves unresolved how the mass transport losses are calculated from the MacMullin number for different operating conditions. There is some question as to whether water transport in the model is a function of GDL thermal conductivity, which other groups claim. This project is a good use of a variety of flow field designs and materials for model validation.

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Very good progress has been made toward the program targets in material development, experimental results, and the development of the model. It remains unclear if operations at high temperatures are sustainable over extended time periods. Results should be created or shared that indicate at what temperature operation is sustainable.
- The project has made good progress during the past year. A major milestone was accomplished on time and another is 60% complete (due 8/15/12). Progress has been made on identifying new membrane materials and incorporating them in membrane electrode assemblies (MEAs) for testing, and a new technique for simultaneous water uptake and diffusivity was devised.
- The team is progressing on schedule and the work seems to be well integrated. The membrane characterization, in particular, continues to show interesting and valuable results. The presented model validation studies met the team's targets, but they were not a stern test. The modeling work shown only demonstrated (averaged over a cell) that the effective transport properties plus that model can match performance and (averaged over the cell) crossover, and not at transport-limited operating conditions.
- The new membranes appear promising, but there is no data on durability. Overall, the experiments seem to be interesting and done well, but some more theoretical discussion of their applicability is required along with the scope of the properties. Some of the model/data comparison is better with the new transport data, but some is not as good. This project is a good use of the different systems to test the model and the various segmented cells.
- Several claimed achievements appear to be about phenomena and ideas relatively well established in the field. For example, what makes the research in slide 7 an achievement is not clear. Both the decal transfer approach and fabrication of MEAs using hydrocarbon membranes have been well known and established in literature. Similarly, the technique of hydrogen saturation and electro-osmotic drag measurements in slide 10 is not especially new. Several bullets in that slide seem to re-state the obvious. Proton conductivity data in slide 6 may have been mislabeled; the numbers for Nafion, especially at low operating temperatures, seem much too low.
- Results on water diffusivity differ from those in the General Motors (GM) project by a factor of about 3–4. The GM project claims that a decrease in diffusivity at higher lambda (as is shown in this project) is due to a "device resistance." Projects should check data with each other. An excellent match was shown between models and

experiments for all cells studied. It would be interesting for the project to report whether the outlet relative humidity (RH) for the cells in the validation efforts managed to exceed 100%. While the model-to-experiment match is impressive, there should be some analysis to understand whether wet conditions in the cells are really being achieved, or if the models are only accounting for dry conditions. Water transport numbers given for anode-to-cathode and cathode-to-anode should match. Model convergence needs to be slightly improved.

### Question 4: Collaboration and coordination with other institutions

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

- There appears to be good collaboration and coordination between the projects.
- Excellent collaboration among the members of a well-qualified, highly interactive team has benefited this project.
- This is a strong team involving organizations with complementary skills. The roles of the team members are well defined.
- This project appears well coordinated and executed. Information and results are apparently moving among the team members effectively and being utilized.
- This project has an excellent mix of partners from industry, academia, and national laboratories. There is a clear separation of tasks that requires a significant amount of exchange and interaction between partners. The progress made shows that this required interaction is managed successfully.
- With the exception of some membrane property measurements, it is difficult to see how Virginia Tech membranes are being integrated into the project, or whether they are necessary for the development of a transport model. Ballard GDLs are contributing to the measurement of GDL properties. Good use was shown for GM flow fields in model validation, as well as thin metal plates from Tech Etch. The University of South Carolina was evidently very involved with both experiments and modeling. With the possible exception of Virginia Tech, all collaborators were well integrated into the project.

### Question 5: Proposed future work

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

- The plans for the future work appear to be a reasonable continuation of the activities already underway.
- The future work aligns well with performed work and the program approach. The future work should additionally contain durability studies on either single cells or stacks at the proposed elevated operating temperatures.
- On a high level, the plans are good. Effective transport in “wet” diffusion media, and eventually moving on to short stacks and coupling thermal effects, is important for demonstrating sensitivity of performance to the measured properties in experimental hardware and in the models.
- The testing of membrane properties at lower temperatures should be done, because many fundamental aspects might change with temperature. Model validation at other conditions such as higher and lower humidity levels would be good to see. It is not clear as to whether transient modeling is going to be done.
- At the present stage of the project, future research should focus on final deliverables and concentrate on transport in stacks, as promised by the project title. Proposed work between now and the project end date in August 2013 remains focused mostly on component development and testing.
- The future work focuses on expanding the analysis to new materials. However, it may be interesting first to consider whether expanded yet realistic operating conditions might reveal inadequacies in the model. Inlet RH has been varied in the model validation exercise, but it is not clear if the model has really been run at wet conditions. Low temperature may be of interest, as well as freeze conditions. The project may also want to consider to what extent the model is capable of responding to changes in the catalyst layer parameters. Assumptions for the catalyst layer should be shown.

### Project strengths:

- This project has a good team and comprehensive approach.

- This project is a solid re-examination of phenomena and materials that, to some degree, have already been studied previously.
- This is a strong, highly organized team working well together with good publications listed.
- The measurement of membrane properties with novel fixtures and the results that show the matching of models with experimental results are strengths of this project. Most of the collaborators have been integrated to work on something useful in the project.

### Project weaknesses:

- This project has only a moderate level of innovation. There is also a disparity between work done and the stack-focused research originally promised.
- No information was shared about long-term operation at high temperatures. While the model is shared with the public, the results of the hardware development do not seem to be shared. This reduces the value for the fuel cell community.
- It is not clear as to what the objective of the project is. It could be the experimental setups, the model, the data, or the new materials; more focus may be required. It is not clear whether the key and critical properties are being measured. Some sensitivity analysis using the model is required.
- This project needs to report in greater detail the methodology behind the model treatment of water transport in GDLs. There is relatively little discussion of catalyst layers and how the model would respond to changes in catalyst layer properties (e.g., platinum loading, ionomer/catalyst ratio, etc.). This project team needs to better understand why there are differences between its measured water diffusivity and what is measured in other projects. There need to be better definitions of wet and dry conditions (beyond just what is given for inlet RH). If wet and dry conditions are varied more, it would be interesting to see if the model still matches the experiment.
- There was little publication or presentation in the open literature related to the accomplishments presented. The results need to be communicated more effectively to the community at large, particularly the membrane characterization. The modification and analysis of the diffusion media needs more inspection. For the substrates, the characterization is valuable, but there is no reason to expect a single MacMullin number relation to describe materials with vastly different microstructures and corresponding tortuosities. Some of the "papers" have significant amounts of binders and particulates. More importantly, the multilayer structures require analysis to make the effects of individual layers clear, unless a single layer is dominating. It is also not clear if the model is capable of treating the multilayer media as an assembly of distinct layers with varying properties.

### Recommendations for additions/deletions to project scope:

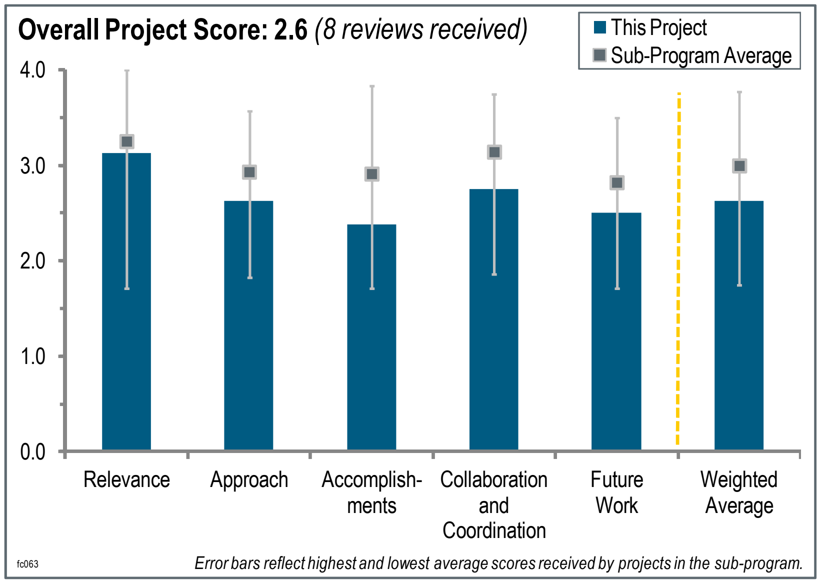
- This project should study the effect of long-term operation at high temperatures.
- The proposed work on cold start-ups should be done, as indicated in the project approach.
- Thermal conductivity of the new GDLs should be measured, along with membrane transport properties under liquid water. Capillary properties and breakthrough pressures of the novel GDLs should also be measured.
- With no connection between structure, chemistry (for the membranes), or materials and the effective properties, this is essentially a component characterization project. Incorporating some level of structure-property correlation to enable actual materials design would be valuable.
- This project should add more operating conditions at which to validate the model, particularly lower temperatures. The researchers should also report on whether thermal conductivity was considered and report on catalyst layer properties. Unless the model has already been validated at a wider range of operating conditions than those described, the project should not focus on new materials yet; the team should be sure the model works for baseline material sets.

## Project # FC-063: Novel Materials for High Efficiency Direct Methanol Fuel Cells

David Mountz; Arkema

### Brief Summary of Project:

Goals in this project are to: (1) develop ultra-thin membranes for fuel cells having low methanol crossover, high conductivity and durability, and low cost; (2) develop cathode catalysts that can operate with considerably reduced platinum (Pt) loading and improved methanol tolerance; and (3) combine the catalyst and membrane into a membrane electrode assembly (MEA) having a performance of at least 150 mW/cm$^2$ at 0.4 V and a cost of less than \$0.80/W for the membrane and cathode catalyst.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.1** for its relevance to DOE objectives.

- The project supports critical DOE objectives for direct methanol fuel cells (DMFCs).
- MEA work for portable power is in direct alignment with overall DOE objectives.
- The development of improved membranes, MEAs, and methanol-tolerant cathode catalysts are all relevant to DOE's portable power targets. The concept of a composite membrane to mitigate methanol crossover effects in a DMFC is not new. Previous attempts have had rather limited success.
- The project is relevant to the objectives of the Fuel Cell Technologies Program Multi-Year Research, Development and Demonstration Plan. The activities are aligned to DOE's goal. This project is focused on low crossover membranes and MEAs for DMFC application, which is very important for the commercialization of DMFC technology.
- Development of MEAs with better performance for liquid fuels is a critically important issue for DOE because ultimately liquid fuels will be much preferred to gaseous or liquid hydrogen ($H_2$). The work on reducing the methanol permeation is an important, although not particularly novel, approach. The catalysts development appears to have been a "bust."
- The program's overall objectives for the membrane performance and improved catalysts are good, along with the expected benefits in cost reduction and lower crossover. The project objectives provide a good link to the overall power density at the MEA level with a target loading, but the membrane objectives may be better targeted at a final MEA efficiency at the target operating point. The methanol permeability is not quantified under the operating conditions.
- The DMFC technology is an attractive one because liquid fuel is an easy way to store energy. To date, DMFC work is focused on two things: (1) making steady power from a high-energy-density methanol fuel and (2) achieving high power with lower than 2 mg/cm$^2$ of precious metal. Many have demonstrated short-term, high-DMFC activity, but the key issue with DMFCs is achieving a high steady state activity of the electrode that is measured after operating for two days and for continued operation up to or greater than 200 days. This project deserves a "3" because the authors show good relevance for some issues, but they said "lifetime" at their catalyst level was "in progress," yet this is the key issue.
- The project addresses DOE targets for performance, cost, and lifetime of portable fuel cell systems. The system targets are broken down to reasonable targets for the MEAs. Furthermore, the project addresses the important problem of methanol permeation, which is not directly related to DOE targets but indirectly influences

performance and cost. However, catalyst-specific power and platinum group metal loading are limited to the cathode, and no work is planned for the anode, though this may be equally important.

### Question 2: Approach to performing the work

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

- The technical approach used in this project is adequate and in accordance with the set objectives.
- Both the composite membrane and the cathode catalyst choices have been questionable from the beginning. So far, there has been little, if any, evidence of the performance advantage over benchmarks brought about by the materials developed in this project.
- The approaches taken for the completion of all four tasks are adequate. However, it is not clear why the team took the approach of using palladium (Pd)-based co-catalysts to suppress the methanol oxidation on the cathode of DMFCs. It is very obvious that the blending of Pd into a Pt/carbon catalyst will create a mass transport limitation in the cathode. This approach is not very prudent for suppressing methanol oxidation at the cost of higher oxygen mass transport in the cathode.
- The approach of first developing and characterizing materials individually and then combining them is reasonable. However, materials characterization is focusing on performance and disregards durability almost completely. Starting long-term testing late in the project bears the risk of missing the project's target without sufficient time remaining for counter measures.
- The approach is good for membranes but poor for the catalysts. It appears that the original catalyst results were incorrect. This may be a just reward for proceeding with a project that has little or no basis in theory beyond faith that a different metal would work better. A more theoretically based approach would have avoided this. The membrane work appears to be based on a mechanical method of reducing methanol permeation. No basis is offered for selectivity. This is a pity, because the results appear to be encouraging.
- The approach is in four parts: (1) membrane, (2) catalyst, (3) MEAs, and (4) durability testing. The authors say catalyst work is 100% complete, yet they have not demonstrated steady catalytic response in a fuel cell. As long as the anode potential is greater than 0.1 V, then one is dealing with an $H_2$ anode that then "burns off" $CO_2$ at approximately 0.4 V to $CO_2$ and not a true direct methanol to $CO_2$ electrode at 0.1 V. It appears the team expects the membrane to solve all the problems. This does not seem to be a reasonable approach. The catalyst work should be considered incomplete until membrane and system durability are better.
- The overall approach of increasing the membrane stability and mechanical properties is good. However, the approach of cross linking the sulfonic acids may be problematic. As demonstrated in Arkema's data, incomplete cross linking of the sulfonic acid containing molecules can lead to significant loss of the ionic species over the operation of the fuel cell or increased electrochemically active impurities that can adversely affect the durability. The approach of lowering the crossover seems to be a more viable approach to improve the performance, whereas the improvements in the electrochemical performance may result from methanol-tolerant catalyst problems that are likely to occur with increased venting of unused methanol and potentially flooding of the cathode. Additionally, as water management is generally an issue in DMFCs, attention should be paid to the electro-osmotic drag (EOD) of the membrane, especially as the proportion of sulfonic acid groups are increased to decrease the membrane resistance. This may lead to increased water management issues and losses of performance and efficiency in the final MEA. For portable applications, it is important to decrease the air stoichiometry to increase the system level efficiency below the stoichiometric ratio of five used in the MEA testing. It is not clear how the approach of adding silica materials to the membrane provided a benefit in the crossover or other properties of the membrane, even if good dispersion is obtained.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Eliminating part of the project that was not advancing made sense at the go/no-go review.
- The major accomplishments are two membranes and 50% lower Pt loading, which met the milestones.
- The performance is decent, but it all seems to be short term. The team needs to show good activity beyond two days of operation.

- The progress on the membrane selectivity is good from the empirical point of view, but the presenter gave no basis for the selectivity, which is a problem. The use of sol-gels has been done by Mauritz and others. The catalyst work is clearly a complete failure and the team has accomplished a "kill that" moment. A better rationale for why the Pd would work better would have been helpful, particularly because there are theoretical justifications for why Pt is better than Pd. This may serve as a warning to DOE to avoid empirical projects.
- The membranes achieved June 2011 milestones concerning area resistance and methanol permeation coefficient. The improvements to achieve the December 2012 milestone seem possible. The high sulfur loss, however, indicates poor membrane stability. MEA-performance targets are achieved with commercial catalysts; the catalyst developed in this project shows lower performance, and consequently work on the Pd catalyst development was stopped.
- The membrane optimization for the trade of the crossover and the area-specific resistance has made good progress. However, the data concerning the level of sulfonic acid group loss is concerning because this will lead to both a serious durability issue and a decrease in the membrane conductivity over time. The researchers do indicate that they understood the issues and expected to make progress toward resolving this. The MEA fabrication with the Arkema membrane appears to have made good progress in terms of power density in the lower methanol concentration. The performance seems to be in line with literature performance levels for similar membranes and electrodes. However, the data is gathered with a high-air stoichiometry (5X) on the cathode for both the perfluorosulfonic acid (PFSA) and the Arkema membrane. Additionally, Arkema does not provide any MEA data for the crossover flux ($mA/cm^2$) of their membrane compared to those they reported for the PFSA membranes at the same operating conditions. The catalyst data does not show any encouraging results in a practical MEA and appears to have been discontinued. It is not clear what benefit is expected to be gained by incorporating the silica materials into the membrane even if the dispersion issues can be resolved.
- Performance of the best membranes synthesized in this project show very little improvement over PFSA benchmarks once feed conditions are optimized for each polymer and membrane thickness. The membrane performance target should have been more demanding than the one used. Performance trade-offs between membrane resistance increase versus lowered methanol permeability should have been realized earlier. The choice of Pd-based "co-catalysts" for the cathode has been difficult to rationalize from the beginning, either from the point of view of methanol tolerance or oxygen reduction reaction (ORR) activity. Not surprisingly, the performance of MEAs with Quantum Sphere Inc. (QSI) Pd co-catalysts at the cathode has turned out to be much below that of MEAs with a "regular" Pt/C cathode. An increase in ORR performance with an increase in methanol concentration (slide 23) is stunning; it may indicate some serious problems with rotating disk electrode (RDE) experiments.
- Although the team had reported success in meeting the goals on membrane development in June 2011, the new, low-cost polyelectrolytes are highly leachable and may not be adequate to provide desired durability in DMFC conditions. The team needs to go back to the previous polyelectrolyte chemistry, which was not highly leachable, and attempt to address the membrane cost using polyelectrolyte chemistry. Moreover, the team needs to optimize the membrane composition to achieve at least similar, if not better, performance than standard PFSA membranes. The testing of Pd co-catalysts was done under extremely high air stoichs, which is inappropriate for portable DMFC conditions. The team should conduct their testing at air stoich of 2.0, which is close to that being preferred in portable DMFC systems. The team should also look into the leachability of a TPS additive. Given their small size and high acidity, which is expected to enhance their solubility in methanol water mixture, dissolution/extraction from the membrane matrix will result upon prolonged DMFC operation.

### Question 4: Collaboration and coordination with other institutions

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

- Overall, the collaboration seems reasonable.
- The project activities are well coordinated among the team members.
- Arkema is teaming with QSI and the Illinois Institute of Technology (IIT). This is a strong academic-industrial team with proven accomplishments in catalysts and membrane developments.
- Obviously, the collaboration with the catalyst developer was not good, and the only other collaboration is with IIT. The knowledge from this project is not being disseminated satisfactorily.
- The partners are working in their respective fields of expertise. Coordinated interaction or collaboration with other experts is not mentioned.

- The team consists of good partners, including a university and catalyst company. However, inclusion or consultation with a national laboratory or DMFC portable power manufacturer could have helped the team to evaluate their membrane and catalyst under realistic DMFC operational conditions.
- While collaboration between Arkema and IIT appears to have yielded some promising materials, cathode catalysts from QSI have performed much below expectations. There seems to be very little information revealed between catalyst developers and the lead organization, which might have contributed to very disappointing results.

### Question 5: Proposed future work

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

- The future work is fairly vague and appears to be more of the same empirical development.
- The proposed future work is adequate and laid in accordance with the project objectives.
- Developing MEA and testing durability is needed, but the lack of ongoing catalyst work is surprising, especially before long-term stability of this DMFC system has been determined.
- The future work described is aligned with the proposed work of the project. The team needs to consolidate on a membrane/MEA structure and then start the durability work quickly to ensure completion of the durability testing within the proposed period of the project.
- The direction of proposed research is unconvincing, especially in the MEA task. The proposed path forward does not seem novel, nor does it promise significant improvement in DMFC efficiency (the claimed objective of this project). Some work is unnecessary. For example, the task of "understanding the role of methanol crossover on performance" has been accomplished and published by now.
- The proposed future work is suitable to meet the December 2012 milestone in membrane development and the September 2012 milestone in MEA development. Work on durability is limited to testing. It remains to be seen if the proposed work to address the sulfur loss can be done without a negative effect on membrane performance, but the approach seems reasonable.
- The future plans for the membrane appear to focus on two key areas: (1) improving the membrane conductivity and lowering the crossover, and (2) increasing the durability by addressing the stability of the sulfonic acid polyelectrolytes. They do not discuss measurements of the membrane EOD, which may be an important factor in the overall performance of the MEA, especially at low-air stoichiometries. It is not clear how the silica additives will provide a benefit in the membrane selectivity for this type of membrane. It would be useful to look at the overall crossover of the MEA under operating conditions, as well as look at the methanol permeability to examine the behavior of the methanol crossover and selectivity. The MEA development objectives loom, but they may need to be tied to understanding improvements in the membrane behaviors to optimize the overall performance as well as evaluating improved electrode fabrication. As part of understanding the areas for improvement, it may be important to understand the effects of both the methanol crossover as well as the water crossover into the cathode, and the effects on the performance. Water management issues may be important to obtaining the optimum efficiency. More MEA durability measurements should be included in the future work; it was not clear this was an important part of the plans and seemed to be a weakness of the current target membranes (sulfonic acid group loss).

### Project strengths:

- This is a strong team, with all of the resources needed to solve the problems.
- The goals of the project were in alignment with important DOE barriers.
- The membrane work is very strong, and the ability to make membranes and MEAs was demonstrated well.
- The partners are experts in the fields of membrane development and catalyst development.
- The team showed a great ability to produce and test a wide variety of membrane compositions.
- This project did good, reproducible membrane development work on the base membrane. MEA development looks promising.
- The team is well organized and capable of developing DMFC membranes and MEAs. The team is equipped with the necessary resources required for the success of this project.

### Project weaknesses:

- There is a lack of preliminary indications that DMFC activity will be steady in this project.
- The approach is much too empirical. There is little justification given for why the researchers expect the approaches to work, and indeed one approach certainly did not.
- Catalyst development and characterization of catalyst-specific power are limited to the cathode; no work is planned for the anode, which may be equally important.
- The effort in this project appears disjointed, with insufficient progress achieved to date. MEA performance has been below expectations and some RDE data has been downright confusing.
- The team could have benefited from consulting with a national laboratory or DMFC company to determine adequate testing procedures for DMFCs. The team also needs to move quickly to consolidate on the final membrane and MEA structures to facilitate initiation of durability testing.
- Increasing the stability of two selected membranes by cross linking is a high-risk approach. Typically there is a trade-off between membrane resistance and cross linking by increasing the number of polymer cross links, usually resulting in the decrease of membrane conductivity. However, to achieve a performance target with a lower Pt loading, the membrane conductivity must be improved.
- The increased need of catalyst and MEA optimization is a weakness of this project. Issues with the sulfonic acid cross linking may provide a difficult approach to getting a uniform conductivity and good durability. Some data on the membrane electrosmotic drag would be useful, as would comparable measurements of the methanol crossover for PFSA membrane and the Arkema test membranes under the same conditions, preferably in an MEA at the same conditions that the electrochemical performance is measured in. This project did not have enough MEA durability measurements, especially given the highlighted issues with the approach of incorporating cross-linked polyelectrolytes and their potential leaching.

### Recommendations for additions/deletions to project scope:

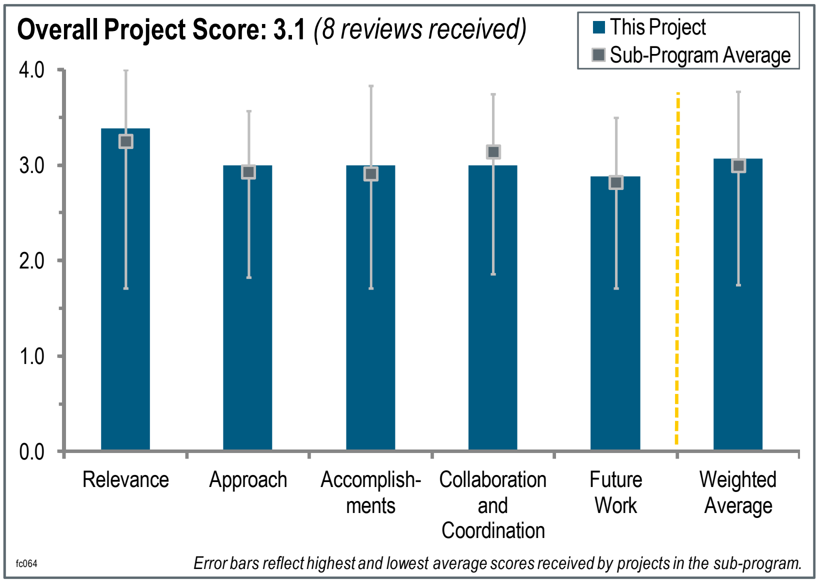
- This project needs to add some partners with some theoretical strength.
- Stopping the work on catalyst development was probably a good decision.
- It would be ideal to mitigate the catalyst agglomeration issue or find an alternative catalyst to use.
- The ongoing catalyst work should still be considered until the term stability of this DMFC system has been determined. Catalysts developed to date may have good short-term activity, but this activity may not last.
- The project needs the addition of a more intensive catalyst characterization with a clear understanding of the causes of the results obtained.
- The researchers should consider what benefits are expected from the addition of the additives (silica materials) to the membrane. They should also increase the measurement of the water crossover as part of the scope and metric to understand the water management of the MEA.
- Given the team's strength in membrane development and the time left in the project, the team should focus solely on task 1 and task 4. The team should drop task 3, specifically of the goal of 150 $mW/cm^2$ at 0.4 V. This goal seems to be a long shot, and presently there is no indication that the goal is achievable. By dropping task 3, the team will have more time to complete the other two tasks.
- Given the very poor outcome in the cathode catalyst task, there seems to be little justification for QSI's further participation in the project. To make the best possible use of the remaining resources, the focus of this project should shift to membranes with high conductivity and reduced methanol permeability. All MEA testing of performance and efficiency validation should be carried out using DMFC benchmark catalysts available commercially.

# Project # FC-064: New MEA Materials for Improved DMFC Performance, Durability, and Cost

Jim Fletcher; University of North Florida

## Brief Summary of Project:

The project objective is to increase membrane electrode assembly (MEA) functionality and internal water recovery to facilitate direct methanol fuel cell (DMFC) system simplicity, increase power and energy density, and reduce costs to address the U.S. Department of Energy's (DOE's) consumer electronics goals. This objective will be accomplished by: (1) improving the performance and durability of the University of North Florida (UNF) MEA to increase power and energy density and lower the cost; (2) developing commercial production capabilities to improve performance and lower cost; and (3) increasing catalyst stability to lower degradation rates and catalyst loadings.




## Question 1: Relevance to overall DOE objectives

This project was rated **3.4** for its relevance to DOE objectives.

- This program addresses using liquid high-energy storage in a stable power generation system with modest cost.
- This project has high relevance to DOE portable power goals, but also in water management issues and liquid-fuel MEA design, which should be the ultimate goal for DOE fuel cell technology.
- Early applications such as portable DMFCs are critical to show that fuel cell technology is real and ready. There is also a real benefit over batteries regarding high-duration energy density
- The project addresses the simplification of the water management in DMFCs, which should ultimately result in cost, weight, and volume reduction in a fuel cell system.
- This project is very relevant for portable power to address DOE's 2013 consumer electronics goals. The plan is to transfer processes to Johnson-Matthey Fuel Cells Inc. (JMFC) to improve performance and cost, which will ensure industrial relevance.
- The project is relevant to DOE's objectives because it addresses the most important limitations of state-of-the-art DMFCs: low catalyst stability and high degradation rates, and poor water management and low energy density depending on a complex water management system.
- Water management is a key issue for the implementation of portable DMFCs. This project takes on improved water management using a passive cathode approach to reduce system complexity and size associated with active cathode water recovery.
- It is very challenging to achieve a commercially viable DMFC system for consumer electronics goals because of the balance-of-plant (BOP) requirements, high catalyst loadings, and the low power density achievable with DMFCs. Overall, the team has demonstrated a terrific DMFC system; however, it is unclear whether this is a commercially viable solution for consumer electronics. Unfortunately, 300 W-hr/L will not be competitive against current consumer products. Even the 2015 goal of 800 W-hr/L seems a little low for making a significant impact toward replacing the current power solutions for the consumer electronic market. It would be nice to see how the cost is addressed in this work. For example, the current catalyst loadings and how much reduction in loading is expected to be achieved need to be defined, the cost of the MEA compared to current DMFC MEA projections needs to be projected, and the work being done on the BOP to ensure a smaller

footprint to enable a significant improvement in system energy density needs to be outlined. It is difficult to correlate the colors on the weight/volume breakdowns to the actual component; it would help if a letter were also assigned so that it is clear which pie wedge corresponds to which component.

### Question 2: Approach to performing the work

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

- This is a novel concept.
- This project uses a clever membrane and system design to maintain high system performance over 10,000 hours, which is impressive.
- This project has a good approach to increasing the specific power density by simplifying the system. Focusing on catalyst reduction late in the project bears the risk of missing the project targets.
- The approach is good in that it pulls together a number of developed components and tests the water management and liquid handling properties. The diagram of the cell is, however, unintelligible and makes no sense. It is unclear if this was deliberate.
- The addition of a liquid barrier to the cell design allows for a simplified DMFC. It also reduces the oxygen diffusion at the cathode, and thus reduces the performance of the MEA as compared to one without the liquid barrier. The performances are approaching the DOE targets, but are not there yet and the project has no specific plan (or time, since there are only a few months left on the project) to reach those targets.
- Designing direct methanol systems with a water-balanced operation through system/component engineering is an approach that has seen significant investment, including failed attempts at commercialization by at least two companies (MTI Micro Fuel Cells and Polyfuel). The likelihood that the investment of this project, focusing on minor optimization of components, will lead to a commercial or close-to-commercial device is low.
- Overall, the project follows a good engineering approach that addresses the major attributes required to advance DMFC technology. Specifically, the incorporation of the Polyfuel membrane and stable anode catalyst from JMFC combined with the barrier layer approach to simplify the system provides a very good approach with a reasonable probability of success of meeting DOE targets. The project team addressed durability and operational issues. It is not clear how the project team is addressing the integration issues for the stable anode catalyst.
- The presentation materials were well organized and presented clearly. Having the system in hand was also excellent. The critical barriers of water management, durability, and methanol crossover were the key focus, and it appears that very good progress was made. The only area that did not seem to progress well was the anode catalyst development at Northeastern University (NEU); it was not clear what the critical issues were and how the team was going to address them in their "optimization" work. It would be helpful to see some electrochemical impedance spectroscopy of the NEU MEAs. Because the partnership of JMFC more than makes up for this aspect of the program, it is still fair to give a rating of 4 rather than a 3.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has great results going against difficult metrics.
- The optimization of individual components, such as the catalyst layer and membrane, has led to improvements in fuel cell performance.
- Achieving a commercially viable DMFC system for consumer electronics is very challenging. While the progress is excellent, it is not clear if all the barriers (specifically the BOP and stack weight) can be overcome such that 800 W-hr/L will be demonstrated.
- The progress made in the project to reduce off-state degradation and improve catalyst durability is good advances. The use of a thicker membrane is rather obvious and seems like it should have been used earlier. The performance improvements relative to DOE's targets has been modest.
- Achieving 10,000 hours of continuous DMFC operation in a portable package (not a laboratory bench test) is very impressive. The reason this is not "outstanding" is that the issue of deactivation when the system is "off" is now an issue. Short circuit deactivation is an issue. These issues can be dealt with as is indicated in

slide 16. What is impressive is the team has overcome the Achilles heel of DMFCs (the catalyst) by clever membrane and system development.

- Excellent progress has been made on the water management but progress has not been so great on understanding the behavior of the electrode layers, as demonstrated by the poor performance of the NEU catalyst. There is no evidence given for poor conductivity in the catalyst layer. This needs to be probed since this will be necessary for future work. The variability in the catalyst's performance indicates there is a long way to go in understanding how the catalyst layer works.
- The team has produced an MEA capable of supporting a small system, close to meeting 2013 targets. The researchers have achieved 20% improvement in MEA performance and 10,000 hour durability demonstrated, with some operational issues identified and mitigated. They have also achieved a significantly lower crossover for a given cell resistance with their UNF HC membrane, and they can get the same performance with a 45 µm membrane, but with significantly reduced methanol crossover. Fuel efficiencies of >90%, and overall efficiency of approximately 30% were achieved.
- Good progress has been made in membrane optimization regarding methanol crossover. Also, the degradation rate in discontinuous operation was improved, but the cumulative (real operation) time should be increased for the long-term experiments. A degradation rate of 34 µV/hr seems to be not good enough to meet DOE's targets, which are 3,000 hours or 5,000 hours for 2015. The degradation rate was measured in an on-off mode only with 200 hours of real operation.

### Question 4: Collaboration and coordination with other institutions

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

- This project has good collaborations with JMFC and NEU.
- Collaboration with JMFC has resulted in improved MEA performance and scalability of the process.
- The UNF/NEU/JMFC team is very strong and they are using their strengths to effect outstanding results.
- The project has a reasonable level of collaboration, including JMFC to ensure industrial practicality and promote commercialization.
- There is a good cooperation with JMFC and NEU. The partners are working on their respective fields of expertise.
- The work suffers heavily from the lack of involvement with a systems integrator, a role that is being attempted by a university and leaves questions about whom ultimately would manufacture these devices should the project be successful.
- The NEU contribution appears disconnected. The technical transfer to JMFC is good, but then this appears to limit the extent of the collaboration possible. This seems to be a pity, because there is a big problem to overcome in understanding the catalyst layer operation that more collaboration might help with.

### Question 5: Proposed future work

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

- The future work is good as far as it goes, but it needs a more fundamental approach to catalyst layer design and development.
- The proposed work includes a reduction of catalyst loading, stack testing, technical transfer, and anode development, all of which are laudable next steps.
- The future work is just a continuation of what has been done so far, which is appropriate considering the amount of time left in the project.
- The proposed work seems suitable to meet the targets in the remaining time, except the catalyst loading reduction and the improvement of the MEA with NEU catalyst.
- The project is almost complete. The technology transfer to JMFC and continued integration work is appropriate. It is not clear if issues with performance for stable catalysts will be overcome within the time frame, though.
- It is not clear how much NEU contributes to the success of this program. The technical plan for optimizing and improving the performance of the NEU catalyst was not presented and therefore cannot be evaluated properly. The researchers should partner with a group that can help reduce the BOP size and weight.

- The future work is somewhat disconnected with very little synergy between the tasks of each participant and too broad to lead to substantial advances in any specific area. MEA development seems to be the primary need so that performance capable of meeting out-year targets can be approached. The stack testing should be limited; it is unclear who the consumer of commercial MEAs would be, or if they should be a focus at this time, and the value of NEU's catalysis work has not been demonstrated and does not seem to be as important as the focus on barrier layers.

### Project strengths:

- This project has a strong team.
- This project has good focus on and success with the water management issues.
- The team has demonstrated systems and long-term durability of DMFCs.
- This project provides a new approach to the simplification of the BOP components.
- This project takes a practical engineering approach with strong linkages with an industrial partner to enable commercialization.
- This project has made excellent progress toward showing the benefits of fuel cells over batteries. The researchers have achieved or exceeded most DOE metrics. This is the only company at the DOE Hydrogen and Fuel Cells Program Annual Merit Review with a working fuel cell system.
- MEA, stack testing, system operation profiles, and the partnership with JMFC are the strengths of this project. The fact that the researchers have demonstrated a nice system with excellent durability is another strength.
- The project idea to use a water barrier layer to minimize the system components is an interesting concept. The project partners have great experience and expertise in their respective fields, especially in catalyst development.

### Project weaknesses:

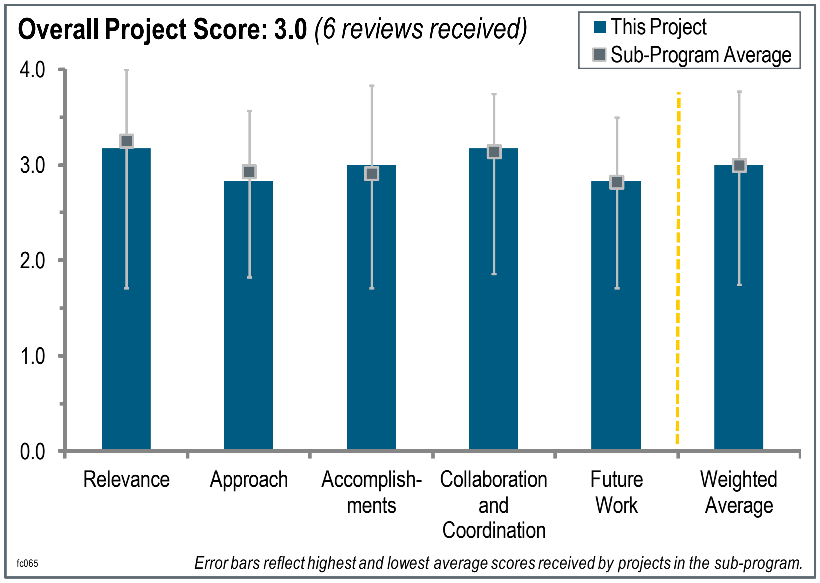
- This project has no weaknesses.
- The work at NEU appears weak, and the lack of a BOP partner seems to be a weakness.
- The diagram of the water management system is confusing. The approach to the catalyst layer design is too empirical.
- The MEA integration issue is not understood and the approach to understanding this is not outlined.
- The project is more procedural oriented. To solve the degradation problems, a more scientific approach would be helpful. It is unclear if the development will help to commercialize this kind of DMFC system with a passive water recovery system.
- The largest weakness of the project is the lack of a system integrator/manufacturer. It leaves major concerns for what the path forward is for findings from the project, assuming a high level of success. An additional concern involves how far the temperature can be pushed and whether or not current thermal gradients in the system will prevent higher temperature and therefore more-efficient, higher-power-density operation.
- At temperatures greater than 50°C–60°C, water will be lost through vapor diffusion through the liquid barrier. This has two primary consequences: (1) limiting the operating temperature/tolerance to temperature upsets, and (2) not eliminating water storage and pump, but only reducing their sizes, resulting in a lower power density than claimed today.

### Recommendations for additions/deletions to project scope:

- This project should add more collaborators to help develop a basis for catalyst layer design.
- The project team should focus on the optimization of the system operating parameters and the conditions during on/off. Mechanically “sealing” the cathode is one promising approach.
- Considering that the methanol cross-over is a minor component in DMFC performance losses at the fuel cell operating methanol concentration, it would be interesting to have a perfluorosulfonic-acid-based MEA for comparison of performance.
- Focus on catalysis will only result in modest improvements in performance and work by others, including Los Alamos National Laboratory’s project on DMFCs, which has made advances that this team could apply without using resources. The team should focus just on optimization of water management and increasing temperature of operation while maintaining water balance.

## Project # FC-065: The Effect of Airborne Contaminants on Fuel Cell Performance and Durability

Jean St-Pierre; Hawaii Natural Energy Institute




### Brief Summary of Project:

The objective of this project is to identify and mitigate airborne contaminants that adversely impact system performance and the durability of fuel cells. The project includes contaminant studies, real-world operation and mitigation strategies, model development and application, and outreach. The Hawai'i Natural Energy Institute (HNEI) will identify the contaminants contributing to losses in fuel cell tests and will include in situ and ex situ tests to determine the loss mechanisms.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- Studying the effect of airborne impurities on fuel cell performance is important.
- Airborne impurities have significant impact on fuel cell performance, especially on the cathode, making this project highly relevant to the objectives of the DOE Fuel Cell Technologies Program.
- This type of understanding is absolutely critical to meeting all the fuel cell system, membrane electrode assembly (MEA), and MEA component targets simultaneously in actual commercialization.
- This project is quite relevant because it continues to be important to understand the effect of air contaminants on fuel cell performance and durability. It is also relevant to develop or design mitigation strategies.
- Impurity testing is quite important for characterizing possible fuel cell performance degradation mechanisms. A comprehensive analysis of impurity effects is needed to prevent failure of early fuel cell demonstrations. A number of early adopter projects have already been impacted by a failure to understand the role of contaminants. These include premature failures in high ammonia and sulfur gas environments, fuel cell degradation due to system contaminants, and inadequate feedwater quality.
- Studying the impact of airborne contaminants and developing mechanisms is critical to extending the life of the fuel cell system in real-world applications. The concentrations used to study the impact of the contaminants on fuel cell performance works well for screening purposes. However, the use of high concentrations could lead to false "positives," hence it is recommended to crosscheck the impact at realistic concentrations for a few select contaminants.

### Question 2: Approach to performing the work

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

- The approach is somewhat empirical; however, it does identify potential contaminants.
- The approach is good and straightforward and includes identification, contaminant studies, and modeling. It seems that the contaminant studies will not continue after the third year.
- It is not clear if the approach misses, for example, environmental impurities that are entrained in the inlet air streams, not as gases, but as micro-particles such as road salt in the upper Midwest.

- The approach is sound, but more work is needed to establish allowable concentrations into a fuel cell system to minimize voltage loss. Further, the interactions between contaminants are not being currently addressed; any potential negative interactions need to be identified.
- The effects of impurities on, for example, catalyst loading, should have been explored not only to reveal the mechanisms, but because operation at low loadings can often show exaggerated effects. The relationship between multiple exposures' reversible decay and non-recoverable performance should be explored in at least one of the 1st tier impurities.
- The approach used in the project is clear and is likely to be effective. While somewhat involved, the selection criteria for the 1st and 2nd tier contaminants have already proven to be quite effective. Avoiding duplication of other contamination-related projects is a plus. The wording of go/no-go decisions (slide 7) is confusing, i.e., more appropriate for milestones rather than decision points. It is not clear what the go/no-go decision points really are.
- The approach is fundamentally flawed, which is odd, given the strong team. Both UTC Power and Ballard have extensive experience operating bus fleets in various parts of the world, including the United States, Canada, Germany, and China. In addition, UTC Power has extensive experience with phosphoric acid fuel cells for stationary applications. It is not clear why the contaminants were not selected based on the knowledge base of these two partners.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The researchers continue to make good progress in evaluating the Tier 1 and Tier 2 potential contaminants.
- Excellent progress was made on screening the contaminants using the two down-selection criteria and testing those in a fuel cell.
- HNEI has made good progress to date and has a good publication record. The researchers need to provide more details on which performance model is being used and at what level the impurities are being dealt with. It is unclear if surface area measurements (electrochemical surface area obtained by CVs) are being made to assess the effect of each of these contaminants.
- This project appears to be meeting its targets. In some cases, more in-depth understanding of the causes of the observed performance losses would be welcome. Development of predictive models for various contaminants is needed. This project would benefit from earlier development of mitigation strategies.
- HNEI has tested 19 Tier 1 contaminants and four Tier 2 contaminants. However, much of the data analysis appears lacking. The conclusions: "Higher concentrations are worse," "Higher current densities are worse," and "Lower temperatures are worse" could have been made before testing. The voltage recovery work lacks analysis. Knowing which contaminant can be recovered from is important, but understanding why recovery from some contaminants is possible but not others is lacking.
- The results generated are no doubt very reliable and the models that result will be useful. It is not clear that the data are sufficiently large to make the models as reliable as they could be. Also, long-term exposure to any one impurity at low levels, repeated exposure to higher levels of any one impurity, or long-term exposure to multiple impurities could manifest in different long-term decay rates that in turn are dependent on the type of catalyst alloy or loading in use. Some preliminary investigation of these effects with key impurities would be good to see if further funded research in that direction is warranted.

### Question 4: Collaboration and coordination with other institutions

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

- The established collaborations are working well.
- The workers are collaborating effectively with many partners.
- The collaborations appear good and valuable, with actual data and designs being transferred.
- It is not clear what, if any, work was performed under this project by UTC Power and Ballard. The work in slide 13 looks like it was performed by C2E2. The other interactions with General Motors and Nuvera are good.

- This is a well-integrated project involving strong industrial partners that should ensure sufficient validation of performance loss data and the use of relevant test protocols. Participation with organizations other than partners in the project has been very good.

### Question 5: Proposed future work

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

- Planned work on metal plates and contaminants should be coordinated with this work.
- It is unclear when the segmented cell work is going to be performed.
- The future work plan is good and well thought through. There should be gradually more emphasis on mitigation strategies or a means of preventing performance loss that cannot be reversed.
- The proposed work to understand the interactions between contaminants is critical. Further, the impact of contaminants at lower loading of platinum (Pt) catalysts is needed. Work to establish a tolerance limit for the contaminants is needed.
- The future work proposed could be better focused by continuing to build on the more basic investigations of impurity type effects on MEA performance and performance decay models. The impact of non-reversible decay from repeated exposures/recoveries should be explored because that is what will happen in the real world, and models based on short-term exposure/recovery will not anticipate longer-term effects. Other researchers have seen that after repeated recovery steps, non-recoverable permanent damage can be done to the catalyst surface structure, which is what will determine the long-term durability. Finally, it all gets worse at lower catalyst loadings. So at the minimum, the types of tests being done with the Tier 1 and 2 impurities should be done on state-of-the-art PtCo/C alloy catalyst electrodes at the DOE target loadings of $<0.125$ mg/cm$^2$.

### Project strengths:

- This project has good techniques, collaborations, and methodology.
- This project has many collaborators to screen families of potential contaminants, and the work progresses on schedule.
- HNEI and the principal investigator are performing extensive experimental work and publishing results for use by the fuel cell community.
- This is a well-executed project that stands a good chance of determining the impact of some common contaminants on cathode performance.

### Project weaknesses:

- The data are treated with a somewhat empirical approach rather than a more chemical interaction type of modeling.
- This project needs to better explain the overall objective of the project and provide more details on model development.
- Most of the analysis presented is qualitative or semi-quantitative. More value would be added if the individual mechanisms of the contaminants are understood.
- Limiting the scope of the impact of impurities with just a single functional impact mechanism that is independent of catalyst type, loading, or prior exposure history is a weakness.
- A certain weakness of the approach used in this project is the disconnect from the very rich literature database on strongly adsorbing unsaturated and aromatic hydrocarbons, many of which have been the focus of this research. Literature can be of help in determining both the mechanism of the electrode performance loss and mitigation strategies (if at all available). Increasing the understanding of the contamination process for different pollutants should be accelerated.

### Recommendations for additions/deletions to project scope:

- This project should specify tolerance limits and contaminant impacts at automative-relevant Pt group metal catalyst loadings.
- This project should include more in terms of understanding the fundamental degradation mechanisms at work.

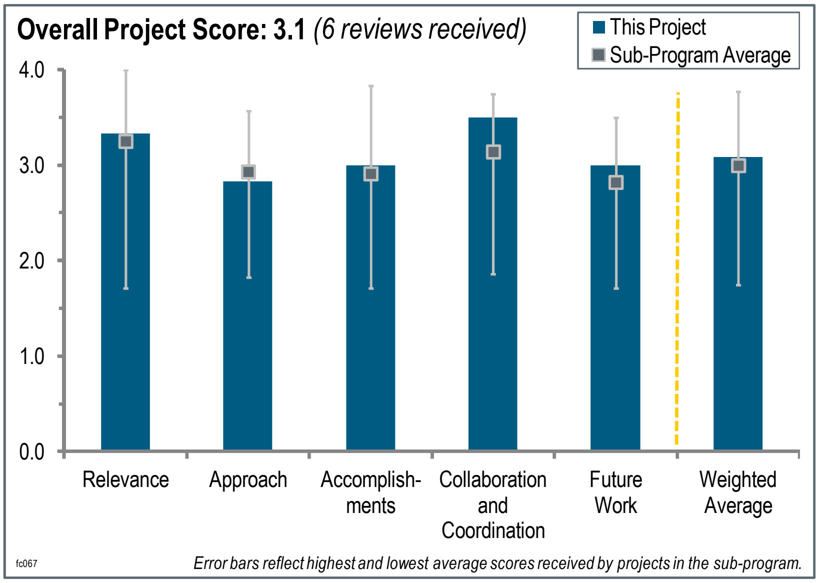
- More chemical/electrochemical diagnostic work to identify the mechanisms of contaminant impact on the performance is needed.
- This project scope should include a clear statement as to whether performance recovery is possible or whether filtering is the only option. Such a recommendation should include concentration limits relevant to either approach.
- HNEI should add a focused task to look at the long-term effects of repeated exposure and recovery steps, using one of their worst offending impurities on an MEA with a cathode that has a loading of only 0.1 $mg/cm^2$ and a very thin (approximately 10 µm) membrane.
- Bin impurities based on: reversible catalyst contaminants, irreversible catalyst contaminants, membrane contaminants, contaminants contributing to other performance loss, and contaminants with no deleterious effects.

## Project # FC-067: Materials and Modules for Low-Cost, High-Performance Fuel Cell Humidifiers

Will Johnson; W.L. Gore

### Brief Summary of Project:

This project seeks to develop a durable, high-performance water transport membrane and a compact, low-cost, membrane-based module utilizing that membrane for use in automotive, stationary, and/or portable fuel cell water transport exchangers. More-efficient, low-cost humidifiers can increase fuel cell inlet humidity, which reduces system cost and size of balance of plant (BOP), improves fuel cell performance, allows for reduced fuel cell stack size, and improves cell durability.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- Successful development of this humidifier would help systems meet overall DOE goals.
- Humidifiers are important for meeting high-temperature automotive targets, although they are ancillary to specifically quantified targets in the technical plan.
- Durable, efficient, and low-cost humidifiers are critical to optimizing both thermal management systems and fuel cell stacks. This project addresses critically important objectives for fuel cell commercialization.
- For the automotive application, the continued development of a membrane humidifier is key. The case was not presented for the level of need in the stationary or portable application.
- The project supports DOE's research and development objectives, in particular for the BOP, by developing more-efficient, low-cost humidifiers. This will lead to reducing system cost and size of BOP, improving fuel cell performance, potentially decreasing the size of fuel cell stacks by running under wetter conditions, and improving fuel cell durability.
- This work is extremely relevant to the DOE Hydrogen and Fuel Cells Program (the Program). However, this type of work is expected to have occurred without DOE funding because it is a core-competency effort of the original equipment manufacturer (OEM). The benefit to the Program is that it sheds light on humidifier technologies. It may be more beneficial to benchmark humidifier technologies and publish findings than to help individual companies' humidifiers. By benchmarking humidifier performances, competition will be spurred and both automotive OEMs and humidifier manufacturers will benefit. OEMs will have a clear picture of product availability, and humidifier manufacturers will have a better idea of areas they need to improve to stay competitive.

### Question 2: Approach to performing the work

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

- Technically, this approach is very good.
- Stability issues show that a broader range of materials should be considered before the down-select step.
- The combination of using a custom membrane material well suited for the application with a simple and cost-effective module design is a good approach.

- The approach is consistent with the project aims, including starting with material improvement of the new Gore membranes optimized for high performance and low cost and then design and optimizing the humidifier design with the subcontractor, dPoint.
- The focus, to date, has been on materials evaluation. The project needs more emphasis on the module (underway) and the integration into the system to better understand the effects of the edges of the operating envelope, start-up/shutdown, etc. For example, earlier testing would provide feedback on how best to test the membrane.
- The goal of developing a low-cost, passive humidifier module is good. The campaign to understand performance and durability of such a module is also good. The breadth of the program is a bit disappointing because it focuses on one specific proprietary material and one specific proprietary module design. This limits the program value for the industry.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project is proceeding through its targets.
- Good progress has been made so far. The work put into identifying the degradation mechanism caused by the formation of anhydride species reducing the accessible number of $SO_3$ sites was appreciated.
- The data provided were helpful in understanding the performance and challenges of this humidifier. The team seems to understand the importance of durability and the criticality of evaluating the impact of environmental contaminants. The fundamental information on degradation mechanisms was good.
- The Gore material has shown promise, but there are significant issues with degradation. The presenter appeared to downplay a potentially fatal issue as shown in the data from General Motors (GM). The laboratory testing does not appear to simulate actual conditions; for example, the water quality that will be achieved in an operating engine.
- It is not clear that there is a viable path to overcoming the limitations of the current material. There is also a concern about the cross-flow design of the module. The claimed 96% effectiveness is physically impossible except in counter-flow designs.
- This project has made excellent progress toward objectives with main accomplishments: understanding of the source of durability loss—chemical changes in PFSA (as shown with the GM experiment) in accordance with the literature. It appears reversible with acid heat treatment, but this will not be possible to do in a system. There is also the risk of contamination (e.g., from sodium, magnesium, and potassium). The membrane acts as a filter and may protect the fuel cell stack if there is no other filter in the system. The module performance is consistent with single cell, and ex situ testing shows a loss of performance at 80°C of 20%–30% over 5,500 hours. The sub-scale module design is complete and the sub-scale prototypes are built—some first results would have been appreciated. Module cost is estimated to be approximately $100 at high volumes, achieving DOE targets. It appears two times the cost of tubular humidifiers.

### Question 4: Collaboration and coordination with other institutions

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

- This project has an appropriate mix of collaborators.
- The company is working with automotive OEMs and is acting on feedback for device specifications.
- The prime contracting team is a little thin, but outside testing by automotive OEMs and modeling by Argonne National Laboratory (ANL) brings an important balance to the project.
- There is a good amount of collaboration for a project of this size. The collaborator dPoint seems like a good choice for module development and testing.
- It was very good to include the end user in testing. The module development should have begun earlier, even if the material had not yet been optimized.
- This project has appropriate collaboration with a product integrator (dPoint), with automotive OEMs (in particular GM), and ANL (to have a link with the system modeling of Gore membranes in humidifier modules).

### Question 5: Proposed future work

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

- The future work is adequate for this project.
- This is a reasonable work plan, given the current barriers.
- This is a good plan. The results of the durability testing and the final full-scale module are eagerly anticipated.
- There is no opportunity to feed back the data from module testing (or module integration into a working fuel cell system) into the membrane development.
- The proposed work is in accordance with the project goals and the time remaining for the project. It will allow for complete membrane durability testing, the scale-up membrane manufacturing for final module build, finished sub-scale validation testing, and a final full-scale module.
- Given the specific nature of the materials and module design being developed with funding from this program, more effort to distill learning more broadly useful to the industry at large would be valuable. More targeted poisoning studies with likely environmental contaminants would be valuable.

### Project strengths:

- The Gore material has advantageous performance parameters.
- There is a good balance of partners and collaborators, a good focus on performance and durability, and a good exploration and explanation of fundamental mechanisms.
- The combination of using a custom membrane material well suited for the application with a simple and cost-effective module design is a good approach.
- The objective is still worthwhile and the overall approach has been reasonably well structured. The materials expertise brought to bear on the problem is impressive.
- This project has many strengths. The approach of starting from an existing membrane material, which has been improved during the project, was appropriate. The collaboration from the beginning with an integrator was relevant. It has been appreciated to investigate the degradation issue and to understand the mechanisms; it may lead to future improvements.

### Project weaknesses:

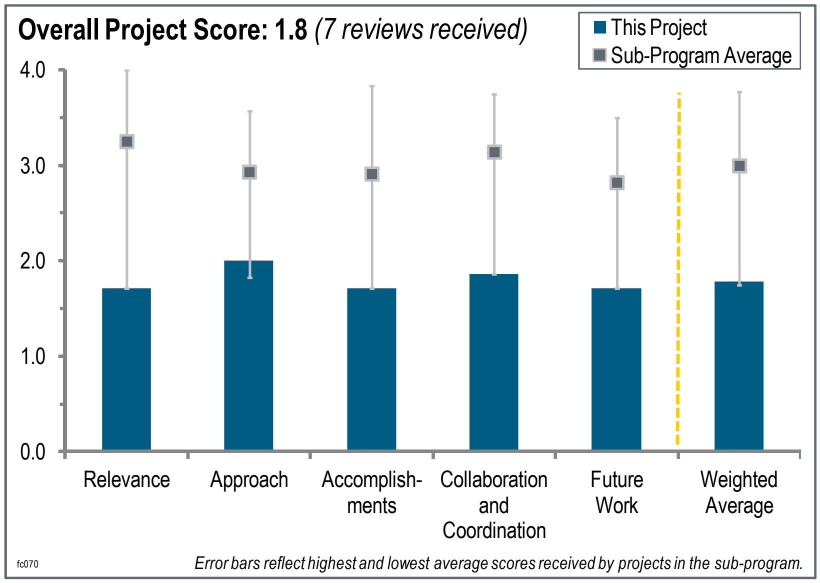
- The module design should have been better reported at this final stage.
- There is not enough emphasis on meeting all of the application requirements, including solving the degradation issue. This project needs better feedback of module and system testing into the material development process.
- The commitment to a narrow family of materials at an early stage seems to have boxed in and limited the project. It is also unclear how well designed the module is.

### Recommendations for additions/deletions to project scope:

- This project is near completion; no changes are recommended.
- There are no recommendations this near to the completion point in the project.
- Some specific and targeted testing of the impact of environmental contaminants on durability and performance, and perhaps some collaboration with projects FC-048 and FC-065 would be useful.
- There are no particular recommendations, because the project is at its final stage and the results are in accordance with the aims. However, it would be interesting to investigate the behavior at higher temperatures such as up to 95°C, as some automotive systems are rising to such temperatures.

## Project # FC-070: Development of Kilowatt-Scale Coal Fuel Cell Technology

Steven Chuang; University of Akron




### Brief Summary of Project:

The overall goal of this project is to develop a kilowatt-scale coal fuel cell technology. The results of research and development (R&D) efforts will provide the technological basis for developing megawatt-scale coal fuel cell technology. For fiscal year 2012, the project objectives were to test the effect of operating conditions (temperature, voltage load, and concentration of CO, $CO_2$, and $H_2O$) on the performance and energy efficiency of the coal fuel cell, and to investigate the integration of coal fuel cells in series and parallel stack configurations.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **1.7** for its relevance to DOE objectives.

- It was not clear from the poster how the work was addressing DOE Hydrogen and Fuel Cells Program (the Program) objectives.
- The project is focused on the use of a fossil fuel, and thus not well aligned to DOE's Office of Energy Efficiency and Renewable Energy (EERE) objectives.
- Although this project supports DOE research, development, and demonstration objectives, it is not clear why it is part of the Program.
- The results of this R&D effort will provide the technological basis for developing megawatt-scale coal fuel cell technology.
- This project does not bring new or unique information to the fuel cell community. Fuel cell operation on coconut coke/Petcoke has little value and apparently fuel cell operation on coal gasification products was never done.
- The project is a poor fit for the EERE Fuel Cell Technologies Program. Coal-fired generating systems are being considered for very large installations where the economics are more favorable. Coal and other solids handling equipment are expensive per unit material handled in the smaller sizes.

### Question 2: Approach to performing the work

This project was rated **2.0** for its approach.

- The approach is good. The researchers are working on increasing anode performance and understanding carbon reactivity.
- Although the project had overly ambitious objectives, the approach is reasonable. The approach is laid out to address the barriers.
- The project should focus on addressing key technical issues, such as performance and performance degradation, cell component stability, lifetime, etc. for direct operation of solid oxide fuel cells (SOFCs) on coal.
- The primary objective of the project was to demonstrate and characterize fuel cell operation on coal at the kilowatt-output level. Apparently this was never done and easier-to-use substitutes for coal were used instead.

- There was no obvious connection between the work and the technical barriers it claimed to be addressing. From the chemical analysis performed, it appears that coconut-based coke is not chemically similar enough to real coal or coke to be an appropriate substitute for laboratory experiments.
- The approach is unfocused and cannot lead to long-term evaluations of electrode activity, because the project is nearly over. The approach appears to avoid any activity that would lead to progress in feeding and handling coal in an SOFC. It is not clear how the project will lead to megawatt-scale fuel cells that operate on coal. Handling of ash constituents, including contaminants, which is usually the limiting factor in long-term durability in coal-fired power plants, has not been addressed.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This progress has made some accomplishments, but no breakthroughs.
- The progress appears far from achieving the objectives of developing kilowatt-scale coal-fueled SOFCs.
- Little progress has been made toward the original goals and objectives of the project.
- Poor performance has been demonstrated. The cells show 12% degradation after only 100 hours (even when operated on methane instead of coal). Current densities are far too low to make an economic system. While the objective of the project was to develop kilowatt-scale fuel cells, the project was limited to fuel cells with only tens of milliwatts. Too much of the work has been devoted to exploring SOFC materials composition and tape casting methods, which duplicates work done elsewhere in the Solid State Energy Conversion Alliance (SECA) program.
- The accomplishments appear modest. The performance of coconut coke and Petcoke is not very good. Performance of coal would likely be worse due to the release of numerous volatile contaminants, including sulfur. The performance of the unit cell needs to be improved before any thought of scale-up should be considered. Performance degraded more than 10% after about 100 hours when the cell was fueled with methane. Performance can be expected to degrade even more when coal is used.
- It is hard to believe that all five tasks are equally at 90% complete. The results are obtuse, making it difficult to assess the outcome of the project or their relevance to the overall project or to the Program needs. The plots on slide 7 provide an example of this. Although the label under the left-hand plot asserts increases in methane, carbon monoxide, and carbon dioxide, the plot is labeled with "m/e." Thus, the reader must first assume that the molecules only lose one electron in the mass spectrometer and must then calculate molecular weights while also trying to interpret the graphs. Although the slide asserts, "water could be a good substitution for hydrogen in coal-based fuel cells due to its similar performance and higher rate of coke gasification reactions," the right-hand plot on slide 7 actually supports a 15%–25% decrease in current density, and thus, performance.

### Question 4: Collaboration and coordination with other institutions

This project was rated **1.9** for its collaboration and coordination.

- The project partners' engagement with the project was not made clear.
- There was little apparent technical interaction with the collaborators on this project.
- The collaboration was with the Ohio Coal Development Office, FirstEnergy, and Coal Fuel Cell.
- The financial support of the collaborators is good, but there should be more collaboration with other developers of SOFC technology from the SECA program to help solve some of the anode/cathode/electrolyte composition and fabrications problems.
- There is evidence of collaboration with an end user, but it is not clear what role they played in the project. Collaboration with a major fuel cell developer would have been much more helpful in determining the direction of the research.
- This project has good collaboration with the Ohio Coal Development Office (state), which focuses on the fundamental research for the determination of the fuel cell efficiency, and with FirstEnergy Corp (industry), which addresses practical issues of the fuel cell stack scale-up.

## Question 5: Proposed future work

This project was rated **1.7** for its proposed future work.

- This project is essentially complete and no further support for this project is recommended.
- The principal investigator has a clear vision for the next steps within the project, but, with the exception of durability testing, the steps still do not appear to address the stated barriers.
- Any additional work should focus on coal injection and fly ash removal (the unique aspects of the project), and defer trying to scale up to kilowatt size until better performance is demonstrated.
- This project needs to focus on addressing key technical issues of fuel cell operation on coal. It is too early to evaluate coal injection and fly ash subsystem and design, or fabrication and testing of a kilowatt-scale demonstration system.
- The proposed future work needs to be completed to demonstrate the feasibility of this approach. However, the project is nearly over with not much to show for the effort.
- The proposed work should advance development by further testing the coal injection and fly ash removal units, demonstrate the long-term performance and durability of the fuel cell stack in series and parallel configuration, and further test a small-scale (<10 kW) coke/coal fuel cell system.

## Project strengths:

- The researchers have an excellent understanding of the barriers.
- The direct coal utilization fuel cell concept is a strength.
- This project has good leverage in using funding obtained from the State of Ohio (Ohio Coal Development Office).
- Using coal directly in an SOFC offers the potential to facilitate carbon capture from what is otherwise a fuel burdened with significant greenhouse gas potential. It looks like the project team has made some modest steps toward that goal.

## Project weaknesses:

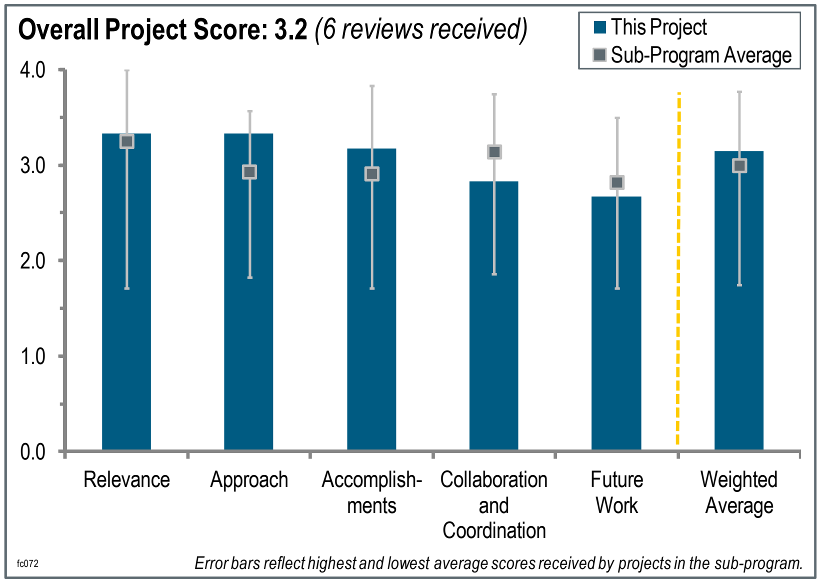
- This project is a very difficult, long-term R&D effort.
- The fuel cell performance presented to date does not justify further investment in this pathway.
- There is a lack of focus and expertise/experience on SOFC technology. Collaboration with SOFC developers and SOFC experts is needed and the metrics/targets are not defined.
- The primary objective of this project, fuel cell operation of coal gasification products, was never done or attempted. Apparently, because of impurities the coal has streamed, cleaner but meaningless substitutes were used.
- The project lacks relevance to the EERE fuel cell development effort. Only a small amount of progress is evident over the course of the project. Expectations were too ambitious and the team lacked a major SOFC developer to guide the research in fruitful directions.
- Poor performance has been demonstrated. Cells show 12% degradation after only 100 hours (even when operated on methane instead of coal). Current densities are far too low to make an economic system. While the objective of the project was to develop kilowatt-scale fuel cells, the project was limited to fuel cells with only tens of milliwatts. Too much of the work has been devoted to exploring SOFC materials composition and tape casting methods that duplicate work done in the SECA program in the Office of Fossil Energy.

## Recommendations for additions/deletions to project scope:

- The project team needs to indicate, and preferably demonstrate, how a direct-coal fuel cell can provide performance that would make an industry-scale system worth investing in. It looks like this program is a long way away from that goal, and it is not clear how one would get there from here.
- The project completion date is May 31, 2012, and almost all the funds have been spent so there is little opportunity to change project scope. Nonetheless, any additional work should focus on coal injection and fly ash removal (the unique aspects of the project), and the team should defer trying to scale up to kilowatt size until better performance is demonstrated. Too much of the work has been devoted to exploring SOFC materials composition and tape casting methods that duplicate work done elsewhere in the SECA program.
- Considering the technical status and the progress to date, the researchers should delete tasks 4, 5, and 6.

## Project # FC-072: Extended Durability Testing of an External Fuel Processor for SOFC

Mark Perna; Rolls-Royce Fuel Cell Systems (US) Inc.




### Brief Summary of Project:

The overall objectives of this project are to: (1) conduct long-term tests in relevant environments for the three fuel processor subsystems that support operation of the 1 MWe solid oxide fuel cell (SOFC) power plant, including synthesis gas, start gas, and desulfurizer; (2) determine long-term performance of catalysts, sorbents, heat exchangers, control valves, reactors, piping, and insulation; (3) evaluate the impact of ambient temperatures on performance and component reliability; and (4) determine system response for transient operation. Project objectives from May 2011–2012 include the following: (1) complete desulfurizer subsystems testing in an outdoor facility; (2) evaluate sorbent vessel construction materials for the desulfurizer and reactor components for synthesis gas; and (3) perform start-gas subsystem testing.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- This project fully supports DOE's Hydrogen and Fuel Cells Program (the Program) and research, development, and demonstration objectives.
- Fuel processing for stationary fuel cells is directly relevant to DOE's objectives. The focus on SOFC is narrow.
- The current project investigates the stability of SOFC under trace sulfur contaminants. Fossil fuel contains sulfur-related impurities. It is hard to reduce the $H_2S$ level to less than 3 ppb. The current project is critical for SOFCs to operate under such impurities, especially for a long term.
- This project supports the Program through the development of fuel processing subsystems for a 1 MW SOFC for distributed generation. Specifically, in fiscal year 2012 the work involved an evaluation of the durability and performance of two critical balance-of-plant components: a catalytic partial oxidation (CPOX) fuel processor and a desulfurizer for a pipeline natural gas feed. Both the overall system and key components are being investigated.
- In this project, an external fuel processor is being developed and tested for potential use with a MW-scale SOFC operated on pipeline natural gas and, later, on alternative fuels, such as biogas. Stationary fuel cell systems for power generation or combined heat and power generation are expected to provide opportunities for early market entry of fuel cells. Successful demonstrations of natural gas reforming for this application will help to support the commercialization of SOFC technologies.

### Question 2: Approach to performing the work

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

- This project should focus on testing under realistic conditions.
- The concept eliminates the need for on-site hydrogen storage. A process block diagram would have helped with understanding the three parts of the system.

- The start-up cycles are targeted to the temperature fluctuations, which accelerate the sulfur poison. This acceleration test method reveals the $H_2S$ poison mechanism in cooperation with post-test analysis.
- The approach is sound. The subsystems have been developed over many years and have now been tested over significantly long time periods (the desulfurizer for 8,000 hours). Physical and chemical signs of degradation for key components have been/are being evaluated. All of this is necessary prior to a full-up power plant demonstration in the future. This will occur at some future date when the fuel cell stack becomes available; this is projected to be in 2013.
- The external fuel processor has been configured as a combination of three subsystems, each of which can be designed and tested independently under operating conditions appropriate for each. The synthesis-gas subsystem (for normal operation) is based on a CPOX, and it will be tested for up to 1,200 hours, including 10 start-up/shutdown cycles, and at operating loads of 10%, 50%, and 100% of design flows. The start gas is a non-flammable or weakly flammable reducing gas, primarily for heating the fuel processor system; this subsystem will be tested for 200 hours. The desulfurizer subsystem reduces 1–10 ppm sulfur in feed gas to less than 0.1 ppm using oxy-desulfurization followed by sulfur sorption; this subsystem will be tested for 8,000 hours.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has made excellent progress toward the targets.
- All technical goals will be met by the end of the project; however, cost is not addressed.
- The project has been completed very well with outdoor test demonstrations.
- The major accomplishments in the past year are that the start-gas subsystem was tested for multiple start-ups, its durability testing plan has been prepared, and durability testing of the desulfurizer subsystem has been completed. Spikes of up to 200 ppb were observed at low-load operation, while mostly the measured sulfur levels were less than 40 ppb. The only items left to complete are the durability test of the start-gas subsystem and some post-test analyses.
- The accomplishments of this project are somewhere in the good-to-outstanding range. Major program accomplishments are the demonstration of the desulfurizer subsystem for 8,000 hours of operation, and a synthesis-gas subsystem for 1,000 hours. The start-gas subsystem testing (200 hours of operation) is in progress. This desulfurizer demonstration was a major program milestone. Investigators are concentrating on durability and performance; they appear to have abandoned start-up and shutdown time and transient operation, although this is less critical in most stationary applications. The investigators have achieved their target of <100 ppb of sulfur in product streams for the desulfurizer subsystem, while the DOE program goal is <10 ppb. The desulfurizer can produce sulfur spikes of at least 200 ppb during low-load operation. This could create problems for the SOFC unless sulfur-tolerant anodes have been developed in the Rolls-Royce (RR) system. They have not addressed this issue. The investigators suggest the use of corrosion-resistant, nitrogen-strengthened stainless steel for commercial units, which is a good idea. Finally, the RR target is 95% system availability, while the DOE goal is 99%. Modern competitive molten carbonate fuel cell systems are at 98% availability, creating an advantage in this area (however, SOFCs have many advantages over molten carbonate fuel cells in other areas).

### Question 4: Collaboration and coordination with other institutions

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

- This project collaborated with RR fuel cells.
- The project is performed with the cooperation of the Ohio Department of Development and the Stark State College Fuel Cell Center with students' involvement.
- No stationary fuel cell suppliers, other than RR itself, are involved. No collaboration with gas suppliers or fuel processing catalysis companies is evident. The only paper or presentation was from the 2011 DOE Hydrogen and Fuel Cells Program Annual Merit Review. The fuel cell community does not benefit from the RR results.
- There has not been a lot of external collaboration. Interaction with the Ohio Department of Development is financial and siting. The relationship with Stark State College involves mostly education/training. There are no interactions with other private industry, because the technology is RR proprietary.

- This project involves active collaboration among the project lead, RR, the Ohio Department of Development, and Stark State College. Thus, this collaborative effort includes industry, state/local government, and an institution of higher learning—an ideal combination.

### Question 5: Proposed future work

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

- The future work will contain the final report.
- The work for the remainder of the project is appropriate.
- Investigators will complete the degradation assessment of the desulfurizer and start-gas subsystem. Following the start-gas test, key components will be examined for chemical or mechanical degradation. A full-up test with the RR solid oxide system (large, but not specified size) is planned. The future work is not contingent on DOE funding.
- This project is nearing completion. The only activities left are to complete the durability testing of the start-gas subsystem, conduct post-test analyses, and prepare a final report on the project. Outside and beyond the scope of the project, the team plans to operate an SOFC on the product gas from the fuel processor, if the necessary funding can be arranged.

### Project strengths:

- This project has well-defined targets and plans and a focus on testing and demonstration.
- This project has a very capable and collaborative project team, and a systematic work breakdown structure and program plan.
- This project developed an approach for testing external fuel processors for SOFCs. The desulfurizer subsystem has been evaluated for up to 1,000 hours. Material and catalyst disabilities were also investigated.
- A strength of the project is the demonstrated durability of subsystems and key components. RR considers the development of fuel cell systems for stationary power to be a key future product line and is capable of funding this work going forward.

### Project weaknesses:

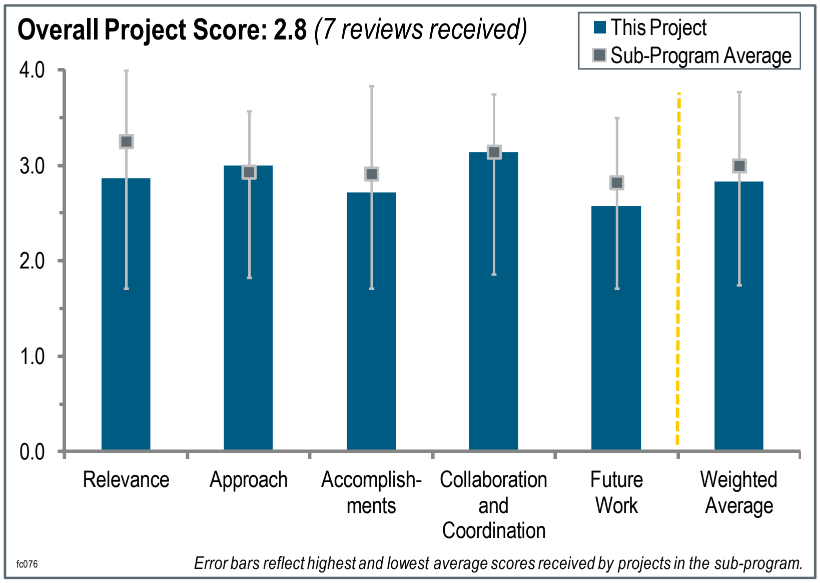
- The integration with SOFC operation is a weakness of this project.
- Spikes of high sulfur in the product line could cause problems unless RR has developed sulfur-tolerant anodes for their fuel cell. This project is also late in meeting milestones.
- Two reviewers said this project has no weaknesses.

### Recommendations for additions/deletions to project scope:

- The Program should continue to follow the work.

## Project # FC-076: Biomass Fuel Cell Systems

Neal Sullivan; Colorado School of Mines




**Brief Summary of Project:**
The main objective of this project is to improve the durability and performance of solid oxide fuel cell (SOFC) systems while lowering costs. The objective is focused on three tasks: (1) develop materials and architectures to improve SOFC durability under biomass-derived fuels, (2) develop biogas fuel processing strategies for SOFC integration and develop low-cost ceramic microchannel reactive heat exchangers for fuel reforming, and (3) provide modeling support for tasks 1 and 2 using computational fluid dynamics (CFD) and chemically reacting-flow tools.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.9** for its relevance to DOE objectives.

- This project is targeted at improving the durability, performance, and cost of SOFC systems. It also seeks to employ wastewater products as a hydrogen ($H_2$) source.
- The project is well aligned with DOE's goals of increasing the durability and efficiency of SOFC systems. The project supports DOE's goal of lowering the cost of SOFC systems.
- This project generally supports the goals and objectives of the DOE Hydrogen and Fuel Cells Program by studying the potential for using bio-waste as a fuel for fuel cell power generation.
- The project goal is to improve the robustness of hydrocarbon- and biomass-fueled SOFCs and systems. The project goals are relevant to the DOE goals of increasing the durability, performance, and transient operation of SOFC systems and decreasing the cost of balance-of-plant (BOP) components.
- The major merit is the durability of SOFC stacks when operated with biogas. The BOP integration serves to increase performance. The durability of the BOP is an issue when operated with biogas, which always contains detrimental contaminants. Indirectly all of these points contribute to lowering cost.
- The goals are to improve the durability of advanced materials, improve control strategies, decrease costs, and develop low-cost integrated reactive heat exchangers. It is unclear how this development fits into overall DOE research, development, and demonstration goals.

### Question 2: Approach to performing the work

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

- The approach is well thought out and focuses on overcoming barriers such as the cost and durability of SOFCs.
- The proposed approach is logical and systemic, combining both experimental and modeling tasks designed to extend and enhance SOFC performance on biomass fuels.
- Collaboration between the tasks has improved. The cooperation between modeling activities and hardware development can still be improved.
- The approach is focused, it creates next-generation SOFC materials and architectures, and it utilizes microchannel-reactor technology for tight thermal integration.

- One major approach is improving the materials and manufacturing technologies of SOFCs. The other major approach is finding new techniques to reform biogas products so that they can be better utilized in the SOFCs. Modeling supports both of these.
- The project consists of three tasks. The approach in task 1 is to develop materials and architectures to improve the durability of the tubular barrier layer and anode supports. The approach in task 2 is to develop bio-fuel processing strategies and low-cost ceramic, microchannel, reactive heat exchangers for fuel reforming. The approach in task 3 is to provide CFD modeling support for tasks 1 and 2.
- The ceramic approach is backed by outstanding competence of CoorsTek Inc., and as such, it is sound. The approach already comprises the cell itself and BOP components, such as a heat exchanger and a reformer based on the heat exchanger technology. The latter is designed for reforming all of the fuel gas because no external water is supplied for pre-reforming, and the $CO_2$ originating from the biogas production is used for CO formation. This approach is outstanding for biogas-operated SOFCs. Developing control algorithms in this program, which otherwise operates on single cells and BOP components, seems premature, particularly because the systems design cannot be tested at this stage or in the near future. The targeted systems size of 1 kW can be understood at this stage of development from a ceramic point of view. For biogas applications, bigger targets should be considered (e.g., in the 100 kW range). One kW is more of a portable application that can be powered with any other, probably bottled, gas.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.7** for its accomplishments and progress.

- This project's accomplishments have been fair. The researchers have demonstrated extended operation of the first tubular barrier-layer SOFC and developed ceramic microchannel reactors for methane steam reforming.
- A demonstration of 50 hours of operation of the perovskite-based tubular anode supports has been performed. The biogas reforming strategies have been completed. Progress has been made on the fabrication and modeling of ceramic microchannel heat exchangers.
- Significant progress has been made toward creating a more durable tubular SOFC by integration of perovskite barrier layers. Significant progress was made toward the development of strategies for biogas fuel processing at wastewater treatment facilities. Significant progress was made toward designing a cost-saving microchannel heat exchanger for fuel processing.
- For the project ending in September of this year, the work in task 1b (perovskite-based anode support) seems far from a positive result. Thermomechanical modeling of ceramic heat exchange is still missing, but it is essential for the evaluation. Pressure drop in the microchannel reactor is critical in system operation. In addition, comparison to a steel heat exchanger is missing and reforming results should be compared to thermodynamic equilibrium.
- Progress seems to be slow and the project is almost finished (85%). The modeling does not seem to follow the experiment very well and the results on the microchannel heat exchanger do not offer much new information. The real challenge for microchannel technology is for the reforming of biomass feed streams where fouling/plugging the channels with impurities, particulates, and delamination of the reforming catalyst is likely to be a serious problem. The flowchart for the system at the Denver Wastewater Treatment Facility is generic and trivial.
- Task 1 has been completed. Tubular perovskite barrier layers were synthesized and integrated with the CoorsTek Inc. SOFC. The project demonstrated >12 days of continuous operation with biogas. The project is extending efforts to perovskite-based SOFC cells with SLT-based anode support and a Ni-YSZ anode functional layer. In task 2, the project developed and validated models to guide the definition of external-reforming operating windows. The principal investigator (PI) demonstrated that the SOFC electrochemical performance on CPOX-$O_2$ and steam-reformed biogas can match the performance with humidified hydrogen ($H_2$). The PI measured the performance of a ceramic microchannel heat exchanger over a broad range of operating temperatures and showed that the addition of rhodium to a steam-reforming catalyst improves the conversion of $CH_4$ to $H_2$. In Task 3, the project integrated the ANSYS-FLUENT CFD model with the CANTERA model for chemically reacting flows. The integrated model was used to optimize the backing-side conditions for reactive testing and to determine the set points for high $CH_4$ conversion. The project is extending the high-fidelity flow models to develop rapid linear models for predictive controls that can be used to meet

load demand while satisfying the constraints. The PI is conducting systems analysis to evaluate the integration of an SOFC system with the Denver Wastewater Reclamation Facility.
- Great progress was made on the SOFC tube by integrating a new SLT barrier layer and achieving a reasonably good areal resistance, the generally low areal resistance of tubular designs considered. The barrier layer test results should be extended to a minimum of 2,000 hours in order to obtain reliable results, because some impurities have the tendency to accumulate and hence only become obvious after extended operating times. The ceramic heat exchanger and reformer are great developments. Using microchannel technology for this allows for high-systems integration. On the systems level, the pressure drop in microchannels should be considered, since it may lead to exaggerated compression losses in the system if the channels were very small. On the systems integration side, early efforts should be started to connect the ceramic devices to the piping, which was a major hurdle for integrating ceramic heat exchangers for industrial applications about two decades ago and which finally made industry abandon that pathway. Further information on the gas-tightness of the ceramic BOP components would be important. Based on the developed models, operating conditions for the reformer could be figured out that allow for biogases use at good efficiencies in the SOFC. Considering the budget, the multiple tasks involved, and the duration of the project, the results achieved are outstanding.

### Question 4: Collaboration and coordination with other institutions

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

- The collaboration between CoorsTek Inc. (Golden, Colorado) and tubular SOFC is fair.
- CoorsTek Inc. is a partner in this project and supplies materials and SOFCs.
- This project has coordinated collaboration well between a university and an industrial partner.
- Further collaboration outside CoorsTek Inc. and the Colorado School of Mines (CSM) is missing.
- This project has good collaboration with CoorsTek Inc. The collaborations could have been expanded to include expertise in biomass reforming, which is ongoing at many locations currently.
- This project has very good collaboration with CoorsTek Inc., a major United States ceramics manufacturer. CoorsTek Inc. receives no funding for their activities on the project.
- The fact that a strong industrial lead from CoorsTek Inc. was combined with the scientific approaches of research institutions is the strength of the project. All partners performed their work with high proficiency.

### Question 5: Proposed future work

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

- The project ends in September 2012 and is 85% complete.
- The proposed work contributes well to the goals.
- The planning should have more detail with well-defined objectives.
- It is not clear what the remaining work is; there should be more detail explaining how the remaining work can be accomplished in the remaining few months.
- The PI has laid out plans to complete each of the three tasks in the remaining few months of this project.
- The plans are built on achieved progress and may lead to the development of a new, more robust SOFC architecture.
- This project is almost over; in the remaining time effort should focus on the experimental components of tasks 1b and 2b and the researchers should work closely with the existing partner.

### Project strengths:

- This project has a strong industrial partner.
- The academic knowledge of CSM and the industrial expertise of CoorsTek, the largest ceramic company in the United States, is a strength of this project.
- This project has a good combination of academic knowledge and industry experience. There is good correlation between modeling and hardware testing.
- This project has a strong industrial connection. The ceramic microchannel work may find applications in a number of different industrial areas.

- This is a multidisciplinary approach that involves catalysis, engineering, and advanced modeling. This project uses CFD modeling as a guide to design heat exchangers/fuel processors.
- This project has a combination of a strong industrial lead, high ceramic competence, and a strong research. Developing a perovskite anode is a good approach for combating contaminants instead of painfully cleaning the biogas to extreme levels.

### Project weaknesses:

- This project has no weaknesses.
- This project could be making progress at faster rate, because it is three years along.
- This project's progress is slow and it lacks an experimental validation of models developed in the project.
- This project lacks a clear definition of the deliverables. The PI has not explained how CoorsTek has benefited from this research.
- Tubular SOFCs are not durable enough to tolerate sulfur, and incorporating additional equipment required to eliminate impurities may increase the cost of the total system.
- The project has not evaluated whether the stack and heat exchanger concept used has the potential to achieve the performance and cost goals.
- The project is very broad and the target of developing 1 kW and considering biogas reforming are not fully in line. The project can be understood as a scouting project on what is possible to achieve using ceramic approaches. In that sense, the broad approach can be understood as a strength.

### Recommendations for additions/deletions to project scope:

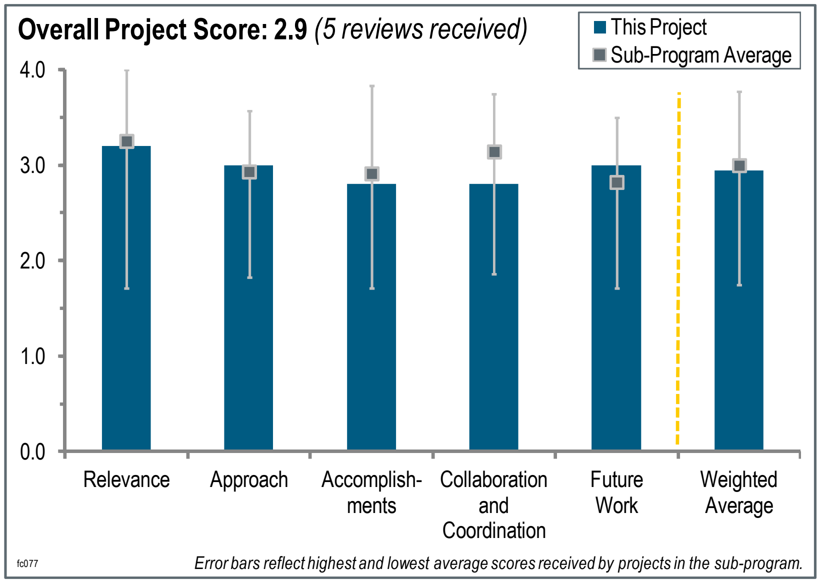
- This project should maximize experimental results in the time remaining in the project.
- This project should concentrate on real stack tests using biogases.
- The researchers should conduct a long-term test of the developed ceramic tubes under biogas (i.e., >2,000 hours), and integrate the ceramic heat exchangers with the tubing at an early stage.

## Project # FC-077: Fuel Cell Coolant Optimization and Scale-up (plus work under SBIR III project)

Satish Mohapatra; Dynalene

### Brief Summary of Project:

The goals of this project are to: (1) develop a flexible, instrumented fuel cell coolant system; (2) experimentally qualify coolant properties and performance through steady operation durability testing, non-operational durability testing, load cycle impact testing, and advanced coolant accelerated qualification testing; (3) determine corrosion inhibition efficiency of coolants; (4) increase surface charge of nanoparticles; and (5) conduct long-term testing and demonstration.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- Long-life, low-conductivity coolants are needed for meeting DOE's fuel cell goals.
- There are already coolants that are suitable for the DOE Hydrogen and Fuel Cells Program (the Program). This project is not a high priority for the Program.
- It would be useful to develop a specific coolant that meets the requirements of automotive fuel cell systems, but there are also other methods to deal with this issue.
- The project supports DOE's research and development objectives, in particular for the balance of plant by developing a new fuel cell coolant. The main innovation is not using a resin filter, which reduces cost, weight/volume, maintenance needs, and the pressure drop.
- This technology is very creative and a good fit for the Program. In industry in the past there was almost no adequate selection of coolant systems and development of a new one was needed. Performing work such as this in the public arena will help other coolant manufacturers to endorse and develop the ideas of additives for optimal fuel cell operation.

### Question 2: Approach to performing the work

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

- This is a very creative and effective approach.
- The project has been well structured from Phase I to Phase III. Returns from field trials should be added and reported regularly in Phase III.
- It is not clear how this approach (nanoparticles) will work outside the laboratory in the automotive application. There were significant issues with test results at 120°C.
- This is a reasonable approach, though it is not clear that the scale-up process is near optimal. More details about the methodology used would be helpful. Higher-temperature operation remains somewhat of an open issue.
- The use of nanoparticles to enhance the thermoconductivity of fluids is not uncommon in the industry. Typically nanoparticles use surfactants to prevent settling or flocculation. This needs to be described in the system. The approach did not seem to include tests at low-temperature operation.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The company has shown a pathway to develop an effective coolant.
- Test data has been generally positive, but incomplete, especially at higher temperatures. No automotive end-user data was presented, although there are indications that testing is underway.
- Because scale-up is a significant objective, it is important that the path be established to much larger production rates. It is not clear that the current manufacturing technique will scale well. Otherwise, this project has made solid progress.
- The accomplishments and progress were good this year and have been presented correctly. The fuel cell coolant was optimized up to 80°C operation, and scale-up to 100 L has been completed. Improvement for operation at higher temperatures is needed. To do that, better mechanism understanding is requested. The testing facilities have been completed, but long-term testing has not been presented. The corrosion inhibitors were validated in short-term testing using immersion and electrochemical methods. Plastics and gaskets contribute more to conductivity increases than metals in pure water. This has to be taken into account in the use of this coolant depending on the system design. But further investigations using the developed coolant are needed to qualify it.
- The degradation of the fluid at high temperatures needs to be understood. Fuel cells are moving to a higher temperature of operation, so thermal stability is important. The conductivity in some cases appeared to increase over the baseline with the addition of the nanoparticles. With a nanofluid, settling and flocculation are important, particularly over time. These characteristics need to be understood over the same range of temperatures that the fuel cells will experience. The presenter mentioned that the nanofluid separates when frozen. This would concentrate the nanoparticles, which would increase the pore clogging and channel clogging as well as concentrate the surfactants to a level of concentration that may impact gaskets and other components, so chemical compatibility needs to be tested.

### Question 4: Collaboration and coordination with other institutions

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

- The inclusion of a fuel cell company to the team is extremely important to validate the project.
- This project collaborated only in the tests. It is not clear that the testing covers the most critical parts of the future operating parameter space.
- The project is limited in scope and funding, and as such, no end-user data is presented, but it is obviously critical to the success of the concept.
- The company is working with fuel cell manufacturers, thus it is following system performance specification flow-downs that will allow it to integrate with field systems.
- Collaboration is correct, but it should be enlarged in particular with systems producers in order to improve the industrial feedback on the approach and the experimental field return.

### Question 5: Proposed future work

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

- The completion plan is acceptable.
- The manufacturer is looking at the right tasks to further deploy their coolant.
- The proposed future work is a good start. They need to expand beyond ambient temperature testing for this system.
- There is still much work (durability testing) ahead, but there is limited funding to feed back the results of the testing into the coolant development.
- The future work is correct. At this stage, cost targets and achievements should be presented. Testing this coolant in different system applications (automotive, forklift, stationary) would make is easier to better appreciate the potential of this product.

### Project strengths:

- This project is an interesting concept.
- The innovation of the cooling concept and combining water-based materials to achieve the goal are both strengths of this project.
- This project is taking an interesting approach to improving the liquid coolants for fuel cells. The project seems to be well organized.
- This is an innovative approach that may lead to a significant positive impact on the fuel cell system in terms of mass, weight, and maintenance by suppressing the resin filter. The different materials present in a fuel cell system have been taken into account.

### Project weaknesses:

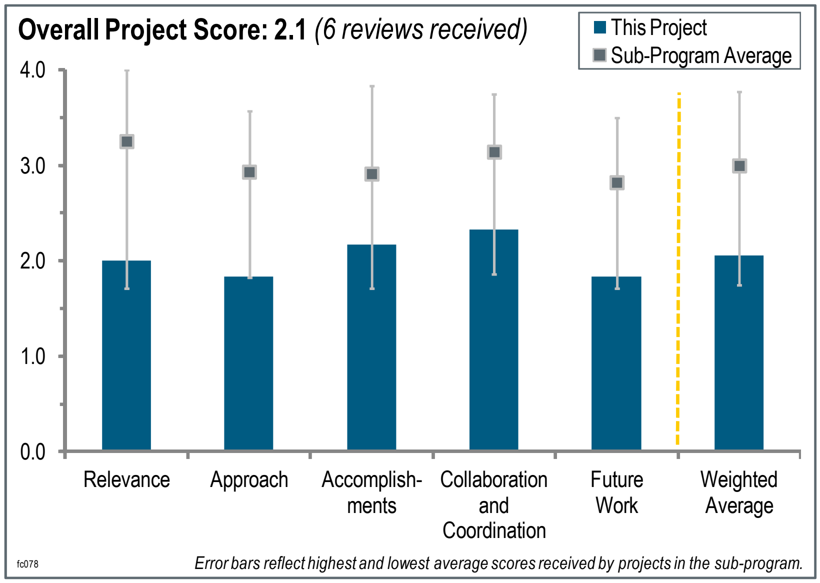
- It is not clear that the scale-up path will be adequate for large production volumes. It is also not well communicated or justified why water-based coolants are required.
- This project needs a better definition of success, especially for accelerated testing. It is not clear if material tests indicate whether the coolant meets requirements or is simply better than the alternatives.
- No long-term data was presented. To facilitate the comparison, the results presentation could be more homogeneous, particularly with regard to the units. Sometimes the durations are in weeks and sometimes they are in hours. A better understanding of the degradation mechanisms should be carried out.
- Freeze-thaw characteristics need to be investigated. Of particular concern is the separation of the materials that was shown in the discussion portion. This was not addressed in the future work section and should be added. The surfactant chemical compatibility with the materials and particularly the seals needs to be considered. While the surfactant concentration is low, the materials will be in contact for long periods of time (years), and during freezing there is a separation of the components, which would concentrate the surfactants in the seals.

### Recommendations for additions/deletions to project scope:

- Considering the constrained budgets for the Fuel Cell Technologies Program, this seems to be a low priority.
- The researchers should work to understand the effect of conductivity increases at higher temperatures (105°C–120°C) in order to ensure the 5,000 hour target with $<2$ µS/cm measures the thermal conductivity effect. The researchers should also investigate the effect of the potential gradient in a high-voltage stack over time to see if it has any effect or not on the nanoparticles and corrosion inhibitors stability. The researchers should measure the conductivity variations in various materials with the developed fuel cell coolant as they have been doing with ultra-pure $H_2O$ at 80°C for three weeks, and then focus the investigations on the materials that present the highest interaction.

## Project # FC-078: 21st Century Renewable Fuels, Energy, and Materials Initiative

Joel Berry; Kettering University




### Brief Summary of Project:

The goals of the project include: (1) the development of an improved high-temperature polymer electrolyte membrane (PEM) fuel cell membrane capable of low-temperature starts (<100°C) with enhanced performance; (2) the development of a 5 kWe novel catalytic flat plate steam reforming process for extracting hydrogen ($H_2$) from multi-fuels and integrating them with high-temperature fuel cell systems; (3) the development of an improved oxygen ($O_2$)-permeable membrane for high-power-density lithium (Li)-air batteries for ease of use and reduced cost; (4) the development of a novel, high-energy-yield agriculture bio-crop (Miscanthus) for alternative fuels with minimum impact on the human food chain; and (5) the extension of the math and science alternative energy education program to include bio-energy and power.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.0** for its relevance to DOE objectives.

- The project is relevant to the objectives of Fuel Cell Technologies Program (FCT Program) Multi-Year Research, Development and Demonstration Plan (MYRDDP). The activities are aligned to DOE's goals. This project brings a very wide variety of initiatives directed toward many different areas of alternative energy projects. This program seems to be very broad and covers very diverse areas of science.
- This project has five separate tasks. Two of the tasks (developing fuel cell membranes and catalytic plate reformers) are directly related to DOE Hydrogen and Fuel Cells Program (the Program) goals, while the other three (high-power-density Li-air batteries, high-energy-yield bio-crop, and alternative energy education) are not relevant to the Program.
- This project has four different unassociated parts. Only the high-temperature membrane is relevant to the fuel cell components portion of the FCT Program. The steam reforming reactor has a slight relevance to $H_2$ production, but this type of design does not appear relevant to the goals of the hydrogen production portion of the FCT Program. It is unclear why battery development is being supported by the FCT Program. The other portion should belong to DOE's Biomass Program.
- This project involves the development of several technologies, including high-temperature PEM membranes, a flat plate steam methane reformer, Li-air batteries, feedstock for biofuels production, and education. The first two support the Program's objectives. Batteries and biofuels production belong in the Vehicle Technologies and Biomass Programs, respectively. Education could be said to be a Program activity.
- The project plans to develop: (1) a high-temperature fuel cell membrane, (2) a steam reforming reactor, (3) a high-power-density Li-air battery, and (4) novel high-energy-yield agriculture bio-crop. Task 1 is relevant to the Program. The battery part might be of interest to the DOE Vehicle Technologies Program and the bio-crop development may be relevant to the Biomass Program. While the overall project supports the DOE Office of Energy Efficiency and Renewable Energy's goals, certain tasks do not appear to be relevant to the Program.

### Question 2: Approach to performing the work

This project was rated **1.8** for its approach.

- There are interesting areas of research here, but it is not clear why there are such diverse topics in one project.
- The approach of this project is unclear and appears scattered due to the four unassociated pieces of the project. The membrane portion objective appears to "increase" conductivity and enable a high-temperature operating PEM with low-temperature start-up; however, it is unclear how the researchers are approaching the low-temperature start-up. Overall, there is almost no detail in the approach section.
- The approach of separating the projects into several different tasks to cover multiple areas of development is the right way to manage different scientific developments required for the success of this project. The technical barriers for each development pathway are addressed properly; however, a little more description would have been better. Very few details were given for the Li-air battery and "Biofuel from High Yield Energy Crop" approaches.
- Insufficient detail was presented to determine how well the relevant barriers are addressed in the two tasks related to the Program. The barriers to the development of the high-temperature membranes are not identified. The membrane work does not appear to address the barriers to membrane durability or the operation over an operating range, which includes water condensation. The leaching of phosphoric acid and Sulfonated-Polyhedral Oligomeric Silsesquioxane (S-POSS) are not addressed. There may be some impact on improving mechanical properties and the durability of polybenzimidazole (PBI)-phosphoric acid membranes by adding the S-POSS. The reforming work does not identify the barriers for fuel reforming. Barriers, such as reformer durability, do not appear to be addressed. The issue of lower surface area on a plate reactor compared to a pelletized bed reactor is not addressed. A non-platinum-group-metal catalyst is used, which should address the reformer cost issue.
- The investigators propose to tie together several different research objectives into a strategy for power generation. The biofuel generated from Miscanthus will "feed" the flat plate reformer, which will provide $H_2$ to a PEM fuel cell (for which the project is developing a membrane for low-temperature operation). When the whole system is used as an auxiliary power unit, the Li-air battery is used to provide power while the fuel cell "gets up to speed" in about 20 minutes. All the technologies under development in this project have significant barriers. Solving any one of them involves a major effort, and trying to do all of them is probably not possible with the funding provided. The project gives the appearance of starting from scratch in several different technology areas. There is no interaction with external resources that have dedicated many years and lots of research and development investment in the areas of research involved in this project, except for membrane development, which Michigan Molecular Institute has experience in. Frankly, it is not obvious at all why the researchers are developing an Li-air battery when there is so much work going on in this area around the world. The novelty of the materials and design they are pursuing is not clear. A similar comment can be made about the development of Miscanthus as a high-yield bio-crop. A lot of work has been done on it at places such as the University of Illinois. The data shown in figure 14 are taken from the literature. The process by which they convert biochar to E85 is not described. There is no plan for commercialization.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.2** for its accomplishments and progress.

- Not enough results were presented to interpret whether the objectives are met, such as electrochemistry using the membrane and testing with standard activity and durability test protocols.
- Progress since fiscal year 2011 has been limited. A different membrane and synthesis pathway has been followed, which will take some time before it is fully developed and tested, although some promising conductivity results are given for 10% S-POSS. Results presented for the Li-air battery in $O_2$ versus dry air are a somewhat obvious results. The experimental results for the fuel reformer are expected. The computational fluid dynamics model used to design the reformer is not described. There is no attempt to relate the SEM results to the Li battery performance, and the anode and cathode reconstruction in cycling is well known.
- No lists of publications were provided. Using carbon for the air electrode of an Li battery makes no sense. This is also irrelevant to the stated goal of developing a better $O_2$-permeable membrane for high-power-density Li-air batteries. The durability of the membrane that has been developed is not clear. The principal investigator (PI)

informed this reviewer that the researchers are waiting for independent confirmation of the conductivity of these membranes. This data will be useful. Two of the four graphs in the reformer section are a repeat of 2011 data.

- Slide 17 indicates that the researchers have presentations, publications, and patents, yet none are listed anywhere. There is one slide on the membrane development, with data only at 90°C, 120°C, and 150°C. It is confusing why there is no data for low temperatures (i.e., approximately 20°C), because one of their main objectives is for low-temperature start-up. It appears they have made a total of seven membranes. For the reformer, the presenter did not show data relevant to the fuel conversion and selectivity to $H_2$ production, and nowhere did the presenter state what fuel was used.
- Overall, good technical accomplishments in all five different developmental pathways had been achieved. However, from the reported results it seems that the remaining part of the project will be difficult to complete by June 2012 when the project ends. It is not clear whether an extension of the project had been requested by the team or not. A good portion of in-situ evaluation work is needed for Li-air batteries and high-temperature membrane areas. For Li-air batteries, $O_2$ performance is reported, while battery performance in the air is the goal of the project and is not finished. Similarly for high-temperature membrane development, the high-temperature fuel cell performance evaluation is not completed.
- The fuel cell membrane work was able to improve on the conductivity the researchers obtained with PBI-phosphoric acid by doping the PBI-phosphoric acid with S-POSS, but this project has not improved on the conductivity of PBI-phosphoric acid reported in the literature at high-temperature conditions. In discussions with the researchers, they claimed to have improved the mechanical properties of the PBI-phosphoric acid membrane, but data for mechanical properties was not shown. It is unclear from this work whether S-POSS will improve conductivity of standard PBI-phosphoric acid membranes. There was a typo in the poster that suggested increasing the fuel-to-steam ratio increases $H_2$ production; however, in discussions with the researchers they revealed that this is a typo and it should have read increasing the steam to fuel (or steam to carbon) ratio increased $H_2$ production. Discussions also revealed that this is a microchannel plate reformer. Microchannel plate reformers have been investigated by Pacific Northwest National Laboratory in the past and there did not appear to be new results here.

### Question 4: Collaboration and coordination with other institutions

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

- This project seems like it is several independent projects run without any coordination.
- The project appears to be a collection of five separate tasks with little collaboration between the groups.
- This project has good collaboration, but is there not a national laboratory or business partner that could give a good perspective on the work.
- The researchers have three different institutions involved in this project, but there appears to be no collaboration between them, as they are working on totally different things.
- The team consists of good partners, including universities and research laboratories. The inclusion of a national laboratory could have helped the team to get access to better research facilities and resources.
- The PIs have formed collaborations with Michigan Molecular Institute and Saginaw Valley State University; however, the project is not well coordinated overall. It appears that investigators proceed mostly independently on the separate components of the project. The investigators do not appear to have reached out to other institutions and private industry with long histories of membrane, Li battery, and fuel cell development. Such collaborators could help the project move ahead more quickly. There is no current industry involvement, which could lead to commercialization. The PI reports just having contact with a private company, Global Energy Innovation, which is also located in Flint, Michigan.

### Question 5: Proposed future work

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

- Given the project is 95% complete, there is too much future work. It is unclear how the researchers plan to accomplish all of it.
- The work is not focused well enough and would require a significant amount of funding to achieve meaningful results for all goals.

- This project should slow down on the biofuel and Li-air battery and concentrate on developing membrane electrode assemblies and testing their results on the high-temperature membranes, which show some promise.
- It is impossible to tell what is different in terms of the membrane development future work and what was done in the past. The other three tasks are not relevant to the FCT Program's MYRDDP.
- The future work described is aligned with the proposed work of the project. The team needs to complete the cell performance evaluation for Li-air batteries and high-temperature fuel cells quickly because the project completion date is only a month away.

### Project strengths:

- This project could make progress in membrane development.
- The team is well organized and capable of evaluating such diverse areas of science. The team is equipped with the necessary resources required for the success of this project.
- The membrane task and reformer task are relevant to the Program. The membrane work could improve the performance of high-temperature PBI-phosphoric acid systems.

### Project weaknesses:

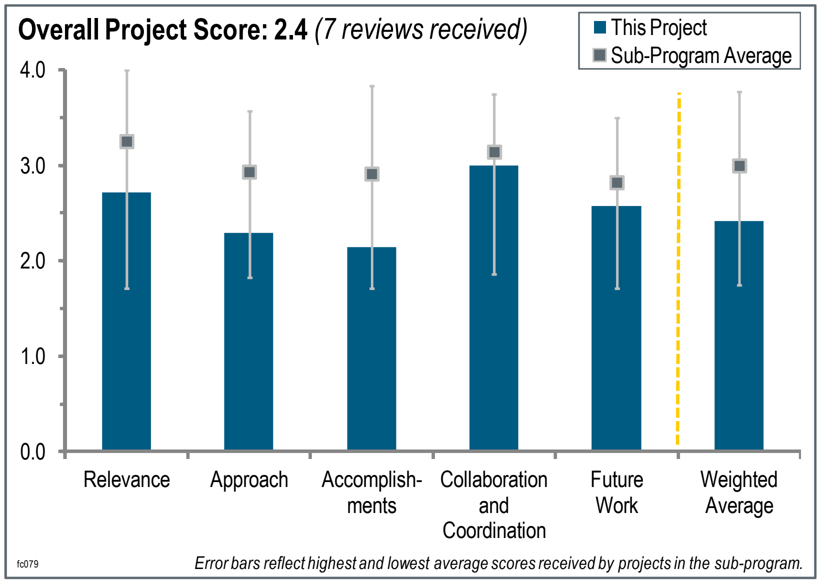
- The tasks of the project do not seem relevant to the Program.
- This project is spread too thin over some interesting areas. More collaborators or a less-broad range of topics addressed would have achieved more results.
- The project does not appear to have specific targets or milestones for performance. Much of the project is unaligned with the Program's goals.
- This project is too diverse. This will make it difficult to achieve significant progress in any one technology area, let alone all of them with enough resources to bring it together at the end. The project coordination is poor, and it does not leverage work that has been done or is ongoing in other institutions/private industry. There is also no industry involvement.
- This project is very diverse and a project duration of two years is very little for the completion of all the proposed tasks in the project. Despite this limitation, the team did a commendable job of pulling together a functional working group between three different institutes and completed the majority of the tasks within the proposed time.

### Recommendations for additions/deletions to project scope:

- This project should not be continued.
- Everything but the high-temperature membrane work should be eliminated.
- The bio-crop and Li-air batteries are unrelated to the Program goals and objectives and should be dropped.
- In the future, DOE should allow more time for such diverse multi-area development projects to allow successful completion of the project in time.

## Project # FC-079: Improving Fuel Cell Durability and Reliability

Prabhakar Singh; University of Connecticut Global Fuel Cell Center




**Brief Summary of Project:**
Goals of this project were to develop an understanding of the degradation processes in advanced electrochemical energy conversion systems, and to develop collaborative research programs with industries to improve the performance stability and long-term reliability of advanced fuel cells and other power generation systems. The technology objectives focused on advancing fuel-cell-based power generation systems architecture, including renewable hybridized energy conversion and storage; developing and testing novel cell and stack structural and functional materials; and gaining a fundamental understanding of chemical, mechanical, electrochemical, and electrical processes.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.7** for its relevance to DOE objectives.

- Most of the topics reported on are in-line with DOE's objectives.
- Overall, the objectives are addressed through a very wide range of interactions with industry partners.
- The project is essentially 10 independent projects with varying levels of relevance to DOE's research and development objectives.
- This project is relevant to DOE's objectives; it is critical to understand the degradation processes and mechanisms for electrochemical systems, especially with the long endurance needed requirements.
- The project seems to cover several different areas and have some general objectives, but it is hard to understand how the teams help each other and what specific goals they want to meet.
- This is an atypical project in that although it can properly cite relevance to the cost, performance, and durability barriers, it looks at all types of fuel cells and therefore is not likely to be able to address the three barriers simultaneously for any one type, such as polymer electrolyte membrane (PEM) fuel cells for transportation.
- The focus on fuel production and processing involved with bio-gas scrubbing, desulfurization, and nanostructured support for acid-based electrolyzers are significant steps in advancing fuel cells. The work related to gas diffusion layer (GDL) manufacturing will definitely be an integral part of membrane electrode assembly manufacturing in the future as well.

### Question 2: Approach to performing the work

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

- The approach here is to broadly address durability and reliability from a system perspective. Many different systems are considered (PEM fuel cells, molten carbonate fuel cells [MCFCs], and solid oxide fuel cells [SOFCs]). This broad approach may not be efficient.
- This project is too broad and the approach lacks a plan that ties the various work streams together. Although each work stream is somewhat organized in its topic area, these are niche areas of research. This seems like a buckshot into the fuel cell research space.

- The large number of small, disconnected projects significantly lowers the potential of meaningful impact resulting from any specific project. Teaming professors with industrial partners helps to provide guidance to projects.
- The approach is well defined, along with the roles and responsibilities for the team members. The presentation did not define go/no-go decision points for the tasks; this would have been helpful to determine the progress to date of meeting targets. It is recommended that go/no-go decisions be presented at the next review to evaluate the basis for the down-selection and scale-up efforts. A modeling effort for tasks 2–4 would have been helpful; modeling could reduce the number of tests to be performed and provide a quick evaluation of catalysts, materials, and electrode architectures.
- The approach is very broad and ranges across all technology aspects of all fuel cell types. It therefore has the danger of not being able to complete anything meaningfully for any one type. But the researchers' unique collaborations and method of execution saves the day on this point. There is also the opportunity for more broadly based mechanisms that apply across the board to come out because of the close networking possible among the co-located C2E2 faculty and students and be understood.
- While the project is very ambitious in tackling multiple problems with various principal investigators (PIs), it feels as though the focus has been a bit diluted in the end. The main goal of the project was to improve the durability and reliability of fuel cells. While the majority of the efforts have been focused on understanding the various processes and the modes of improvements in them, the clarity regarding the big picture is lacking. Maybe a focus on more than one type of fuel cell system might have led to the confusion. A unified focus attempt at incorporating the improvements to demonstrate the overall improved reliability and durability will be very useful. Also, developing a metric to qualify the improvements achieved in terms of life, cycleability, and/or cost would be very useful.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.1** for its accomplishments and progress.

- Some general results have been achieved, but there is no significant progress.
- The accomplishments are somewhat obvious and redundant relative to the other, more focused, programs.
- Progress so far is difficult to judge because each task is so isolated. It would appear that, overall, only two out of eight subtasks are on track to deliver to their stated objectives. These are 2.1 and 2.3. The others are struggling or failing to be relevant.
- An interesting example of how basic mechanisms may apply to widely disparate fuel cells for subtask 1.3 may be the result of nickel (Ni) precipitation in the MCFC electrolyte matrix, analogous to the platinum (Pt) precipitation in PEMs due to high voltage cycling.
- Out of the 10 different thrust areas, only a few have results that show any progress toward overcoming barriers or making progress. The work on MCFCs is interesting and represents an area lacking in current research and development. Most other work has a hard time competing at this scale with current state of the art. The work on fuel reforming catalysts (subtasks 2.2 and 2.4) are two of the more promising thrust areas.
- Little data was presented, accomplishments were made in generalities (promising performance was observed) and data was not presented to back up accomplishments. At this phase of the project bench-top testing should have been underway, and this final year should be focused on optimizing the several systems being evaluated. The presentation does not describe in detail the scale-up efforts (what sizes, what metrics will be evaluated, for how long, etc.). There seem to be too many unanswered questions in each of the four tasks. It is unclear if the goals of the project can be met with the proposed future work.
- While progress has been made in some areas, there is still scope for significant progress to be made. The future work listed by the PIs will be a good start. Furthermore, developing metrics for performance, stability, and durability related to the improvements made in the manufacturing/material design needs to be considered. For example, the motivation for the design of improved GDL needs to be established in terms of a durability measure. Examples include finding out what changes during degradation of the GDL/microporous layer (MPL), how the X-ray tomography addresses these, and how these are dependent on operating conditions. Similar questions must also be posed and addressed for other systems, such as mechanistic understanding of MCFC matrix stability/high-performance electrode development using soluble polymers.

### Question 4: Collaboration and coordination with other institutions

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

- Having industrial partners for each task is a great way to keep the project relevant.
- A strong collaborative team has been assembled and tasks seem well coordinated among team members.
- Several groups and institutions have been involved in the project. The collaboration and coordination is very limited.
- This project seeks to use the DOE project to seed collaborations with a wide range of industrial partners. It is assumed they are successful, but perhaps a national laboratory or two could also be involved.
- Each subproject has coordination between a professor and an industrial participant, although industrial researchers or the mechanism of interaction is not clear. Additionally, there is almost no synergy among the 10 thrusts and how the co-location of these 10 projects is somehow beneficial is not evident. The project has had two publications to date.
- This is an excellent project to demonstrate the multiple collaborations between both academia and industry. There are many players and clear communication between the various institutions is visible in the progress made. The isolation between the various subtasks is due to the nature of this project and makes the degree of collaboration lower to a certain degree. This could be improved by identifying commonalities between various subtasks and working on implementing the improvements across the subtasks under a single roof.
- The wide breadth of collaborations and the way one faculty member and a post-doc are focused on a key issue for each collaboration would appear to improve significantly the odds that some good value will be created for each of the “customer–collaborators” involved. The close collaboration on a problem that the collaborator chose and is partially funding is also a benefit and increases the chances of success for each case because the “collaborator” has a bigger stake in the outcome.

### Question 5: Proposed future work

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

- There were many projects funded for the amount of DOE investment. There is a good probability of at least one being meaningfully successful.
- In many cases, it is not clear how the future work will result in a clear deliverable that is needed by the development community.
- In most cases, future work may lead to small incremental improvements but is unlikely to result in meaningful advances in any given area.
- The future work plans may result in some improvements for each task. The team should prioritize its tasks to achieve more significant progress.
- Generally, there are good plans to go ahead. There appears to be no plan forward for subtasks 1.4, 3.1, and 4.1. The relevance of the plan for subtask 1.2 is not clear; it does not follow from the reported work.
- Plans are defined for the next year, but they lack metrics of what needs to be accomplished in this final year to meet objectives. It is not clear that all future plans can be realistically achieved, because most scale-up efforts have not started or just began. Too many unanswered questions still need to be addressed. An example is on slide 20; future work for subtask 1.3, material solubility, dissolution/growth mechanisms, and additives should have been evaluated at this point in the project. The same goes for slide 26, subtask 2.1, and the list of 11 future work efforts that need to be accomplished.
- In terms of task 1, while understanding the GDL characteristics using synchotron and neutron radiography adds value to the design of GDLs, it is not clear how this will impact the durability or reliability of the GDL/MPL. It is unclear how they will quantify the improvements and if they have considered the implications of the ionic liquid gel-based designs on water/gas transport properties. It was also unclear if this has been investigated, either using modeling and/or in literature. This was not clearly expressed in the motivation for this work. While suggesting that additives will be tried and tested, the PIs do not clearly say the reasoning behind or the selection criteria for these additives. Also missing is the link regarding the mechanism and stability issues in terms of Ni solubility. It was unclear if these are independent events and, if so, why the Ni solubility was focused on as part of previous work. It is an interesting approach to try and influence the microstructure of the catalyst layer using the synthesis procedure. However, the PIs do not address the issues related to the formation of films on the

catalyst particles and the potential impact of such films on resistance to both mass and electronic resistance. It seems unclear how the potential benefits achieved with soluble polymer are different from varying the loading of the binder in the catalyst layer. There is significant literature regarding such things. In terms of task 2 and 3, the future work scope for task 2 seems reasonable and follows the desired initial objectives. There still needs to be a clear definition of how each of these tasks has potential implications toward the fuel cell's operation.

### Project strengths:

- This project covers a wide range of topics.
- There are many diverse projects centered around one department.
- PEM fuel cell and phosphoric acid fuel cell (PAFC) tasks are focused on critical development needs and seem well aligned with industrial partners.
- The relative co-location of these researchers along with a strong fuel cell presence in Connecticut is a project advantage.
- This project has full engagement of a wide range of industrial partners and tackles challenges from a complete system engineering perspective, as well as fundamental materials.
- A large number of PIs focusing on specific projects has shown significant accomplishments in the past year. Involving the industrial partners with every subtask ensures that the research is always related to the measurable properties qualified by the industry.
- This is a strong team with well-defined tasks, roles, and responsibilities. The program outcomes are impressive with several patents and publications/presentations to date. If successful with the tasks at scale-up stages, the team has a strong industry base to transition techniques and technologies into manufacturing.

### Project weaknesses:

- This project has no focus.
- This project has too many tasks listed to be accomplished in the three-year effort. The proposed future work for the final year is not realistic.
- This very broad approach may defocus efforts. One wonders if it is possible to deliver to all 10 tasks at the end of the project. There is no coordination; each project is completely independent of each other.
- Several tasks are not well aligned with the near-term needs of the Hydrogen and Fuel Cells Program. The work is very scattered and there is no obvious plan to tie things together.
- The lack of a unifying vision or connection between the 10 project thrust areas makes this project equivalent to 10 single-investigator National Science Foundation projects. The lack of depth and disconnection of these 10 areas makes reviewing the project challenging.
- The project aims to solve problems in MCFCs, SOFCs, PEM fuel cells, fuel production, electrolyzers, and PAFC systems. While at the outset this type of variety offers significant opportunity for cross-collaboration, this type of effort is found missing. There are no clear measurables listed in terms of how the accomplishments of this project impact the fuel cell stability, reliability, and operation.

### Recommendations for additions/deletions to project scope:

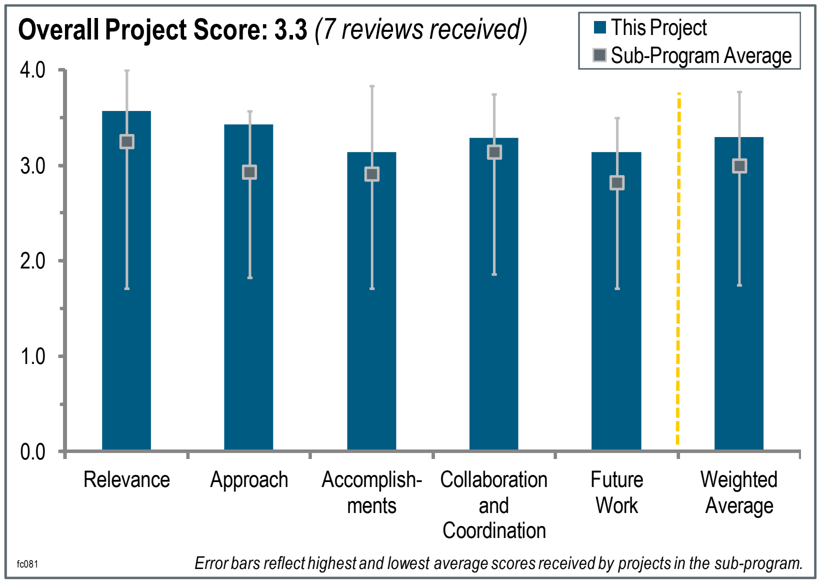
- This project should prioritize its tasks.
- The value of this project could be shown with a system-level demonstration that utilizes the knowledge from several tasks.
- This project needs to increase coordination between the subtasks. The work for Pt/WC focusing on HER is a low concern for electrolysis and should be redirected to OER.
- The future work for subtask 1.2 does not make sense—it does not follow from the reported work. It is assumed that activities will cease in subtasks 1.4, 3.1, and 4.1, because there is no future work planned for these subtasks.
- Tasks 1 and 2 need to be re-evaluated. The model effort should be expanded to make predictive decisions of matrix supports. Subtask 1.4 has a high impact on the industry; this task should be a priority. It is recommended that task 2 be focused on feasibility and efficient fuel processing approaches instead of desulfurization techniques.

## Project # FC-081: Fuel Cell Technology Status—Voltage Degradation

Jennifer Kurtz; National Renewable Energy Laboratory

### Brief Summary of Project:

This project focuses on three main ideas: (1) achieving benchmark state-of-the-art fuel cell durability by developing a snapshot of state-of-the-art fuel cell durability, uniformly applying analysis methods to data accumulated in a laboratory, and obtaining independent assessments and the status of state-of-the-art fuel cell technology; (2) leveraging analysis experience by utilizing analysis methods, experience, and data from fuel cell field demonstrations and comparing laboratory and field data; and (3) collaborating with key fuel cell developers to provide feedback, investigate factors affecting fuel cell durability, and study differences between laboratory and field durability.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- The project is essential to benchmarking the progress of fuel cell systems over time and across industries.
- The objective is to get an independent assessment of the state of the art for durability by collecting information from system manufacturers.
- Collecting field and laboratory data on fuel cell durability is very important to assess the current status of fuel cell technology. The project will help the DOE Hydrogen and Fuel Cells Program (the Program) plan future goals.
- It is very relevant to the goals and objectives in the Fuel Cell Technologies Program Multi-Year Research, Development and Demonstration Plan. Real-world data is important from the industrial point of view, so it should be utilized well.
- This project addresses a key barrier—fuel cell durability, especially real-world durability, which is known to often yield much different results than in the laboratory.
- This is a technology validation project, in contrast with a technology development project. It gathers data from laboratory tests of fuel cells to provide situational awareness on the technology readiness of real-world stacks, and how they degrade over time. While this may be important for strategic decision making within the Program, in and of itself it will not overcome technical barriers.
- This project provides an important service to people running durability tests who do not have the tools for thorough analysis of their results. The composite results, while somewhat interesting, are not of great value because the details of the durability tests being compared (materials set, test protocol, stack design) are not available.

### Question 2: Approach to performing the work

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

- The website is a good idea.

- The project features an excellent scheme and database where the all of the data are consolidated and easy to access.
- The project has a good approach and is excellent with respect to budget effectiveness—it leverages already available tools and obtains data from developers at no cost. It provides outstanding "bang for the buck."
- The approach is sound and aimed at getting progressively higher levels of information through the years and comparing field data to laboratory data. Data is aggregated to promote distribution without infringing on intellectual property (IP).
- The National Renewable Energy Laboratory's (NREL's) approach to measure and project time to 10% voltage loss and a high power point is reasonable. There are many limits to the approach, one of which is that the electric current that NREL uses to track degradation is not based on the design point of the stack/system. Thus, a current is selected that was able to run during the entire durability test even though it is quite possible that life would be much shorter at the actual designed peak power point. Also, the root cause of the degradation mechanism cannot be captured using this approach.
- Although this is beyond the control of the project, the project only collects the data that industry partners voluntarily provide. On one hand, the project data constitute an existence proof. However, this methodology creates a selection bias that may tend to overstate the readiness of fuel cells for field service. Furthermore, data from laboratory tests have a bit less relevance than would data from field tests, where, for example, environmental effects come into play.
- Life projection based on an available data scatter is tough, and this project has leveraged its previous work to make sense of seemingly random data. The electrode life is projected near the maximum power level; it is unclear if the degradation rate at lower power (25% of maximum) and the corresponding life projection is different.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- From last year, the number of data included has increased significantly. Data has been properly "sliced" to understand projected life as a function of stack type (short, full-scale, in system) and cycle type (steady, cyclic, accelerated). A significant gap between field testing and laboratory testing has been identified. The task of finding correlations between field and laboratory data will be very challenging.
- NREL did a nice job of breaking down the results from different platforms, test configurations, and test protocols. One new development in 2012 is the breakdown among tests run using steady-state, duty-cycle, and accelerated protocols. Otherwise, this project seems to continue to do similar analyses using the same methodology as it has done in years past. As NREL compiles more results over the years, the comparative analyses are becoming more informative.
- Within the limitations discussed in the previous two questions, the presentation demonstrates that the project is being executed competently. The project is accumulating a growing volume of data, the data are accessible through Internet-based tools, and the results are well integrated with other technology validation activities.
- The team has made significant progress in summarizing new data and creating histogram plots. It might be helpful if DOE could leverage other original equipment manufacturers (OEMs) to share data to increase the impact of this work.
- While the data in slides 14 and 15 are very useful for the Program, the project needs to include more data sets to be of greater value. The investigators should plan to incorporate a cost factor if possible, because durability and cost targets need to be met simultaneously. At a minimum, a catalyst loading and membrane thickness should be noted, if possible. In addition to 10% voltage degradation, membrane failures should be tracked.
- The technical progress is good. Although it is a tough challenge, the data from the laboratory have been qualitatively correlated with the real-world data gradually. Semi-quantification of their correlation is expected in the next step. The accuracy of decay prediction is to be enhanced more.
- There is a substantial amount of new data since last year. The new website is great; however, it would be outstanding if more developers and more applications (e.g., buses) can be included.

### Question 4: Collaboration and coordination with other institutions

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

- The project features good outreach to developers.
- This project is inherently collaborative. It gathers performance data from fuel cell developers and publishes its results openly.
- Ten of 22 fuel cell developers provided data. The project team should strive to increase this number. The website is an effective way to collect feedback from companies abroad and in the United States, and it should be better publicized.
- NREL has been collecting data and providing analysis from 10 fuel cell developers. It would be nice to see some testimonials (even if they are anonymous) to get a feel for how much value the developers get from this type of analysis.
- It might help to reach out to foreign OEMs to gauge the level of progress in other parts of the world.
- A further increase of collaborators, especially fuel cell developers, is expected in terms of validity of the analysis.
- The fact that 50% of fuel cell developers are participating voluntarily is good, but even higher participation rates should be the goal. The project has established an excellent reputation for confidentiality, so developers that have chosen not to participate should be revisited. Also, the team should explore collaborators outside the United States.

### Question 5: Proposed future work

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

- The proposed future work (slide 18) is well considered.
- There are good plans for the budget.
- Continuing the project is a good plan, but the team should be more aggressive about getting more participants for more field data.
- Cultivating existing collaborations is perceived as an important way to increase further the relevance of the project to the whole industry. The investigation of the decay modes goes beyond the project objectives, and it probably should be addressed by a larger team.
- Some of the future planned work could certainly improve the value of the project. NREL plans to specifically analyze stacks and cells run using DOE durability protocols. The most value should come from NREL's plans to investigate specific aging parameters on fuel cell durability, including start/stop and soak time. For this type of analysis to truly be of value to others besides the fuel cell developers that share their data, more details about the operating conditions must be disclosed.
- It would be useful to think about if there are any membrane-level diagnostics within the existing data set to gauge the life of the membrane in real-world applications. It will be useful to show a summary of the rated power density ($W/cm^2$) of the aggregated fleet and compare it to the DOE target of >1 $W/cm^2$.
- The future plan is basically acceptable. Voltage loss should be predicted not only for the rated current density, but also for other current densities, such as low and maximum current densities. In automotive use, low current density is average in general use, and maximum power corresponds to hill climbing. Therefore, various points are necessary for results that approximate "real world."

### Project strengths:

- Strengths of the project include its sound approach, how the website allows everyone to provide information, and its focus on IP protection.
- NREL is uniquely set up to compare durability data sets from a variety of fuel cell developers for a range of applications. Without this project, such comparative analysis would not be available.
- The project provides performance benchmarks and situational awareness for the Program on technology readiness. The openness of the project and its integration with other technology validation projects are pluses.
- The project provides good value to DOE for the money.
- Strengths of the project include the theme, real-world data analysis, scheme, and network.

- The project features excellent leveraging of existing assets and obtaining data for “free.” There is good dissemination of results, especially with the implementation of the website.

### Project weaknesses:

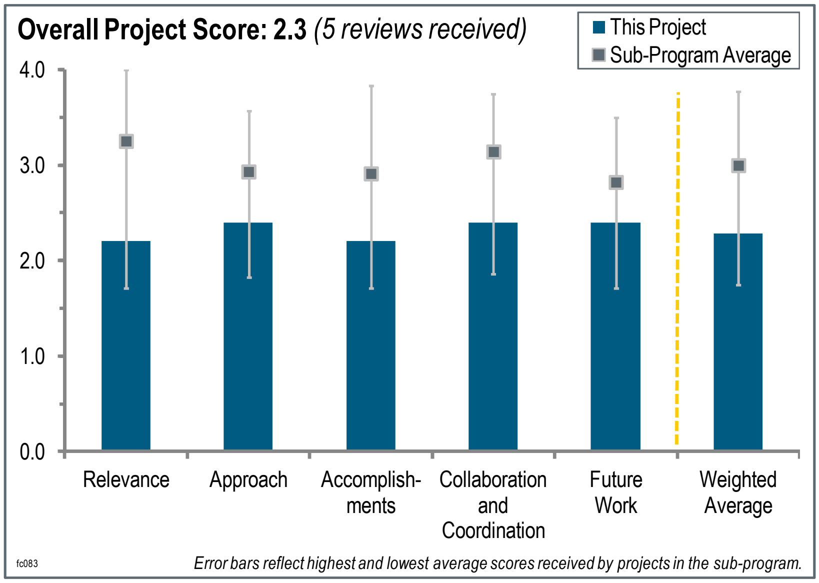
- The task of finding a correlation between field data and laboratory data is very challenging. The pool of data providers is still limited.
- The accuracy of the decay prediction is an area of weakness.
- There is not enough field data; the team needs more in order to add more applications and more results on current applications that are already being reported on.
- A lack of detailed information about the durability tests (operating conditions, load cycles, material sets, and stack design) renders the composite analysis of little value to those besides the fuel cell developers whose data is being analyzed. Of course, it is unlikely that this will change, because developers prefer not to disclose such information.
- The project is inherently limited by selection bias and by the unknown differences between the degradation of fuel cells undergoing laboratory tests and that of those in field service.
- The project is entirely dependent on voluntary contributions, and hence the data may not be from the current generation of hardware.
- Contacting developers to get data is an area of weakness. A bottleneck occurs because participation is voluntary. Maybe the DOE-funded projects can be pressured into providing the data as part of their work scope. This is outside the scope of the project’s principal investigator, but it may be accomplished with support from the Program.

### Recommendations for additions/deletions to project scope:

- Feedback of better accelerated stress test protocols and other requirements based on real-world data.
- In order to estimate the difference between laboratory data and field data, it would be helpful to the Program to collect data on environmental conditions (air quality, temperature range, etc.).
- There is potential for much more information to be gleaned from analyses of the provided data sets. For example, if NREL could report trends of durability with events (e.g., voltage cycles) or operating conditions (e.g., average temperature), that could provide valuable information to the fuel cell community.
- The team should solicit data sets from outside of the United States (e.g., Japan for stationary applications). It should be noted that many of these Japanese systems include U.S. stack technologies. Foreign fuel cell developers who attend the Program’s Annual Merit Review meeting should feel “guilty” unless they participate. The team should also add transit bus applications. If there are not enough different bus fuel cell developers to provide anonymity, the team should ask fuel cell developers if they would be willing to share data without anonymity.

# Project # FC-083: Enlarging Potential National Penetration for Stationary Fuel Cells Through System Design Optimization

Chris Ainscough; National Renewable Energy Laboratory




**Brief Summary of Project:**

The main objective of this project is to build a modeling tool to optimize fuel cell attributes, including control parameters, and system and component sizes for unique individual building characteristics. The tool will add user flexibility for different building, fuel cell, financial, and control characteristics. The tool can be used to minimize life-cycle cost, lifetime greenhouse gas (GHG) emissions, and installed capital costs of fuel cell installations. The project will characterize the largest segments of the U.S. building inventory for use in the tool, leveraging the Commercial Building Energy Consumption Survey (CBECS); characterize building control systems and advanced control strategies for integrating fuel cell systems and building control systems; validate the model outputs against real-world data from stationary fuel cell installations; and determine the set of most-favorable system sizes and types to achieve national GHG emissions and energy demand reductions.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.2** for its relevance to DOE objectives.

- This project is relevant to DOE’s goal of developing fuel cells for combined heat and power (CHP) applications. The stated objective is to build a tool for optimizing fuel cell attributes, including control parameters and system and component sizes for buildings. The scope of the tool extends to minimizing the life-cycle costs, lifetime GHG emissions, or capital costs of fuel cell installations.
- This is a very useful tool for market studies, as well as for fuel cell original equipment manufacturers (OEMs) that are evaluating potential customers.
- The relevance and how the project will support the deployment of fuel cell technology for CHP are not clear. There are no industrial CHP partners.
- There is always a great need for tools of this type to help end users and developers understand how fuel cells can aid them. For this work to be useful, it must be validated against real-world data, the assumptions used in the models must be clearly specified, and the model’s limitations must be clearly explained.
- The project has some relevance to the DOE Hydrogen and Fuel Cell Technologies Program goal of fostering the commercialization of fuel cells in stationary/CHP applications. This project appears to fit better with the Market Transformation sub-program or even the Systems Analysis sub-program. It is not clear how completion of the model will aid in system optimization unless specifics on various design approaches are included in the model. It appears that the model is at a higher level and not specific to any one design that can be optimized.

## Question 2: Approach to performing the work

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

- Working closely with buildings experts and leveraging other work by the National Renewable Energy Laboratory (NREL) for financial calculations is a good approach.

- One "grand unifying model" for all community buildings is appealing. However, the validity of the model and approach was unclear, given the high level of uncertainty on estimates and the number of unknown variables.
- The project has taken the approach to develop a high-level model that captures the interactions between the building loads and the fuel cells. The approach is clearly spelled out and benefits from work being carried out in other activities. In developing the Buildings Module of the model, the project team is integrating the building-related data (building types, locations, vintages) that are available from the NREL Electricity, Resources, and Building Systems Integration Center (ERBIC) and the 2003 CBECS. The project has University of California at Irvine (UC-Irvine) as a partner to develop the Control Strategies Module for refining and validating dispatch strategies. The Manufacturing Costs Module will be developed using the data developed by Lawrence Berkeley National Laboratory (LBNL) and Strategic Analysis, Inc. (SA) in related projects. The Economic Module is based on the financial calculations method implemented in the Hydrogen Analysis (H2A) model. The model has a Grid Pricing Module to reflect the differential prices in winter and summer as well as peak and off-peak times. The Natural Gas Pricing Module uses data from the U.S. Energy Information Administration forecasts.
- The approach of using NREL's buildings program is extremely important for this work to be successful. It is not clear how the heat is being utilized. The electricity rates are clearly set up. The heat value and demand were not described. Ramping the reformers for load following and the solid oxide fuel cell itself will dramatically impact the lifetime of the reformers and stacks. This needs to be considered. It is not clear how operations and maintenance costs are considered. This will be a large cost for the system, particularly for a system that is constantly ramping.
- The model will include fuel cell characteristics such as minimum and maximum power, temperature of available waste heat, and a ramp rate that determine the power and heat available in the next time increment. The partners will partner with UC-Irvine to develop and validate dispatch strategies for various classes of buildings. It is important that the model does not demand that the fuel cell be cycled on and off over short time intervals. Without significant tailoring of the submodels by each developer, the utility of the model may be limited to a high-level view of where a specific fuel cell system may be a good fit, and not necessarily a design optimization tool. It is not clear if cooling loads are being considered, which may be a better fit for a significant portion of the building stock in the United States. The approach emphasizes relatively small CHP systems for which there are not a lot of existing systems with which to validate the model. A few larger-scale systems exist, but their relevance to smaller systems is unknown.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.2** for its accomplishments and progress.

- The project is following the project description; however, the project fails to communicate and demonstrate how this supports DOE goals.
- It seems like there has been significant progress in the development of the tool, but the most important step is validation against real-world data.
- The investigators have analyzed many different building types and processes. The model appears to have a great deal of flexibility, which is necessary. Clear documentation on how to use this flexibility will be needed.
- In fiscal year (FY) 2012, NREL has developed a graphical user interface; incorporated models for phosphoric acid, molten carbonate, polymer electrolyte membrane, and solid oxide fuel cells of different power ratings; and laid out a framework for accepting manufacturing cost data. The project is still in the process of developing the modules and did not discuss specific results.
- The actual start date of the project is not clear, even though it is listed as October 2011. If this is so, progress appears reasonable in the relatively short period since the project's beginning. However, the same comment appeared in the reviewer comments from the 2010 DOE Hydrogen and Fuel Cells Program Annual Merit Review. So neither the start date nor the end date is clear. It is unclear when the model will be available to fuel cell system developers. Some progress is evident, primarily arising from input from other existing projects.

### Question 4: Collaboration and coordination with other institutions

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

- The project features good collaboration with other offices at NREL, UC-Irvine, and LBNL.
- There is a mix of industry and national laboratories that would be able to provide validation of the model output.
- It is unclear whether the approach has been accepted by fuel cell CHP companies.
- The principal investigator (PI) listed NREL ERBIC, LBNL, SA, and Battelle as collaborators, and UC-Irvine as a partner. The LBNL (FC-098), SA, and Battelle efforts are new initiatives. The LBNL and Battelle projects are still formulating models and have not yet contributed data to the NREL project.
- The collaboration appears to be good. The project team is working with NREL ERBIC, UC-Irvine, LBNL, SA, and Battelle, as well as fuel cell system OEMs. These interactions should enable the project to take advantage of existing work and avoid duplication of effort.

### Question 5: Proposed future work

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

- It will be exciting to see the results of model validation against real-world data, and model publication.
- The project seems just to continue the ongoing model work without addressing or improving the relevance, approach, or collaboration.
- The remaining proposed future work for FY 2012 calls for expanding the control strategies and building types, providing input to the 2012 CBECS, and generating initial estimates of fuel cell sizes and building inventory for use by the new LBNL and Battelle projects. The proposed work for FY 2013 includes expanding fuel cell types, optionally continuing UC-Irvine work, implementing Design of Experiments capability and speed improvements to dispatcher code, validating models, and performing detailed optimizations.
- The future work plans are clear and cover the areas needed for a successful project. It is not clear how much additional effort and time are required to produce a product that can be used by developers. It would be good if a fuel cell system developer could exercise the parts of the model that exist and provide feedback on the implementation of the submodels into the overall model.

### Project strengths:

- The approach and plans are well laid out.
- The approach and plans are generally well laid out.
- Working closely with buildings experts and leveraging other work by NREL for financial calculations is a good approach.
- This is showing good collaboration between DOE offices. The investigators are leveraging a lot of work being done in the DOE Building Technologies Program with the fuel cell work.

### Project weaknesses:

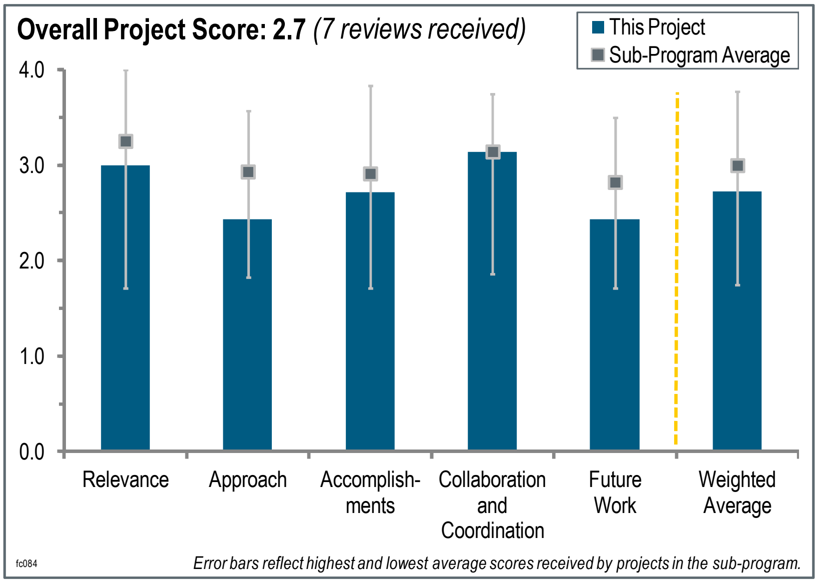
- At best, the project will produce a high-level model that is likely to be more useful for the system analysis team than the fuel cell team. To date, the project has not presented many useful results. The PI plans to generate some rough estimates later this year to focus the LBNL cost efforts. The first useful results are likely to come in FY 2013.
- It would be nice to clearly show the heat requirements and, perhaps, to allow the user to set the heat duty similar to how the user can define the electrical needs.
- The model appears to be suited more for a high-level or policy analysis rather than a design optimization tool. Its utility to fuel cell system developers is unclear. A collection of building energy needs and patterns may be more helpful to developers; they can put in their own system designs and characteristics.

### Recommendations for additions/deletions to project scope:

- The investigators should add fuel cell system developers to the team. Maybe they should just concentrate on compiling building characteristics for use by system developers.
- The project should be terminated if the approach is not acknowledged and supported by the companies making CHP systems based on fuel cells (e.g., UTC Power, Ballard, and ClearEdge Power). These companies should also be the ones that define the major challenges for the deployment of CHP in commercial buildings, and the project should address and analyze these challenges.

## Project # FC-084: WO3 and HPA Based Systems for Durable Platinum Catalysts in PEMFC Cathodes

John Turner; National Renewable Energy Laboratory




### Brief Summary of Project:

The objective of this project is to improve electrocatalyst, membrane electrode assembly (MEA) durability, and activity through the use of platinum/tungsten oxide (Pt/$WO_3$) and heteropoly acid (HPA) modification to approach automotive polymer electrolyte membrane (PEM) fuel cell activity (0.44 mA/mg of Pt) and durability targets (5,000 hours/10 years). The project seeks to: (1) enhance Pt anchoring to the support, thereby suppressing Pt electrocatalytic activity loss under load cycling operations and enhancing electrocatalytic activity; and (2) lower support corrosion for increased durability under automotive start-up/shutdown operation and reduced Pt agglomeration and electrode degradation.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The project is focusing on addressing important barriers, using its proposed catalyst supports.
- The goal of catalyst durability and performance is addressed in this research.
- These objectives do address the stated DOE objectives. It is not clear, however, how the performance is to be improved by these two approaches.
- The durability of cathode catalyst support materials is one key in the quest to fully implement PEM fuel cell technology. As the principal investigators (PIs) proposed, one way to overcome carbon limitations would be to use conductive metal oxide supports such as $WO_3$ and $SnO_2$. Although metal oxide supports have potential, the PIs provided little evidence that the type of oxide they are developing is the right way to go.
- This project addresses DOE targets and goals and is especially focused on durability enhancement. The project evaluates a candidate for robust catalyst supports as a way to replace the widely used carbon supports, which are known to corrode. This could turn out to be an important contribution to achieving durability and performance targets.
- Goals for the project are well aligned, but the approaches used may not be. For example, it is unclear if atomic layer deposition (ALD) will ever be scalable to mass production at a reasonable cost. The presentation discussed a "large-scale" ALD process, but the process still needs improvement and a cost projection. Both $WO_3$ and HPA systems appear to add several steps to the manufacturing process.
- Because the project is intended to enhance catalyst activity and durability, the relevance of the project is high. The greatest strides that can be made toward commercializing fuel cell vehicles are those that can be made by lowering the loading of precious metal electrocatalysts and improving durability.

## Question 2: Approach to performing the work

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

- Anchoring the Pt onto the support may impact durability, but it is unclear how this will improve performance. The poor conductivity of $WO_3$ is known, but it does not seem to have been considered by the PI. This will make it difficult to reach baseline performance, as indeed shown in this review.
- This research has the potential to increase catalyst durability. There does not seem to be a way in this research to address the barriers of cost and performance. If anything, these materials so far seem to show decreased performance and increased cost to state-of-the-art materials. Because the support is not conductive enough, carbon is needed in the ink. This adds further levels of complication to creating proper Pt/carbon interaction above that of traditional inks.
- The project is well planned and each participant represents an important contributor to the overall success of the project. However, it is not clear if the PIs developed the best method for establishing the active surface area. Due to a problem with CO oxidation at low potentials, the CO-stripping approach should be avoided. Although utilization of copper (Cu) UPD may overcome some of the limitations; the assessment of charge from Cu UPD stripping peaks is usually not easy and the method should be revisited.
- There are considerable synthesis and measurement tasks involved. The laboratory tasks seemed a little “casual.” There was no apparent attempt to replicate work and to demonstrate the repeatability of the results. It is true that substoichiometric oxides of heavy metals (such as tungsten) are hard to characterize. The chemistry is complex. But that complexity requires a well-defined set of replicated tasks with thoughtful control of contaminants and process conditions.
- It is clear that some progress has been made, but the overall approach is still questionable. Given that carbon corrosion is a significant barrier to lifetimes, it is unclear if it makes more sense to completely eliminate the carbon in favor of an oxide support (as is being pursued by other teams that presented in the meeting). The half-way approach used here of deposited Pt on an oxide, but then mixing the oxide/Pt catalyst with carbon, may eliminate or reduce some pathways to failure; however, the carbon will still see very high potentials during transients/start-up/shutdown, and it will still corrode, leading to failure. The Pt is intact on the $WO_3$, but it is electrically isolated because the carbon-conductive path is gone, and it is not going to work. The team has looked into using more corrosion-resistant carbon nanofibers. It is hard to determine the benefit of adding the oxide support if a more corrosion-resistant carbon is also required.
- ALD seems to be an expensive technique for producing nanoparticles. Usually the cost of ALD seems most worthwhile when coupled with the anticipation of producing higher specific activity thin films. Pt/$SnO_2$ has been tried before, and it is noted in the literature. The approach to this project should clearly state how the Pt/$SnO_2$ systems studied here are differentiated from what has already been explored. The usage of HPA is potentially risky due to the possibility of leaching; however, the reward could be high if HPAs help to stabilize other components. Pt nanoparticles on various forms of $WO_3$ have been tried in the literature. It would be more interesting if something could be done to put Pt conformally on the $WO_3$.
- The approach may have an opportunity to improve the durability, but the support conductivity issue may limit its activity. If the conductivity could be improved by the proposed approach, then the durability may become an issue again. This is a general problem, though. However, the project has not come up with a unique solution so far.

## Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.7** for its accomplishments and progress.

- Synthesis has been accomplished and performance and durability have been assessed.
- Progress has been shown in the synthesis and feasibility of new supports. Some progress has been made on showing increased durability using new supports. No data was presented to reduce cost or increase performance.
- These electrodes could be very interesting. The chemistry has proven complicated and somewhat difficult to understand fully. However, progress is being made as the synthesized materials are characterized.
- There still seems to be too many approaches and not enough capability to look at each one successfully. It was very surprising to see the $SnO_2$ work; this was a distraction.

- The progress in this project is successfully presented and participants have clearly demonstrated a strong team effort that has been capable of efficiently addressing DOE Hydrogen and Fuel Cells Program objectives. However, there is neither comprehensive discussion about the conductivity of oxides nor a critical charge transfer from oxide to Pt nanoparticles. These results would allow better understanding of key parameters that are responsible for the improved stability of cathode materials.
- The graphitized carbon nanofibers were used to increase the conductivity of $SnO_2$ and $WO_3$. They also assisted with improving $SnO_2$ activity. The major issue with HPAs is usually with respect to avoiding washout in the presence of water. The investigators managed to immobilize HPAs on carbon and demonstrated that the HPAs did not boil off the carbon. The activity for most materials did not meet the targets. The activity reported for materials with HPA was within noise of the baseline. Poor activity was shown for $WO_3$. Some stability was shown for HPA- and $SnO_2$-containing materials. Other studies (Masao et al., 2009) have noted $Pt/SnO_2$ stability at higher potentials. Just like with alloy catalysts that contain base metals, it would be good to see whether the stability is maintained at lower potentials.
- The ALD approach provides good Pt precursor deposition on the support; however, the final Pt particle size and its distribution do not show significant advantages over the colloid approach. Meanwhile, the colloid approach is very practical and scalable.

### Question 4: Collaboration and coordination with other institutions

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

- The comprehensive collaboration in this project may result in promising progress.
- A good mix of key industry partners and academia has been established.
- Little collaboration has been shown with industrial partners.
- The team involving the National Renewable Energy Laboratory (NREL) and university researchers seems fully functional. Tasks are assigned.
- The future role of TKK looks to be very important regarding finding answers to questions of scalability and cost. The university interactions look well coordinated.
- The achieved accomplishments from this project indicate a well-balanced, synergistic effort among participants. Indeed, the success of this project relies on the highly diverse and organized team, which is well coordinated by NREL as a lead institution and John Turner as a lead PI. The role of the participants can be recognized by their contributions in their own expertise that add to the overall success of the project.
- There appears to be clear evidence of how most collaborators are contributing to the project. Nissan has contributed relevant accelerated stress test (AST) protocols. Fuel cell testing is still to come. The Colorado School of Mines has contributed perhaps the most interesting aspect of the project: the immobilization of HPAs on carbon. 3M's contributions are not very clear, although this may be a consequence of the fact that fuel cell testing has not begun in earnest. The University of Colorado-Boulder has contributed the ALD work for $Pt/WO_3$.

### Question 5: Proposed future work

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

- There seems to be too great an emphasis on $WO_3$ rather than HPA, despite significant progress in this area. It is unclear if there is a potential to continue the $Pt/SnO_2$ work with TKK. It looks promising.
- The future work is focused on the durability barrier. Cost and performance are not addressed beyond using alloy catalysts.
- In this phase of the project, the PIs should have a more aggressive approach in designing a new generation of conductive and durable metal oxide supports. The PIs may want to go beyond $WO_3$.
- The "future" was not clearly defined. There was an indication that one task was to "scale-up" ALD, but the details of the proposed route to do that were not described.
- The ALD approach cannot be a viable way to make a catalyst that meets DOE goals.
- Regarding item 2, both the ALD and wet chemistry approaches still need significant work. A choice is to be made in December, which is only six months away. It is unclear whether the project team will actually make a decision in December, or if it will be argued that both approaches are still too immature for a decision to be

made. The team should stick to its plan and down-select. Yes, it is possible that down-selecting could be a mistake, but failure to do so will prevent real progress. The team should drop item 3. Until there is significantly more evidence in both rotating disk electrode (RDE) and MEA testing of the benefits of this approach using Pt, the team should not waste time looking at Pt alloys. The reviewer agrees with items 1 and 4.

- While some conductivity gains have been shown with the addition of the graphitized carbon nanofibers, mass activity is still not at the Pt/carbon baseline. Given this, it would be interesting to understand what remains in the systematic conductivity study. Because other work has produced higher activity for Pt/$WO_3$, it might make sense to reproduce some of the other work, and be able to draw from it metrics associated with conductivity. Decisions should certainly be made soon about whether to continue with Pt/$WO_3$. HPA-containing materials should enter into cells, just as described in the future work. Other scale-up efforts may not be as worthwhile due to low activity. It may be worthwhile to put 12% Pt/$SnO_2$+C into a cell for durability testing.

## Project strengths:

- The project has put together a good team that has various strengths.
- Key industrial partners suggest that materials that show promise can be commercialized rapidly. There is a good balance between national laboratory, university, and industry efforts.
- New supports may lead to more-durable catalysts. The project team is taking time to make sure it has adequate electrochemical tests to measure progress toward the barriers.
- The development of durable (but not optimized) metal oxide supports has been demonstrated. Improved wet chemistry Pt deposition and ALD deposition seems to be applicable on $WO_3$ systems. The supports have shown modest improvements in durability over Pt/C catalysts, which clearly justifies the approach of this project. Highly collaborative team efforts have produced a valuable outcome from this project. The participants are well-known experts in the field of fuel cells, and they bring highly diverse expertise.
- It is important to keep looking for tougher catalyst supports, and "non carbon supports" may be necessary. This project is seeking such materials. It makes sense to keep that search active. The PI is excellent and has a good comprehension of the tools needed to make progress.
- The use of HPA-immobilized materials may be interesting to study from a durability perspective. The presence of Nissan provides for automotive-relevant ASTs. RDE methods appear to be well established. Some measure of durability has been shown for materials containing HPA and $SnO_2$.

## Project weaknesses:

- The project does not directly address cost and performance barriers.
- Some clearer metrics and milestones might help.
- The team needs to improve the conductivity without leading to poor durability.
- It is unclear why so much time was wasted developing NREL's own test protocol. This just makes it difficult for others to compare these results with their own. That is why DOE has its own protocols. The lack of consideration of the commercial feasibility of the synthesis processes makes scale-up difficult.
- The PIs need to explain how, specifically, they plan to improve metal oxide performance, and what methods they will apply to deposit Pt-alloys. It is unclear which type of alloys would be used, as well as the atomic ratio between Pt and alloying components. The reviewer hopes this would not be PtCo or PtNi, because other groups are also focusing on the very same alloys. Furthermore, the $SnO_2$ system should not be taken into consideration. The method for assessing the active surface area should be improved.
- So much time was spent on synthesis development and technique development, when the fundamental benefit of the approach is still not clear. It is hard to see this route resulting in a real game-changing advance.
- The project is essentially using a conformal method (ALD) for applying nanoparticles to a support. The project is looking at a number of catalyst systems―Pt nanoparticles on both $WO_3$ and $SnO_2$—that have already been explored in the literature. High oxygen reduction activity has not yet been realized for any of the materials.

## Recommendations for additions/deletions to project scope:

- The ALD approach should be carefully evaluated. It should not be encouraged.
- The project should quickly move to either develop Pt/$WO_3$ that resembles literature activity or eliminate this aspect of the project. Fuel cell durability should be immediately studied for catalyzed HPA and $SnO_2$ supports.

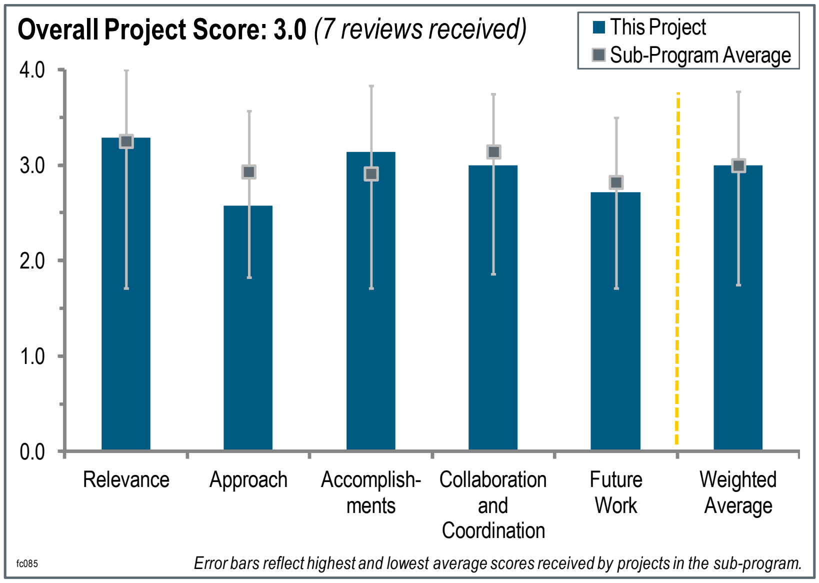
- If there is little hope to increase activity over current materials, the future work for 2013 should be pushed forward to use higher mass activity catalysts and see if they can be incorporated onto support structures.
- The team should continue with the Pt/$SnO_2$ work. Perhaps the ALD scale-up should not be done, because it is not really going to scale-up to industrial volumes. The team should broaden the HPA work in order to understand the protonic conduction of this material and seek to see if ionomer can be reduced within the catalyst layer or if Pt utilization can be increased.
- The team should take one of the $WO_3$ synthesis routes, focus on finding a way to deposit Pt on it with comparable mass activity to baseline catalyst, and demonstrate enhanced stability with that system. The HPA approach is still questionable; it is unclear whether Pt is on HPA or on carbon. Combining HPA with $WO_3$, when neither is well understood, is not likely to be successful.

# Project # FC-085: Synthesis and Characterization of Mixed-Conducting Corrosion Resistant Oxide Supports

Vijay Ramani; Illinois Institute of Technology

**Brief Summary of Project:**

Research objectives of this project are: (1) to develop and optimize non-carbon mixed conducting materials with high corrosion resistance, high surface area, and high proton and electron conductivity; and (2) concomitantly facilitate the lowering of ionomer loading in the electrode through enhanced performance and durability and by virtue of the surface proton conductivity of the electrocatalyst support. Addressing the issue of electrocatalyst and non-carbon support stability will help meet operational longevity, electrochemical area loss, and electrocatalyst support loss.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The project goal of developing a corrosion-resistant catalyst support is important to reaching the overall DOE objectives of implementing automotive fuel cell power systems.
- Improving support corrosion will have meaningful impacts on fuel cell durability and issues involved with start/stop cycling.
- This project addresses fuel cell durability, which is a major barrier to the adoption of fuel cell technology. The focus on catalyst support durability is relevant to automotive systems, which are subject to multiple starts/stops that corrode current catalyst supports. This support corrosion has been identified as a major source of performance degradation.
- The stability of catalysts/supports under polymer electrolyte membrane (PEM) fuel cell operating conditions involves complex processes that need to be resolved in order to fully implement PEM fuel cell technology. The work related to establishing the stability of catalysts on metal oxide supports will definitively be one important step in finding a new class of more durable cathode materials.
- Carbon corrosion is a real problem, and replacing the carbon is one of the few solutions. However, modifications of start/stop procedures and the use of catalysts such as nanostructured thin film are making it less relevant.
- This project started in September 2010 and is now technically in its second year. The principal objective of this project was to develop a stable support material for PEM fuel cell operation. The benchmark for this was set as durability measurements, which would reflect lower than 40% ECA loss or less than 30 mV per 100 hours tested at 1.2 V, both in accordance to General Motors' protocol. The project aims to do this using model mixed metal oxide (MMO) supports principally comprising $RuO_2$-$SiO_2$ (with and without sulfonic acid functionalization), $RuO_2$-$TiO_2$, and $SO_4^{2-}/SnO_2$ materials. Its relevance to the DOE Hydrogen and Fuel Cells Program is good, considering the fact that currently very few carbon supports are able to meet the DOE targets for corrosion resistance.
- The most pressing need toward meeting cost and durability targets for the commercialization of hydrogen ($H_2$) fuel cell electric vehicles is the development of high-activity, durable electrocatalysts for oxygen reduction. The project seeks to provide durable electrocatalysts through the use of metal oxide supports. In terms of cost, the

project seeks to lower the use of ionomer by adding proton-conducting species to the catalyst support. This is not likely to be as effective as developing higher activity.

### Question 2: Approach to performing the work

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

- Work is directly aimed at addressing start/stop durability, which is a critical barrier. Testing methods directly address stop/start issues. The approach of using nonconducting, inexpensive $SiO_2$ and decorating it with conducting oxide particles made from more expensive materials will decrease costs compared to using the conducting oxides alone, and it may allow costs to be competitive with carbon if durability is improved. Utilizing a proof-of-concept with the $RuO_2$-containing system and the use of alternative cheaper conducting oxides (indium tin oxide [ITO]) is a good strategy that mitigates risk if high amounts of $RuO_2$ are needed. Durability testing using separate start/stop cycling protocols (1.0–1.6 V) and operational cycling protocols (0.6–1.0 V) do not take into account that the system operates between start/stops and that the catalyst surface oxidation state is changed, which may impact start/stop durability. In the start/stop protocol, the platinum (Pt) surface is always oxidized. Others have seen an effect from including a cycle going to lower voltage during stop/start cycling, and it may be important to include such a cycle in catalyst support testing. When testing alternative catalyst supports, catalyst-to-support mass ratios generally have been kept the same as for Pt/C. Due to the higher density of metal oxide supports, this will result in thinner catalyst layers and is likely to result in poorer mass transport and flooding under some conditions. It would be better to keep the volumetric ratio of support to catalyst constant to provide a better comparison to Pt/C, because the catalyst thickness would be more comparable and mass transport issues should then be more similar. The Pt-support mass ratio does not relate to a physical parameter that affects performance, so there is no reason to use this as a parameter for standardization.
- The principal investigators (PIs) proposed the synthesis of "model" MMO supports that would be used as a model support to investigate the feasibility of preparing non-carbon mixed/conducting supports. The approach, involving transformations of knowledge from model oxides to real oxide-based supports is, in principle, desirable. However, $RuO_2$- and $SnO_2$-type oxides have already been studied in electrochemistry. In order to learn more, the PIs must develop and use new characterization methods.
- The investigators are taking the logical path by proving the stability of the base material first; trying different base materials; and then trying to improve the catalyst material by controlling catalyst particle size, etc. Catalyst stability is the next biggest question, and the PIs are addressing that with the proper testing. At the end of last year there were a lot of comments on the use of ruthenium (Ru) as a support, which the investigators vigorously defended; however, in a completely qualitative way. They should have better prepared for this question; there is great skepticism about whether RuOx is a suitable support, so they need to first convince people of this. The authors also mentioned that they are using RuOx as a template to explore other metals, titanium (Ti) for example, but those do not seem to be working very well.
- The approach taken involves using MMO supports, which engender mixed proton and electron conductivity while maintaining high resistance to corrosion. In order to achieve this slated objective, the project is using $RuO_2/SiO_2$ composites with and without sulfonic acid functionalization, as well as $RuO_2/TiO_2$ and $SO_4^{2-}/SnO_2$ composites. The principal issue in the approach is that a comparison is being made between an MMO support and a typical carbon black with similar BET surface areas, but different particle size as evidenced by XRD Scherrer calculations. Because most of the Pt deposited on the MMO was larger, its ability to withstand catalyst dissolution tests better is not surprising. Careful comparison between Pt of similar particle size would be necessary to remove this artifact. This is evident from the similar loss of ECA between Pt/$RuO_2$-$SiO_2$ (1:0.5), which has particle size in the same range as 46% Pt/C (TKK). What was most interesting was the start/stop cycling results, wherein despite the potential excursion to 1.5 V (compared to 1.0 V in the catalyst dissolution test), the loss of ECA was lower. This clearly indicates that wetting of the larger Pt particles on the MMO was significantly better as compared to smaller-particle-size conventional 46% Pt/C (TKK). As a whole, in terms of approach, it is risky to use a transition metal that has the potential of electrodepositing onto Pt within the operating window of fuel cell operation. All of the data presented in this work does not simulate conditions in a fuel cell, especially during uncontrolled shutdown conditions where variations of reactant gas fronts create potential fluctuations not simulated in this study.
- The approach is to use stable but not conductive components, and functionalize/coat them with conductive components. The concept is good, but unfortunately it has only been demonstrated with expensive materials

($RuO_2$) that also can corrode at the relevant high potentials. While the PI states in his rebuttal to last year's reviewer comments that $RuO_2$ is only a model system, the effort being put into this model system should be directed toward systems with the potential to meet the cost targets. It is promising that an automotive original equipment manufacturer (OEM) is performing cost analyses, but it would be nice if these cost analyses would be shared with the reviewers.

- The use of $RuO_2$ as a support material has cost and supply concerns, as noted by the presenter. While the presenter stated that $RuO_2$ is a model support material, it would seem to be a better approach to focus on the $SnO_2$ or ITO also proposed in the approach of the project. The importance of including ionic conductivity as an inherent property of the catalyst support is not clearly defined in terms of potential benefit; the target proton conductivity seems higher than it needs to be.
- The project has five different families of oxide supports. Of these five, three contain a precious metal (Ru), which may be a problem for cost. The project needs to incorporate a task for cost analysis. The project intends to thrift Ru by depositing $RuO_2$ onto a secondary support ($TiO_2$, $SiO_2$, or sulfonated $SiO_2$). There is some possibility that $RuO_2$ will not be stable for all of the potentials that a fuel cell cathode experiences. Sulfonating the catalyst support is an interesting development worth investigating. It may be beneficial for the rest of the program to understand if the 50 $m^2/g$ BET surface area requirement was cascaded from the program targets.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This work has made significant headway in a year in partnership with Nissan. However, actual fuel cell durability data is expected to determine the feasibility of this approach.
- The project has made good progress in the last year, both in Nissan's materials characterization and the Illinois Institute of Technology's (IIT's) materials synthesis. However, the focus should be on the development of cost-effective, stable materials rather than materials that have been shown to corrode in the potential range of the fuel cell cathode.
- Initial testing at Nissan North America showed much improved stability in stop/start cycling compared to carbon supports, showing only a 14% loss in mass activity (versus 55% for Pt/high surface area carbon)—approaching the DOE target of 10% mass activity loss in a test more severe than the DOE-suggested test. Initial results suggest that the sulfonated $SiO_2$-$RuO_2$ system has good protonic and electronic conductivity. A measurement technique is needed to demonstrate protonic conductivity in the presence of a good electronic conductor (i.e., after the $RuO_2$ is deposited). An $H_2$ pump experiment might be one option. The baseline is a high-surface-area carbon support, rather than a more stable carbon (graphitized). Better characterization of the morphology of the $SiO_2$-$RuO_2$ support would help direct optimization of the support. It was not clear how the size of the $RuO_2$ particles compared to the $SiO_2$ particles; that comparison will determine how much $RuO_2$ is needed to form an interpenetrating network among the non-conducting $SiO_2$ particles.
- While progress has been made in some areas, the proposed and accomplished work is still way out of balance. For example, methods have to be established for monitoring desolution of cationic species from supported oxides. It is well established that Ru is not stable in $RuO_2$ form; the same is most likely true for tin (Sn) in $SnO_2$. Furthermore, a big concern is the low conductivity of these oxides, especially in combination with $SiO_2$.
- Progress is in line with the investigators' plans; they have done a good job showing the stability of the supports, now they need to show that these can actually act as decent catalyst support materials. There is not much improvement in ECSA loss under cycling data. Not enough justification is given for the surface modifying the supports; perhaps proton conductivity to the supports has been shown to be a problem. In the researchers' own testing, it appears not to be. No work has been done on costing, and they should have seen this as a question.
- The materials synthesized to date have shown advantages in terms of durability to potential cycling in the carbon corrosion region compared to traditional Pt/C catalysts. The performance of these materials and their durability in the Pt dissolution region offer no improvements compared to baseline materials, which currently fall short of performance and durability targets and will be required for commercially competitive materials.
- There has been impressive stability for $RuO_2$-$SiO_2$- and $RuO_2$-$TiO_2$-based catalysts over cycling, especially with higher $SiO_2$ content for $RuO_2$-$SiO_2$ and higher-temperature heat treatment for $RuO_2$-$TiO_2$. It cannot necessarily be said that this stability was expected. (Although some loss of stability with catalyzation could be expected, as well as the lower stability with higher Ru content.) Investigators need to do cost analysis to confirm that Ru would not drive up costs. Investigators need to examine Ru on the anode post-mortem. Nearly

all activities need to be revisited following a reduction in Pt particle size. However, it is recognized that the true focus of this project is activity, not durability. That said, durability improvements cannot preclude needed activity.

### Question 4: Collaboration and coordination with other institutions

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

- Good collaboration is shown between Nissan and IIT.
- The collaboration between IIT and Nissan is clearly outstanding. Nissan is fully engaged in this project.
- The collaboration between Nissan and IIT appears to be working well. Nissan's testing should ensure that auto-relevant testing conditions are examined.
- Although good collaboration exists between the PIs, this proposal will benefit if a tried partner with appropriate characterization skills is involved in the realization of this project.
- The addition of Nissan should help, and it appears that Nissan will do more of the heavy lifting in the future.
- For a project of its size, the two key participants are adequate for the work scope. However, adding additional collaborators, particularly a catalyst supplier, would be beneficial.
- There appears to be just one collaborator: Nissan. Nissan is a good collaborator because it brings the automotive perspective to the work. Nissan helps with implementing accelerated stress tests and fuel cell testing. A few micrographs were shown for the $RuO_2$-$TiO_2$ catalysts—enough to show that $TiO_2$ was not completely covered by $RuO_2$. However, there is more that could be done to reveal aspects of structure, oxidation states, adsorbent species, coordination, etc. Past reviewer comments have questioned the project's access to materials characterization. While it appears the project has arranged for this at University of Illinois, Michigan State University, and University of Michigan, the results shown in this year's presentation reveal that more could still be done. Hopefully, this work is ongoing.

### Question 5: Proposed future work

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

- There are far too many unsubstantiated, declarative statements of *what* will be done, but no explanations of how and why.
- The researchers' plans are addressing the next concerns, and testing at Nissan should help. They will have to show that these supports can support smaller particle sizes to increase mass activity.
- The future plans appear to be very workmanlike. No serious approach is suggested to understand the observed results.
- The near-term focus should be on extending the concept to less expensive materials sets, with a secondary focus on catalyzation and functionalization with proton-conducting groups.
- Work optimizing Pt nanoparticle deposition is critical to obtain higher performance and to get an apples-to-apples comparison of durability, because durability is related to Pt particle size. Work optimizing the catalyst layer is important. The higher density of these supports suggests that a much lower Pt/support mass ratio should be optimum, and likewise that a much lower ionomer ratio should be optimum. A more detailed cost study may be beneficial.
- Further optimization of the Ru-containing supports with $SiO_2$ and $TiO_2$ may lead to incremental improvements in observed properties; however, the gains expected based on the data to date give little hope that materials that can reach DOE performance targets will be made. The focus on the optimization of Pt nanoparticles is unfortunate within the project, because this effort is better spent studying additional supports. A catalyst supplier would have been beneficial in this regard.
- The most important thing that the project has to do at this stage is to get the Pt particle size down, so as to make the activity acceptable. This appears to be covered under the bullet about optimizing the Pt introduction. At the moment, durability appears to be on a rotating disk electrode (RDE) basis, but this needs to be repeated in a cell for $RuO_2$-$SiO_2$ and $RuO_2$-$TiO_2$ catalysts. This work also appears to be going forward.

### Project strengths:

- Having an OEM as a partner is a strength of this project.
- The focus on the metal-oxide supports chosen is bold and compelling. There are some good examples of lessons learned from $RuO_2$.
- The project has shown the potential of using mixed proton-electron conducting element for improving support corrosion resistance. This is done using Ru-, Ti-, and Sn-based metals and silicon-based proton-conducting supports. Good corrosion resistance is shown for some of these model systems.
- One strength of this project is the collaboration with an automotive OEM with extensive experience in membrane electrode assembly (MEA) fabrication and testing.
- Corrosion-resistant supports have relevance, and studying mixed ionic/electronic-conducting materials is a novel area that has not seen much effort.
- The synthesis of new oxide supports appears to be a strong part of this project. Other strengths include the collaboration with an automotive OEM and the ability to perform RDE measurements and screen for durability.

### Project weaknesses:

- The focus on Ru-containing materials is an area of weakness.
- The development of new supports is rather slow, and the PIs may not have enough time to deliver the promised results. While the PIs had many of the needed skills, they did not have all of the skills necessary; it would be desirable to add the skills needed for characterization.
- Principal weaknesses are: (1) the use of Ru as a model; this is not only expensive, but it has dubious stability under potential cycling conditions of a fuel cell, especially the onerous start-up and shutdown steps; and (2) the approach does not compare similar systems for proper conclusions to be made. Particle sizes of Pt and the support BET surface areas are different; these differences could also be responsible for some of the observed results.
- The comparisons between the novel catalysts synthesized and the baseline Pt/C are unclear for relevance, particularly for cycling durability, because these tests are normalized for initial surface area and involve very different particle sizes. The presentation submitted for review contained 80 slides not including reviewer-only slides. The presentation itself was long and made focusing on key findings difficult. The supplemental slides, which were supposed to be limited to five, numbered 50 and were not reviewed thoroughly and could not be evaluated for potential impact or importance.
- Materials characterization to identify catalyst structure, oxidation states, etc. still remains a weakness. The premise of using a precious metal in the support will always raise some questions about cost. The current inability to avoid generating large nanoparticles of Pt has caused low mass activity measurements.

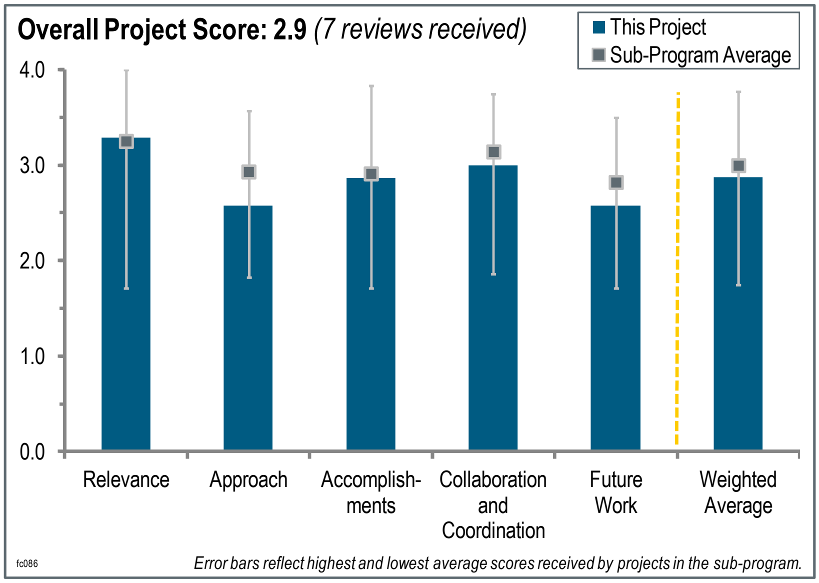
### Recommendations for additions/deletions to project scope:

- A more detailed cost study may be beneficial.
- The team should use graphitized carbon with larger Pt crystallites as reference to truly extract the role of the MMO in providing stability. Also, proper fuel cell durability tests need to be reported, especially with $H_2$/air operations under lower relative humidity conditions.
- The team should move its focus beyond $RuO_2$ supports. Side-by-side comparisons with Pt/C, including both graphitized and high-surface-area carbon supports with comparable Pt deposition, would be useful.
- For materials synthesis, there needs to be immediate emphasis on lowering the size of Pt nanoparticles for both $RuO_2$-$SiO_2$ and $RuO_2$-$TiO_2$ catalysts. Despite low activity, $RuO_2$-$SiO_2$ and $RuO_2$-$TiO_2$-based catalysts should be made into MEAs and durability testing should proceed in a cell. Although durability results from RDE are good, the relevant environment is an actual fuel cell.

## Project # FC-086: Development of Novel Non-Pt Group Metal Electrocatalysts for Proton Exchange Membrane Fuel Cell Applications

Sanjeev Mukerjee; Northeastern University

**Brief Summary of Project:**
This project will develop new classes of non-platinum-group metal (PGM) electrocatalysts that will meet or exceed U.S. Department of Energy (DOE) 2017 targets for activity and durability in fuel cells. This will enable decoupling of polymer electrolyte membrane (PEM) technology from platinum (Pt) resource availability and lower membrane electrode assembly (MEA) costs to less than or equal to $3/kW. The science of electrocatalysis will be extended from current state-of-the-art-supported noble metal catalysts to a wide array of reaction centers.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.3** for its relevance to DOE objectives.

- Developing electrocatalysts for the oxygen reduction reaction (ORR) supports the DOE Hydrogen and Fuel Cells Program (the Program).
- Lowering or eliminating PGM and its cost is relevant to DOE objectives.
- It would be a technical breakthrough to develop inexpensive, non-precious-metal catalysts that perform as well as or better than Pt.
- Removing PGM from the cost of MEAs is in line with DOE objectives.
- The project is aligned with the Program and fully supports DOE objectives to decrease fuel cell costs and increase durability. This project aims to develop non-PGM catalysts and offers the potential to substantially reduce costs by removing Pt, one of the largest costs associated with PEM fuel cells, while still meeting performance metrics.
- Lowering the cost of PEM fuel cells is critical to ensuring their commercial success; catalyst cost is a significant portion of the MEA cost and this project intends to develop non-PGM catalysts, which should significantly decrease the cost of the catalyst.
- Non-PGM electrocatalysts are critical to the Program. The cost of an MEA made with non-PGM catalysts needs to be carefully analyzed, including the catalyst production, reproducibility, and cell performance control, so that the real cost can be compared with that of mature PGM catalysts.

### Question 2: Approach to performing the work

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

- The approach is sound.
- The approach is based on using suitable complexes and reacting them with metal sources on porous carbon to generate catalytic sites in novel materials. Adequate characterization is applied.
- The approach includes fundamental modeling, mechanistic studies, ex-situ (spectroscopic) characterization, rotating disk electrode (RDE) testing, and cell testing. The principal investigator reported that the density

functional theory (DFT) calculations were performed in a vacuum. Drawing conclusions and direction from this is risky when the actual system includes oxygen and water.

- The project's reasonable approach leads to promising catalyst activity. The non-PGM-catalyst approach still has some uncertainty, such as catalyst active sites, corrosion issues, and surface hydrophobicity characterization, which may have a significant impact on the activity, and hence cell performance.
- The rationale behind the materials selected for development is not obvious; it is unclear what precedence in the literature led the team to select and design the material targets. There was no description of how the team was going to tailor catalysts and control the metal support interactions or control the reaction center's electronic structure. It is unclear what is different about this team's approach to understanding the mechanism of ORR electrocatalysis and mass transport compared to other research groups in this field.
- The approach is directed toward identifying the catalytic center in non-PGM ORR catalysts, improving the catalyst, and improving the electrocatalyst structure for mass transport. Identifying the mechanism and catalytically active site will allow for real advancements to be made in non-PGM catalysts. The computational studies have been idealized and have been vacuum calculations. In the cluster approach, the calculations appear to utilize an N-5 coordinate metal (with an imidazole group occupying one of the axial sites). The nature of the group in the axial position trans to the site where the reaction is occurring can have a significant impact on the reaction energetics because this impacts the electron density available for back bonding to the reactant at the reaction site. It seems unlikely that an imidazole group with strong electron donating character would be present in the real-life (pyrolyzed) system. Due to a higher concentration in these pyrolyzed systems, a weaker-donating -$\pi$ system interaction (with the next layer of N-substituted carbon) appears to be more likely. A weaker donor is likely to have a significant impact on the bonding energy. Future calculations should trend toward more realistic systems, eliminate the imidazole, and move away from vacuum conditions if possible.
- The project focuses on the development of iron (Fe)-based catalysts for the ORR, and so far results have been equivalent to some of the best non-PGM-based catalysts to date. The project uses a combination of experimental data and molecular modeling, but with little evidence that the model and/or spectroscopically identified species are indeed the active catalyst sites. Some catalysts are tested using RDE and others are tested using the polarization curve; considering that RDE results do not always correlate well with fuel cell performance, it would be good to maybe use RDE as a screening tool and MEA testing as a validation.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The team has developed electrocatalysts that are as active as the best ORR non-PGM-based catalysts.
- It would be interesting to know what the polarization curves are for Fe and FeCo at lower loadings than 0.6 $mg/cm^2$.
- Scaling up the catalyst production is very important to validating the approach and revealing more issues, as well providing valuable information for understanding the catalyst's intrinsic properties.
- The DOE 2010 target for performance has been met. The durability cycling results are encouraging. Other tasks seem to be on track.
- Two to three electrocatalysts displayed high activity for the ORR that is close to the present DOE milestone of 150 $A/cm^3$. Stability has to be improved for most of the catalysts. The mechanistic studies are based on several spectroscopies. These were skillfully used to elucidate several features of these electrocatalysts. The bifunctional mechanism is not strongly supported by the data. "A lack of Fe-Fe bond" is only a suggestion. When $H_2O_2$ desorbs from one site, it is not likely that it will readsorb on similar ones. RDRE should help in elucidating this mechanism. DFT calculations were used as an additional tool for reaching the conclusions.
- There is no technical data showing that either the precursors or the pyrolyzed materials were obtained as proposed. It is unclear what happened to the metal-organic frameworks described in 2011. There appears to be little recent progress as compared to 2011. Milestones are only slightly changed from 2011. The polarization data should have a control that uses a traditional MEA for benchmarking. Polarization testing conditions should be specified. RDE data and Tafel slopes should have Pt run as a control for benchmarking.
- The team made good progress toward understanding the catalytically active site via the delta-mu EXAFS studies, which have shown a correlation between the presence of Fe-Fe-bonded species and increased activity, and suggest a dual site mechanism. The project has developed non-PGM catalysts with good activity, 150

A/cm$^3$, but this is still significantly short of the DOE target (300 A/cm$^3$). The project has developed catalysts with good durability for a non-PGM catalyst over the catalyst durability cycle (0.6–1.0 V); however, durability is poor when subjected to catalyst support durability tests. The researchers have shown some of the losses are recoverable, but stop/start (support) durability needs to be improved.

### Question 4: Collaboration and coordination with other institutions

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

- All collaborators participated in the research.
- The team is complete, and includes representatives from fundamental science, stack suppliers, and auto companies.
- There is no description of the nature and frequency of the team's interactions.
- The project features an excellent mix of collaborators.
- BAS might be interested in scaling up the catalyst, but there is probably not much involvement at this time.
- The coordination within the project is good. Coordination with the top experts in the field of non-PGM catalysts outside of the project is occurring.
- The project has strong collaboration with academic institutions. The team should work with more industry partners that will lead the team to gradually resolve the real industry concerns and issues.

### Question 5: Proposed future work

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

- The future work is logical and rationally follows the progress made so far.
- Verification of the catalysts' activities and selecting the catalysts for further work is planned to be done using the RDRE technique. The proposed reaction mechanism should be checked using the same measurements. Other plans are a reasonable continuation of the results obtained so far.
- The work proposed for 2012 looks remarkably like that proposed in 2011. There was no description of how the researchers propose to improve mass transport, or even why they think mass transport is bad with the current electrode.
- The studies designed to identify the reaction mechanism should be given the highest priority of the proposed future work.
- Because it is very difficult to identify the active species for such catalysts, the planning proposed may lead to marginal improvements if the species identified are not the active catalyst(s).
- The team should emphasize the research and development work on the batch-to-batch comparison.

### Project strengths:

- This project features a strong team and excellent in situ and ex situ techniques.
- The team appears to have access to a lot of really good equipment for testing and characterization.
- The effort to determine the reaction mechanism has provided key insights and is a strength of this project.
- Improvements in transport phenomenon and electrode nanostructures could be applicable to other systems.
- The modeling is strong, as it needs to be, because the actual reaction site is still not known with certainty.
- The project is very strong on research, characterization, and analysis.

### Project weaknesses:

- The materials the investigators have chosen for development show little promise for meeting the DOE goals.
- The team needs to strengthen the collaboration with industry partners.
- The characterization of bulk catalyst by XP and the assumption that the main species identified is the catalyst site might not be accurate.

- It appears that the cost to make these materials would be very high, offsetting the cost for PGM cells, which are steadily reducing their PGM loading. These are currently at <0.1 mg/cm$^2$ Pt on the cathode and <.05 mg/cm$^2$ on the anode. It is unclear what the cost structure of these routes would be.

### Recommendations for additions/deletions to project scope:

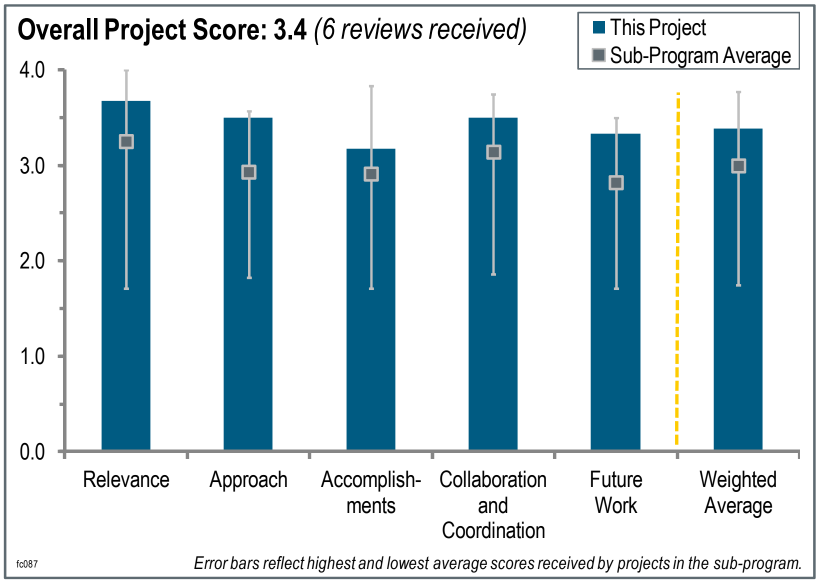
- The team should make additional efforts to verify the reaction mechanism, and perform more long-term stability tests of the catalysts.
- The team should make several larger batches of catalysts that are enough to carry out systematic investigation and cross-checking experiments.

## Project # FC-087: High-Activity Dealloyed Catalysts

Fred Wagner; General Motors

### Brief Summary of Project:

Project objectives are to: (1) demonstrate reliable oxygen reduction reaction kinetic mass activities; (2) demonstrate the durability of the kinetic mass activity against U.S. Department of Energy (DOE)-specified voltage cycling tests in fuel cells; (3) achieve high current density performance in hydrogen/air fuel cells adequate to meet DOE heat rejection targets and platinum (Pt)-loading goals; (4) scale up to full-active-area fuel cells, to be made available for DOE testing; (5) demonstrate the durability of high current density performance; and (6) determine where alloying-element atoms should reside with respect to the catalyst-particle surface for the best durable activity.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- The project is well aligned with DOE goals on reducing the cost of polymer electrolyte membrane fuel cells' cathode catalysts and improving their durability.
- Increased catalyst activity and reduced catalyst cost is paramount to the DOE objectives. Dealloyed catalysts are a promising avenue to reach these objectives.
- This project represents an important contribution to the DOE Hydrogen and Fuel Cells Program (the Program). The main focus is placed on improving the durability and reliability of fuel cells by utilizing durable catalysts with improved performance and low Pt content.
- Objectives of the project show direct relevance to DOE's overall objectives of cost reduction (reducing Pt loading), increased durability (maintaining kinetic activity and high current density), and improved performance (achieving high current density with high voltages).
- The development of a cathode catalyst with high activity, performance in the high current density region, and durability is critical to the implementation of fuel cells for automotive applications.
- Catalyst cost is a significant barrier to meeting automotive targets.

### Question 2: Approach to performing the work

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

- The project is well designed and sharply focused on DOE targets for catalyst activities and durability.
- The focus of this project has been improved by concentrating on nickel (Ni) and cobalt (Co) systems. The project is well designed and led.
- The project is well planned and each participant represents an important contributor to the overall success of the Program. The major barriers are successfully addressed, and a systematic approach has been pursued in achieving the projected milestones.

- The approach is focused and clear, and milestones and go/no-go decision points have been established. The project has gone through several iterative cycles of synthesis, evaluation, and characterization; the next step is scale-up and durability testing with large batches.
- The approach to improving catalyst activity is excellent, but there is the issue of improving activity and durability while also achieving high current density performance. As identified by the principal investigator (PI), a big risk with this approach is whether durability can be maintained or even improved while meeting the activity targets. Also, the project has gone away from copper (Cu) as the non-noble metal due to its tendency to plate on the anode and degrade anode performance, with the implication that Ni and Co are still leached from these alloys and transported to the anode, but not plated. Perhaps this leaching is then an issue for other components of the cell and system. It is unclear where these leached metals are ending up.
- The project demonstrates a feasible approach, which is relatively low risk compared to other approaches (e.g., non-PGM catalysts). Durability appears to be the key challenge, and some good concepts to overcome the issues to date have been formulated. However, it is not clear why the Pt-Co system is not more at the forefront as compared to the Pt-Ni system. Both should be considered.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Half of the DOE targets with respect to catalyst durability and activity are already met. The project demonstrates steady progress toward overcoming barriers.
- Good progress has been made, but there is still a relatively strong risk that durability targets may not be met. The team is making acceptable progress for this point in the project schedule.
- There has been good progress on meeting the goals of durability and activity in the same sample, but it is not yet clear that any one sample will be able to meet all of the goals. It seems that the Co systems may be more likely to meet the goals, but the Ni systems have greater promise for high activity if durability issues can be overcome.
- The progress in this project was successfully presented and participants have clearly demonstrated a strong team effort that has been capable of efficiently addressing Program objectives. The concept of utilizing dealloyed catalysts has been implemented for several Pt-bimetallic alloys and was verified as a potent approach in pursuing catalytically active and durable systems with a lower Pt content. A number of characterizations were performed, ranging from RDE, MEA, electron microscopy, in situ EXAFS, etc. The synthesis of a Pt bimetallic system was performed at laboratory-level scale (2 g), and also for larger batches (100 g). All of that has contributed to achieving a better understanding of the key parameters that are responsible for the improved catalytic performance and durability.
- A six-month extension has been requested, and only 30% of the tasks are completed to date. Lost more than 60% activity at 30,000 cycles, not the 40% target. The team has identified the reason for the loss in durability, and it is verifying the hypothesis now. It moved away from Pt-Cu to Pt-Ni because of Cu dissolution issues and Cu deposition on anode. Also, the team is no longer working with Co; Ni should be more durable than Co. The membranes are still too thick at 25 microns.
- Milestone 1 was not achieved. Is it not implied in the milestones discussion that the different milestones should be achieved in one catalyst material at a specified catalyst loading. It is unclear why there is a milestone of achieving mass activity, another for achieving kinetic activity durability, and then another for achieving the two milestones in one material. The accomplishments are good, but there are still many issues to be solved in a short amount of time—the biggest being retention of enough non-noble metal to effect activity but not degrade durability.

### Question 4: Collaboration and coordination with other institutions

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

- The project is well coordinated due to General Motors' (GM's) collaboration between industry and universities.
- The project shows good collaboration between all partners and features work done by both academia and industry.
- The coordination of the team is excellent. Contributions from all of partners were evident.

- The project features a nice team composition, which appears to be well coordinated.
- The achieved accomplishments from this project indicate a well-balanced synergistic effort among the participants. The success of this project in addressing critical barriers in fuel cells is due to the highly diverse and organized team, which is well coordinated by GM as a lead institution. Participants have contributed their expertise, which add to the overall success of the project.
- An outstanding team has been established with clear roles and responsibilities. The project's iterative process includes all of the participants. The project will have a strong technology transfer due to having the right team members from the universities characterization capabilities, the catalyst manufacturers, and the stack integrators.

### Question 5: Proposed future work

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

- Barriers are clearly identified, and plans are focused on overcoming those barriers.
- The future plans are clearly defined and focused. The future plans are realistic in scope. The team did ask for a six-month extension. Mitigation strategies are in place. The team must still address membrane thickness and durability. Durability testing has already started.
- This project excels in adapting the focus and approach—as evident in the future work—to attempt to achieve all of the targets simultaneously with other materials and processes.
- The PI has shown that the team is clearly focused on addressing the major barriers remaining and has a good approach to do so.
- The proposed future work seems reasonable, but there do not seem to be alternative paths to take if altering synthesis and dealloying conditions does not increase Ni durability. The amount of alloying material should be calculated in relation to the amount of replacement of protons in the membrane. The high current density operation problem will be underestimated if state-of-the-art thin membranes are not used in testing. For instance, it is unclear what percentage of protons will be replaced in a 20-micron membrane, and what levels of contamination are considered acceptable and why. Additionally, polarization curves should be taken past 1.5 $A/cm^2$ to show the full effects of the cationic contamination. This should be done by taking the curves to true limiting currents, and also by doing some sensitivity of high current performance to air pressure.
- The accomplished achievements in this project are not in ideal accordance with the proposed future work. The diversity of catalytically active and durable systems in the proposed future work is not sufficient. Instead of focusing on alternate dealloying methods, more efforts are needed toward utilizing different alloys. This team has demonstrated great potential in combining expertise in MEA, RDE, dealloying, characterizations, and synthesis; however, only a few promising systems have been studied with a rather narrow distribution of constituents, mainly a 1:3 ratio. Additional focus is necessary to synthesize nanoparticles with even size and elemental distribution.

### Project strengths:

- This project features a combination of advanced characterization techniques with state-of-the-art methods of fuel cell manufacturing and testing. The project has a solid fundamental background that allows the team to make fast go/no-go decisions.
- The team succeeded in showing high-activity catalysts that have a great chance to meet the DOE targets.
- The project features a well-organized approach with realistic goals and go/no-go decision points. An excellent team has been established to transition this technology from a concept to manufacturability/industry.
- Strengths of this project include the abilities of the team and the willingness to change approaches and materials to achieve the targets.
- The project team includes academics, component industry representatives, and original equipment manufacturer industry members. The approach is scientifically based and has merit. The project is focused on critical barriers and has good future plans.
- The team demonstrated the utilization of dealloyed catalysts that are different than the Pt-Cu system. The concept of strain that influences catalytic properties of alloy catalysts seems to be applicable not only to Pt-Cu, but also to Pt-Ni and Pt-Co systems. These materials have shown improved performance and durability over Pt catalysts, which clearly justifies the approach taken in this project. A highly collaborative team effort has

allowed this project to produce a valuable outcome. Participants are well-known experts in the field of fuel cells, and they bring highly diverse expertise in MEA, RDE, synthesis, characterization, dealloying, and EXAFS. Kinetic and mass activities have reached the projected milestones.

### Project weaknesses:

- The synthesis work does not look as strong as the other parts of the project.
- One weakness is the understanding of the trade-offs between durability and activity. So far there is no great concern about high current density operation, but this will likely be addressed in the following year.
- Only 30% of the project is completed to date. A six-month extension has been requested.
- The project appears to be too focused on one alloy composition (Pt-Ni).
- The project is mainly focused on a few systems, instead of providing an evaluation for a number of dealloyed catalysts with a wide range of elemental ratios. In the current work, particle size distribution seems to be a significant factor that can compromise the quality of measurements and may divert conclusions and future work into the wrong direction. Having ultra-small and large particles at the same time can largely affect the dealloying process, and, therefore, elemental distribution in particles may vary. The lack of theoretical input is a missing part in the listed accomplishments.

### Recommendations for additions/deletions to project scope:

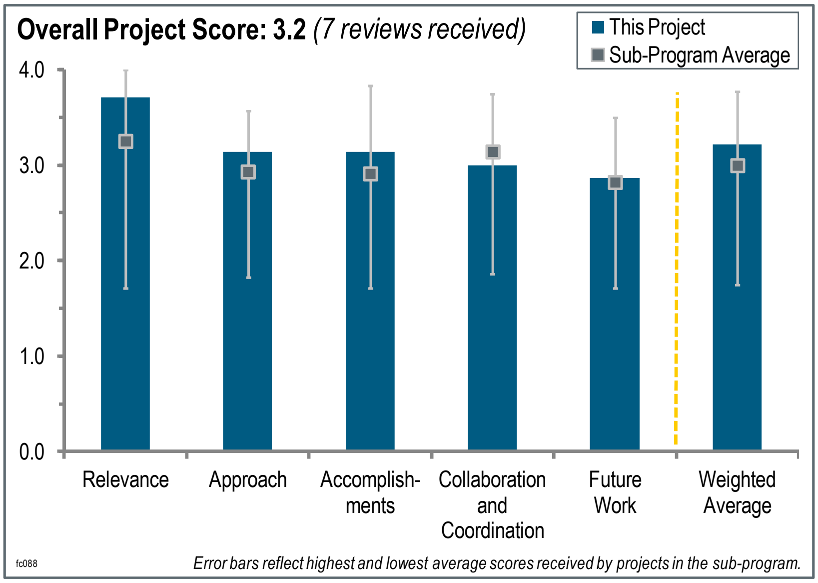
- It would be good to see more durability testing on other alloys (e.g., Pt-Co).
- Next year the team should move to evaluate the durability of supports. It would be interesting to complement a GM evaluation of non-noble metal diffusion time from the center of the particle to the surface by fundamental research. Durability as a function of particle size for dealloyed catalysts needs to be evaluated.
- More work on maximizing Co alloy activity may be necessary if Ni durability cannot be increased. Research seems focused on Ni systems that show greater promise, whereas Co systems may be closer to reaching DOE targets.
- This reviewer suggested that the team keep the project tasks and future work as is.

## Project # FC-088: Development of Ultra-Low Platinum Alloy Cathode Catalyst for PEM Fuel Cells

Branko Popov; University of South Carolina

### Brief Summary of Project:

This project will develop a high-performance, low-cost, durable cathode catalyst and support that is able to meet the 2017 U.S. Department of Energy (DOE) targets for polymer electrolyte membrane (PEM) fuel cells. The goal will be met through: (1) optimization studies of carbon composite catalyst (CCC) support, (2) development of advanced hybrid catalysts based on CCC support and platinum (Pt), (3) development of carbon nanocage (CNC)-supported Pt-alloy catalysts, (4) synthesis of corrosion-resistant hybrid supports, and (5) development of high-volume procedures for the synthesis of promising catalysts.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- The development of Pt elctrocatalysts is critical for the DOE Hydrogen and Fuel Cells Program.
- The project is aimed at reducing platinum-group-metal (PGM) content and cost.
- The development of low-PGM catalysts is highly relevant.
- The nature of the work, the motivation for the study, and other aspects of the project are relevant to DOE objectives.
- Reduction of total Pt loading in the fuel cell stack below 10 g is vital for fuel cell electric vehicle commercialization. This project is clearly aligned with this target due to its development of ultra-low Pt alloy cathode catalysts.
- The project is focused on a critical issue, which is to reduce the cost of the catalyst and improve the performance and durability of the catalysts, the catalyst layers, and the membrane electrode assemblies (MEAs) that support them.
- Commercialization of fuel cells can only happen with a reduction of the catalyst cost, which will happen by significantly reducing the amount of Pt used or switching to a non-PGM catalyst. This project takes the approach of significantly reducing the amount of Pt at the cathode.

### Question 2: Approach to performing the work

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

- The project builds on previous CCC non-PGM work by the principal investigator's (PI's) group.
- The approach is multifaceted with the investigation of two different supports: CCC and CNC. In both cases, the alloying of Pt with cobalt, copper, or nickel will eventually be employed to further increase activity.
- Development of CCC and modified $TiO_2$ supports for Pt appears to be a useful approach that has produced active catalysts.
- The experimental approach lacks detail in some places. A significant amount of data was presented; however, the presentation lacked clarity and focus. Almost no details were given on the approach used to synthesize the various catalysts studied in the project.

- The project has made impressive progress through its empirical approach. However, much is empirical and it is concerning that the unknowns are building up to a point where future progress will be stymied due to the existence of too many empirical optimizations that cannot be explained by scientific means.
- The project title emphasizes the development of a Pt alloy cathode catalyst, but a lot of work has been devoted to the development of catalyst supports in order to primarily address the durability of the cathode. The project addresses critical performance and durability issues. It is not always easy to compare the results obtained, because they were not run under similar conditions; in addition, it would be interesting to compare this project's results with a commercial catalyst on the developed support.
- The approach of combining high-activity Pt alloy catalysts with oxygen reduction reaction (ORR)-active supports is a sensible path toward high activity with low PGM loading. The use of conductive oxides as stable supports is also a good approach.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The progress and accomplishments are outstanding. This project is a very productive use of resources.
- ORR performance, mass activity, and durability (for at least one catalyst system) have been achieved, per the project milestones.
- The PI shared many, many results, indicating a lot of work has been done. Some of the results are very impressive with very clear analytical results supporting RRDE and fuel cell results. Furthermore, the PI has clearly synthesized and validated a whole host of different catalysts in both an RRDE and a fuel cell. However, most, if not all, of the IV data is iR-corrected, which, while valuable, needs to be benchmarked against non-iR-corrected data, otherwise it is not clear how relevant the benchmark data is. NRE212 is often used as the benchmark membrane in MEAs, but for an automotive study, at least, NRE211 should be used. Not enough durability work has been performed to successfully demonstrate the durability of the alloy or support.
- The activity and durability of the Pt catalysts (0.1 $mg/cm^2$) modified by carbon and pyridinic nitrogen are quite good. The stability of $Pt_2Ni$ requires additional characterization. If the Pt is covered by layers of graphitic carbon, it is unclear how oxygen ($O_2$) is reaching it. The properties of the $Pt/TiO_2$ catalyst,—particularly its corrosion resistance—are not sufficiently described.
- Many of the DOE targets seem to have been met. The June 2012 go/no-go decision point has not yet been met, but it seems likely to be achieved. It is not always clear at what conditions the testing is done. For example, Yonsei University seems to use fully humidified $O_2$ and not air, and data are reported on an iR-free basis. Real-world conditions would provide better information.
- The effect of the proprietary membrane on performance needs to be clarified. Because the results presented are iR-free, the membrane conductivity cannot account for the enhancement. The difference is most likely related to the electrodes. Hydrogen ($H_2$)/$O_2$ testing is appropriate for mass activity determination, but for MEA testing at high currents, gases should be $H_2$/air. The durability results of $Pt/TiO_2$ are encouraging, but higher initial performance is required. The high mass activity of the HCC catalyst is promising, but $H_2$/air performance at high current densities is still lacking. The Pt/C baseline used in the $H_2$/air results is below state of the art, but this is mitigated by the indication that the catalyst layer structure is still being optimized. The activity and durability of the PtNi/CNC catalysts are promising.
- It is difficult to evaluate progress in some areas because confusing, contradicting, or insufficient information was presented. For example, the results from MEA optimization studies for non-carbon supports were given. Durability comparisons were made between carbon and non-carbon (e.g., $Pt/TiO_2$) corrosion tests and related fuel cell performance. However, it was not explained how this level of performance is achieved from a non-conductive support. Moreover, no description of how the support was prepared was given. It is also difficult to ascertain progress when the PI does not always differentiate between old and new progress. For example, a JACS publication from 2009 (vol. 131, no. 39) showed the same corrosion data and figures for the $Pt/TiO_2$ catalysts that were presented as new work in this DOE Hydrogen and Fuel Cells Program Annual Merit Review and clearly not conducted in fiscal year 2012. This paper does give insight as to how the $TiO_2$ support was prepared, and it is a carbon-based route that raises the question of how carbon-free the non-carbon supports really are. The data presented by the PI conveys that the sample is composed of only oxide and Pt. As was seen in an earlier presentation by another PI working on non-carbon PEM fuel cell supports, a small carbon residual can significantly impact (i.e., add) electronic conductivity to the non-conductive $TiO_2$. This group also shared

data showing that even a small carbon residue also impacts durability negatively. The PI needs to explain the fuel cell performance and durability shown in light of probable carbon contamination.

### Question 4: Collaboration and coordination with other institutions

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

- The results of the collaboration are clear, and the collaboration makes a substantial contribution to the project.
- The project's collaborations are good and include universities and auto manufacturers.
- There seems to be a competent team assembled.
- The University of South Carolina (USC) and Yonsei University seem to be working independently on the catalyst support, but coming together on the Pt alloy used.
- The PI states that Yonsei University and Hyundai are partners. Yonsei University's work is barely covered and seems to be quite minimal. Hyundai's funding was stopped in December 2011, and it will resume in February 2013. It appears that the PI is doing all of the work at USC with very little interaction with the partners.
- Collaborations are very minimal, which casts some doubt on some of the results. The development of more collaborative work with, for example, Los Alamos National Laboratory or Argonne National Laboratory for durability studies, and Northeastern University for the CCC work, would be very useful. This work needs to be reproduced by others. Furthermore, there is a need for greater theoretical underpinning, and the team should consider how to accomplish this.
- The collaboration with Yonsei University is valuable because the parallel development of alternative catalyst structures mitigates risk. The collaboration with Hyundai is valuable for hardware design and stack testing. Collaboration with a participant with better MEA development expertise could be helpful.

### Question 5: Proposed future work

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

- For future work, the team plans to further improve the best systems. The improvements appear reasonable.
- The future work is focused on meeting go/no-go points and milestones, but there is little discussion on how they will be met.
- The future work looks reasonable. The USC tasks are presented in terms of goals; it would be good to see more details about how these goals will be achieved.
- The future work simply states future milestones, but not the approach that will be used at USC. The PI in this case did share plans from Yonsei University, but the PI made no mention of Hyundai's future planned work.
- The gaps between the project targets and the results today are well understood, and the future milestones present a path to achieving those targets. No alternative approaches have been highlighted if the durability issue of the support (carbon-based) or the performance of the catalyst on $TiO_2$ support cannot be addressed.

### Project strengths:

- The project features reasonable performance for a low-Pt-based catalyst with encouraging durability.
- The basic approach is sound and the participants have extensive relevant experience. The project leverages earlier successful work on designing PGM-free catalysts.
- Obviously, many different catalyst types have been validated and tested at USC. Many of them appear to achieve the necessary performance levels. There is very good correlation of work between the analytical and RRDE/fuel cell results.
- This project features a very broad and quick verification of the catalysts' performance in MEAs.
- The project is attacking known areas in catalysis needed for the advancement of PEM fuel cells.
- One area of strength for this project is its very productive and apparently reliable measurements. Data is plentiful and demonstrates excellent progress.

## Project weaknesses:

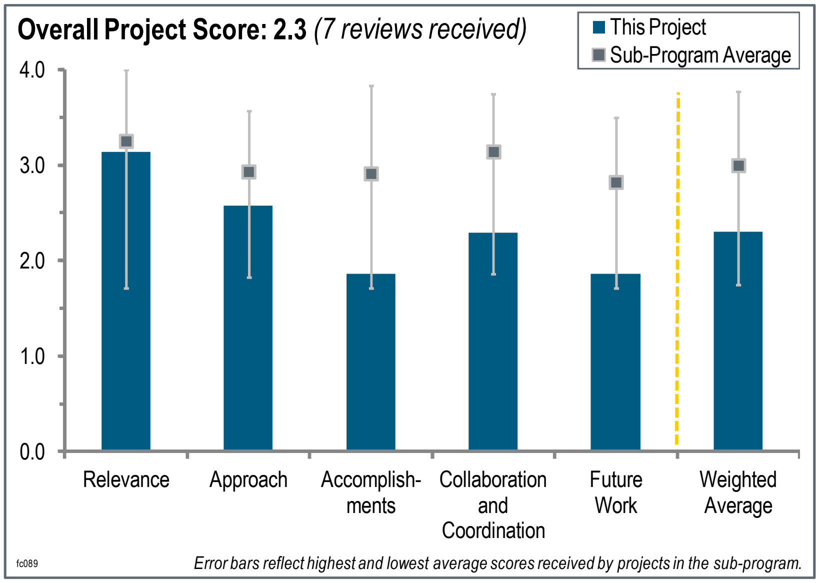
- There is poor interaction with the collaborators. The fuel cell data is all iR-corrected, which takes away from its value—the investigators need to show absolute data. The durability data is lacking when compared to the other data generated.
- The understanding of the behavior of some systems should be improved to make the future work more successful.
- One area of weakness was the confusing and unclear presentation of methods and data.
- The project was not reproduced by others—the team needs collaboration. The project needs better theoretical basis for the improvements.
- Very little data is available on the catalyst particle size before and after operation. More characterization is needed to understand better the degradation mechanism(s).
- The promising mass activity results are not all translating into high-performance MEAs, as of yet. It is not clear that the participants have the MEA integration skills to match their catalyst development skills.

## Recommendations for additions/deletions to project scope:

- Additional characterization of the $Pt/TiO_2$ system seems necessary.
- The team should add more collaboration for both practical measurements and theoretical basis.
- The team should focus on one or two high-potential catalysts. It should also make better MEAs with NRE211 and show its absolute performance under a variety of conditions. Researchers should clearly show their future plans and get Hyundai involved again.
- The team should not make any changes.
- No additions or deletions should be made.

## Project # FC-089: Analysis of Durability of MEAs in Automotive PEMFC Applications

Randy Perry; DuPont




**Brief Summary of Project:**
This project addresses gaps in the understanding of fuel cell durability and modeling of fuel cell performance degradation. The project focuses on durability at low relative humidity and during automotive cycling operation, and addresses short side-chain polymers (-O-CF2-CF2-SO3H). Three main objectives of the project are: (1) determine accelerated stress tests (ASTs) to be used to generate data for modeling of the individual degradation mechanisms; (2) develop an overall degradation model that correlates the stack operating conditions to the degradation of the membrane electrode assembly (MEA); and (3) develop MEAs with a design lifetime target of 5,000 hours with ≤7% degradation and that show a clear path toward meeting U.S. Department of Energy (DOE) 2017 technical targets.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.1** for its relevance to DOE objectives.

- This project is generally well aligned with the performance and durability goals for MEAs.
- Improving durability is critical for the commercialization of fuel cells. The project is defining ASTs, developing a degradation model that includes operating conditions, and working to develop durable MEAs.
- ASTs are potentially very valuable tools for assessing fuel cell materials durability; however, they do need to be validated.
- Developing a degradation model and the goal of achieving the DOE targets are critical for polymer electrolyte membrane fuel cell commercialization.
- The topic area and original intent of the proposed project was relevant, but for various reasons the project has not followed the proposed scope or added much value to the DOE Hydrogen and Fuel Cells Program.
- Delays in the start of this project perhaps hurt its timeliness; at the same time, the delays could be an advantage if the researchers could build upon the learnings of the other durability projects. Unfortunately, it was unclear from the presentation how the projects differentiate and relate to one another. This project seems to be using protocols that are slightly different from DOE's protocols. That can be an advantage and a disadvantage in comparison.

### Question 2: Approach to performing the work

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

- The approach of looking at durability issues in Nafion-related membranes is good, as are the diagnostics and modeling to understand degradation.
- The approach appears to rely solely on a few materials and AST tests. The team dropped the development of a durable MEA. It is unclear why it took so long to get the Nissan subcontract in place. Nissan should be commended for providing so much work while not under contract.

- The researchers performed two types of ASTs—the test protocol developed by the U.S. DRIVE Partnership's Fuel Cell Technical Team (FCTT) and a Nissan protocol; however, it is difficult to evaluate the validity of the tests because no long-term degradation tests were performed. The value of the study without baselines is unclear.
- The proposed approach and intent of the project were useful and effective, but the actual execution of the project has been limited to contributions from Nissan using the Japanese fuel cell durability protocols and comparison to the DOE protocols.
- The approach of this project seems well thought out, with clear go/no-go points, although the "Approach" flow chart on slide 5 is not very helpful. There is a typo on slide 4, go/no-go 1, sub-bullet 2, which sets a target of 1 $kW/cm^2$. This should be 1 $W/cm^2$.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **1.9** for its accomplishments and progress.

- There does not appear to have been much development of mechanistic models or validation of the results.
- The project describes MEA testing, but little progress has been made toward the goal. The only thing that the researchers have really done is develop test plans.
- The amount of work completed and the results presented seem in line with expectations for this type of project. The details provided from the test protocols, especially from Nissan, were very informative and helpful in interpreting the results.
- This project is adopting the "Nissan/Japanese" fuel cell carbon corrosion start/stop protocol, and it shows data from approximately five catalyst-coated membranes. The MEA on slide 9 appears to have originated with a cracked catalyst layer. These are probably not the best samples to use, because the cracks have known degradation effects. Most results appear to have come from Nissan's own work, not as part of this project.
- The strength of the project has been in the background work performed by Nissan. The accomplishments to date have been limited compared to the proposed statement of work. Also, development of the model based on experimental/characterization results is not evident, and the application of the suite of characterization techniques listed is also not evident. Due to difficulty in establishing subcontracts, the progress of the project in general has been minimal, specifically the contributions from Illinois Institute of Technology (IIT).

### Question 4: Collaboration and coordination with other institutions

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

- The project seems to have a well-balanced scope of work and good interaction between the partners. The partnering organizations represent a nice balance from academia to materials development representatives to original equipment manufacturers.
- The collaborations in the project are unclear. IIT is supposed to do the modeling, but nothing was shared until the reviewer-only slides; even so, the IIT work does not show if there is any real collaboration between IIT and other project partners. Nissan appears to have done most of the work to date, yet it was not under contract.
- 3M was a subcontractor in 2011, and it is no longer one. It is unclear whether there was a particular issue that precluded their involvement.
- The only progress made with Nissan involved deciding which ASTs to use.
- Good collaboration between DuPont and Nissan is evident. The collaboration with IIT has either not started or is minimal.
- The collaboration is weak because the partners were so late to get under contract. DuPont could have been more creative to get its partners going earlier. DuPont does get some credit for working with Nissan to develop test protocols.

### Question 5: Proposed future work

This project was rated **1.9** for its proposed future work.

- The proposed work is fine, except for the lack of a model.

- The project needs more analytical work to understand the reasons for differences in degradation. Long-term drive-cycle testing also needs to be performed to validate the ASTs.
- There appears to be a tremendous amount of work to be done before the go/no-go decision this fall. It is unclear if the team will have sufficient time to gather enough data for the reviewers to make an informed decision.
- The planned future work seems to be on target to continue addressing DOE's objectives and is generally responsive to the detailed feedback provided by the FCTT and reviewers. Researchers should pay close attention to distilling the key learnings from their work and put those learnings in clear context in next year's report to the community.
- Future work includes beginning tests with HSAC. It is not clear from this project what the researchers intend to learn from this. Testing the effect of polymer morphology seems like an area that has been unexplored and one at which DuPont should excel. New work on gas diffusion layers and plates was mentioned in terms of post-mortem, but no information was provided on what type of testing/characterization was used, what type of plates, or what data the researchers hope to get and how to use it.

### Project strengths:

- This project features good collaboration and a well-balanced team. The team employed good transparency of protocols.
- This project has good coordination between the partners.
- The original proposal was reasonable.
- The team and its complementary expertise is the project's strength.
- The industry lead provides relevant membranes.

### Project weaknesses:

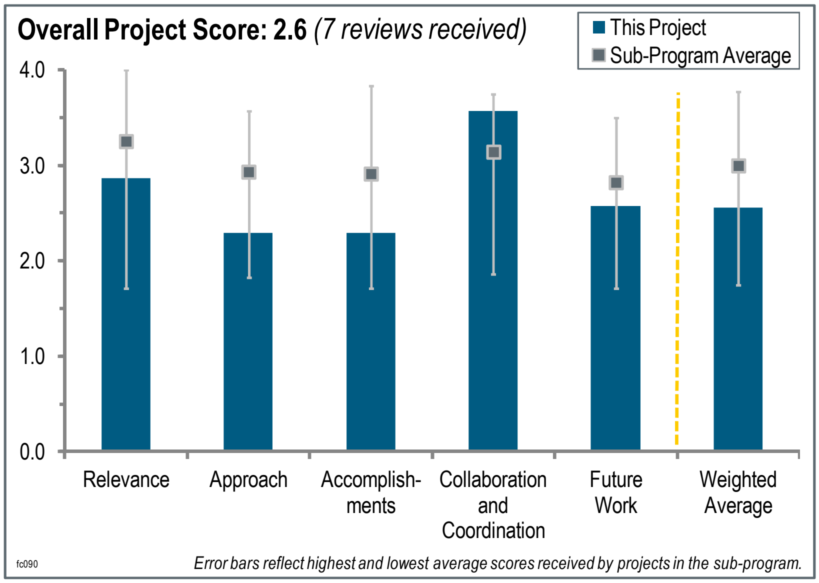
- The project needs to provide greater contextualization of what the team has learned that is important to the industry.
- The project should show real collaboration and how data, materials, and/or information are being exchanged to make a stronger project, rather than just being three independent projects.
- The project needs more modeling and validation.
- The project has not yet really started.
- A weakness of the project is the lack of coordination and development of the model based on experimental results, which has been hampered by subcontract issues, delaying the actual flow of funds to the partners.
- The project has not achieved any modeling despite being a modeling project.

### Recommendations for additions/deletions to project scope:

- The researchers could be much more descriptive concerning the nature of the future work and what they intend to learn. The integration of the modeling effort appears to need work. The carbon corrosion work would be more valuable if $CO_2$ evolution data was reported with recovery sequences. Similarly, only electrochemical surface area was presented; particle distribution analysis is more valuable.
- The investigators should begin work on the effect of short-chain ionomers and model development immediately.
- With 80% of the project funds remaining, this effort should be significantly re-scoped to make it an effective use of DOE's investment. A partner with a practical model is needed, and the appropriate measurements and diagnostics should be carried out to get useful results from the model.
- There are no additions or deletions to make to the project scope.

## Project # FC-090: Corrugated Membrane Fuel Cell Structures

Stephen Grot; Ion Power




### Brief Summary of Project:

The main goal of this project is to pack more membrane active area into a given geometric plate area, thereby allowing both the targets of power density and platinum (Pt) utilization to be achieved in fuel cells. In support of this goal, the project will: (1) demonstrate a single cell (50 $cm^2$) with a twofold increase in the membrane active area over the geometric area of the cell by corrugating the membrane electrode assembly (MEA) structure; and (2) incorporate an ultra-low Pt-loaded corrugated MEA structure in a 50 $cm^2$ single cell that achieves the U.S. Department of Energy (DOE) 2015 target of 0.2 g Pt/kW, while simultaneously reaching the power density targets of 1 W/$cm^2$ at full power and 0.25 W/$cm^2$ at one-quarter power.

### Question 1: Relevance to overall U.S. DOE objectives

This project was rated **2.9** for its relevance to DOE objectives.

- The project is relevant to reducing the cost of polymer electrolyte membrane (PEM) fuel cells by using a novel cell/stack design architecture.
- This is a nice project that addresses key barriers within the DOE Fuel Cell Technologies Program (FCT Program) related to costs and performance.
- Enhancing membrane and MEA performance is very important to reaching FCT Program targets.
- The team’s two goals are laudable: (1) raise volumetric power density and (2) lower catalyst loading. The attempt to raise power density by geometrical change of stack is laudable, although the validity of achieving this goal by the team’s approach has not been demonstrated—even preliminarily—after one year.
- Any method to decrease material costs while maintaining performance and durability would be highly relevant. However, the ability of the structures developed by this project to do so in a cost-effective, readily manufacturable way is questionable.
- The basic construct for a PEM fuel cell, bipolar plate, and frame has not been seriously questioned for decades. It makes sense to review such assumptions periodically. So, it is good to see that DOE is willing to explore basic stack design issues.
- New, “out-of-the-box” ideas are welcome to overcome barriers to fuel cell adoption, but this project has a low likelihood of being incorporated into automotive fuel cell stacks. It is not easy to understand how corrugated structures can significantly increase the system power per volume or significantly reduce the system cost for the same power. The corrugated structure greatly increases the complexity of the stack components. Durability will also likely be negatively affected.

### Question 2: Approach to performing the work

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

- This is a nice approach to a novel, single-stack system that is early in the project.
- This is an innovative approach to enhancing fuel cell power density and Pt utilization.

- The task structure, schedule, and milestones were not presented, so the approach is not clear. The approach seems to be almost trial and error. Additional modeling (e.g., fluid flow and cell compression) would be instructive. The presenter did not discuss whether the adjacent cells are nested, pinched, or parallel. This is a critical design and assembly parameter.
- The approach is to raise the real versus projected area by corrugation. Calculations suggest this gain will persist in three dimensions, but this is not a "given," and therefore the premise needs validation by experiment as soon as possible.
- A corrugated structure was made, but the team gave no indication that a thought process was used to determine the optimal corrugated geometry. It is unclear why the aspect ratio between the frequency and amplitude of the corrugations was chosen. In order to maximize total surface area per square centimeter, one could build a corrugated structure with a very high aspect ratio. This would definitely have higher current density per $cm^2$ flat area, but it would not necessarily make a better power-to-volume or power-to-weight ratio in the stack. The price modeling needs more explanation. It is hard to see where the actual cost benefits are coming from. It is unclear what assumptions are being made. It would be interesting to know if the total amount of Pt used to reach the same system power is decreasing, and how this decrease in Pt is affecting voltage at maximum power and efficiency. Corrugated materials cannot be less expensive than non-corrugated materials. Also, system volume is shown to decrease, but the team did not present any real data showing how the size of these new systems compares to the size of the state-of-the-art fuel cell stacks. Investigations into metal flow fields show more promise than the corrugation work.
- In principle, this is a promising approach, but serious concerns remain as to how this approach could be implemented in a high-throughput manufacturing process. It will be much more difficult to achieve low tolerances in the corrugated structure than in a conventional flat structure. There may be issues with pressure distribution and contact resistance. Also, the 1 $W/cm^2$ target is an MEA target, which presumably applies to the real area of the MEA, so the corrugated structure would have no bearing on the achievement of this target.
- The basic assumptions made in the proposal were perhaps not well thought out. Considerable time and effort has been spent working on materials issues and determining how to build the gas diffusion layer (GDL) instead of wondering why it should be built. The critical issue of power density seems to be ignored, as was the issue of operating any stack with very low Pt loading with the expectation of having useful durability (ultra-low Pt loading and excellent durability tend not to go together.) There was also no apparent questioning about the complexity of manifolding, the issues of water removal, or thermal management. Likewise, the fluid dynamics of reactant flow appears to be overlooked.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.3** for its accomplishments and progress.

- A cost-benefit study showed positive results (reduced cost) for the concept. Progress is respectable, considering the funding level.
- Most work has been on non-corrugated flat screens and projections based on estimations. Real effects such as compressive deformations (shorts by sharp points) have not been experimentally checked.
- Some thoughtful design and fabrication work has been completed. However, early results tend perhaps to show that the design will be difficult to accomplish.
- Because the project is only 25% complete, there are still a lot of questions to be asked, especially in the area of GDL and membrane corrugated manufacturing to allow more active surface area. Subassemblies have been built but not utilized.
- Progress has been made toward creating corrugated structures. Cost analysis with different Pt loadings has also been completed.
- Researchers developed very nice tooling to properly implement this concept. General Motors (GM) developed a very interesting cost model that will help focus the future effort toward the overall lowest cost.
- The project is still only 25% complete, and fuel cell testing with the corrugated membranes has yet to begin. Some progress has been made with GDL development and with the modeling effort. The lack of reference to any schedule, milestones, or decision points makes it difficult to assess actual progress versus the proposed work. The MEA testing results with flat cells are hard to interpret because they are plotted in terms of current (should be current density), and because the operating conditions are poorly specified. Not knowing the

experimental details makes the comparisons to the baseline materials unconvincing—it is not clear if these GDL materials really provide an improvement, or if the baseline is just bad.

### Question 4: Collaboration and coordination with other institutions

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

- Strong collaboration was shown.
- The collaborations are good, covering screen and plate suppliers and an automotive original equipment manufacturer.
- GM, Graftech, Ion Power, and their vendors are all first rate.
- This team is very strong technically with significant collaboration with industry.
- This project features excellent collaboration with GM and various suppliers.
- The collaboration with GM is very valuable.
- The collaboration from GM apparently was focused on cost. It seems a little early to worry about cost; there is no firm evidence that the concept is sound. The other collaborations seem to be interactions with vendors.

### Question 5: Proposed future work

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

- The proposed future work does not show a comprehensive plan of design.
- Fuel cell testing is critical to validating this concept.
- More detail is needed. Dates, milestones, and decision points are needed.
- A critical part of the project is single-cell testing, which is planned for the next period assuming that all of the cell parts (plate, GDL, etc.) can be fabricated and assembled.
- The proposed work involves making a corrugated GDL and testing it in a single cell. Stack work needs to be done as well to experimentally test the premise of lower catalyst and higher volumetric power density.
- The proposed future work is exciting, as the project team pushes toward working with the new subassemblies to manufacture the corrugated GDL and start single-cell testing.
- It appeared that the required funding to move this project forward is much more than is in the budget. The pathway forward was not described.

### Project strengths:

- This project has a strong team.
- The project features a good concept and a great team to implement it.
- This project has a novel approach that will allow for more surface in the membrane (e.g., more active surface area), which could increase single stack performance. The cost analysis presented showed the potential for significant cost savings by using these corrugated systems.
- The metal meshes show interesting results.
- The idea is innovative and, if implemented successfully (a big if), could provide significant cost reduction.
- It is worthwhile to explore possibilities for new electrochemical reactor design, and this is one of those.

### Project weaknesses:

- The physical (flow, structural) modeling is inadequate.
- The team did not complete enough engineering design work before bending the metal.
- This is taking a long time, and secondary issues such as the corrosion cost effects of mechanical stresses and lifetime should have been addressed by now.
- Because the subassemblies for GDL and membrane production have just been completed, more single-cell testing is needed. Mechanical compression, bonding/interface of cell setup, and uniform thickness are a concern going into cell testing.

- The assumptions made in modeling are not intuitive. It is hard to imagine how a corrugated structure is better than a structure that is running on a lower Pt loading but with more active area.
- The corrugated structure will be more difficult and more expensive to manufacture than conventional flat MEAs. Increased dilution of Pt across the MEA leads to higher peroxide production. Reactant distribution and water management may be more difficult.

### Recommendations for additions/deletions to project scope:

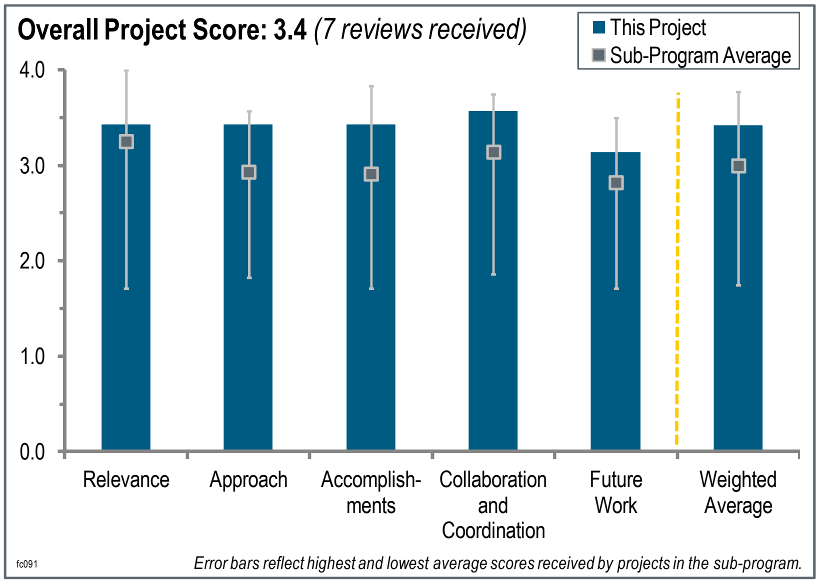
- Stack work needs to be done as well to experimentally test the premise of lower catalyst and higher volumetric power density.
- It is recommended that this project should continue in anticipation of results next year.
- Work on optimizing metal mesh flow fields seems to show more promise than corrugated geometry. A more fundamental and detailed understanding of these results would be more useful for the community.
- Doing some computational fluid dynamics to describe the flows of reactants, products, and heat makes sense. There also needs to be some thoughtful analysis on this design, even if the initial proposed advantage is not real. (That assumed that operation with a lower current density would result in higher voltage, even with very low electrode loadings.) There could be benefits in this approach. The Argonne National Laboratory team could be of good help here, and several issues might be quickly resolved. Clearly there needs to be some thinking about reactant crossover, and the implications of having much more surface area with the same number of electrons generated. At low current density levels, “i” is low, and thus iR is too. Perhaps crossover can be reduced to a low level. The GM help stressed costs. Costs are not the issue here. (Solid oxide fuel cell designs have for decades used corrugated cells, and that design was selected for higher durability.) The analysis needs to look at the complex set of variables. There are many; for example, if one works at 85% efficiency rather than 50% efficiency, there is much less heat to manage.
- The team should not make any changes.

## Project # FC-091: Advanced Materials and Concepts for Portable Power Fuel Cells

Piotr Zelenay; Los Alamos National Laboratory

### Brief Summary of Project:

This project's objective is to develop advanced materials (e.g., catalysts, membranes, electrode structures, and membrane electrode assemblies [MEAs]) and fuel cell operating concepts capable of fulfilling cost, performance, and durability requirements established by the U.S. Department of Energy (DOE) for portable fuel cell systems and to ensure a path to large-scale fabrication of successful materials.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.4** for its relevance to DOE objectives.

- The project is excellently aligned with DOE portable power and overall fuel cell goals.
- The project is relevant to meeting DOE targets for portable power.
- The project's multiple goals seem to be aligned with the overall DOE portable power goals.
- Catalyst and electrode work in particular are relevant to the DOE Hydrogen and Fuel Cells Program for portable power devices.
- This project fully supports DOE objectives related to direct methanol fuel cells (DMFCs).
- The project is relevant to the objectives of the Fuel Cell Technologies Program's Multi-Year Research, Development and Demonstration Plan. The activities are aligned to DOE's goal. This project is focused on the development of advanced materials, such as catalysts, membranes, electrodes, and MEAs for DMFC application, which is expected to fulfill the cost, performance, and durability requirements established by DOE, and is very important for the commercialization of DMFC technology.
- The project addresses the main DOE challenges of durability, cost, and performance for portable fuel cells. The project is aligned with the DOE goals and performance targets for portable fuel cells. The DOE performance targets have been modified since the project proposal and may present an opportunity to have a successful outcome even if the original proposed targets are not met.

### Question 2: Approach to performing the work

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

- The technical approach used in this project is adequate and well defined.
- Overall, the approach is sound and focused toward addressing barriers.
- The work is a good balance of introducing new materials into DMFCs and working on enabling technology for next-generation fuels. Degradation studies indicating the change in crack formation in catalyst layers as a function of methanol concentration and voltage are useful.
- The approach is quite broad and covers catalyst research; innovative electrode structures; hydrocarbon membranes; research into the use of alternative fuels; and characterization, performance, and durability testing in multicell devices. The approach appears to be well balanced with numerous go/no-go decision points.
- The multidirectional approaches taken for the completion of all tasks are adequate. All of the technical barriers have been addressed appropriately. The responsibilities for anode, membrane, alternative fuel development, and performance/durability testing were given to the research teams with significant experience and strength in those respective areas of research.

- The overall approach seems to be a sound evaluation of the materials' performance and durability. The project team seems to be pursuing a range of different approaches to improving the performance of the subcomponents. Making gains in these materials for DMFCs should provide gains related to the performance and durability. The work in alternative fuels with dimethyl ether (DME) and ethanol provides an interesting alternative approach; however, the ability of these materials to meet the DOE objectives for portable power in a practical packaged design was not clear either due to fuel storage and use issues or uncertainty about their ability to meet the energy density targets.

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The team has made significant progress in many areas.
- Very good progress has been made. The team has generated a significant amount of high-quality results.
- Significant progress toward project goals was made. There should be some sort of cost estimate on how use of the BPSH copolymers is going to lead to a lower MEA cost.
- The team appears to have made good progress in several areas, in particular in ternary anode catalyst development by showing good mass activity for the catalyst. Additionally, the work on understanding the degradation mechanisms looks promising as a mechanism to understanding the long-term performance of DMFCs for portable applications. While the work on catalysts for ethanol oxidation looks promising, it was not clear whether sufficient performance would be obtained for a practical MEA. The results for the DME looked promising, but it was not clear how the fuel could be utilized in a practical portable power system.
- The project team has made several achievements, including excellent improvements in anode potential with advanced catalysts, and successful scale-up to 100 g without performance loss. The anode research is on track to reach the target of improved activity of thrifted PtRu catalysts without a durability loss, and to reach the project goal of 150 $mA/cm^2$ at 0.60 V (DMFC). The work on ternary catalysts for ethanol oxidation is very promising.
- Los Alamos National Laboratory (LANL) has achieved numerous milestones to date. The mass activity milestone for a tin-containing PtRu catalyst was exceeded by 150%. Three multi-block copolymer membranes bettered the performance of Nafion® 212 in DMFC testing with a 55% reduction in methanol crossover. LANL achieved a DMFC fuel utilization of >95% at peak power. Good performance was achieved for direct ethanol fuel cells, but the stability of the catalyst needs to be improved.
- Significant achievements have been realized in membrane and anode developments. Impressive performance was observed with multi-block copolymer membranes. The gas-feed DME is showing little performance advantage in the mass transport region. However, overall performance efficiency, considering the parasitic loss incurred by pressurizing the system, should be considered for making comparisons between liquid- and gas-feed DME cells. The team has reported that the DME cell performs better than a DMFC at a voltage higher than 0.49 V. However, the team should consider their respective performances at higher current densities, where a practical cell will be operating; from the given data, a DMFC clearly outperforms a DME cells at 500 $mA/cm^2$. The team should calculate the parasitic loss associated with the back-pressure of a gas-feed DME system to make a true comparison between gas-feed DME and liquid-feed DMFC systems.

## Question 4: Collaboration and coordination with other institutions

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

- Many good collaborators with key areas of expertise have been pulled into the project.
- The project features a large number of high-quality collaborators.
- The project activities are well distributed and coordinated among the team members.
- Collaboration is very strong with universities and a major catalyst supplier. The project team is strong; however, it is not clear how much funding and effort is placed with the collaborators.
- The team consists of a good mix of university, national laboratory, and industrial partners. The proposed collaboration with Oorja Protonics will also be very advantageous to the team because Oorja has commercial high-power DMFC system offerings, which will help the team obtain good information on the material/performance requirements for a high-power DMFC system.

### Question 5: Proposed future work

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

- The future work described is aligned with the proposed work of the project.
- Reducing methanol crossover in the BPSH block copolymers by removing the 6F moiety and replacing it with hydroquinone may lead to higher interfacial resistance between the membrane and the electrode.
- LANL should propose a critical path that will lead to a viable product with commercial potential for at least one of the paths being investigated. This critical path will necessitate some form of go/no-go decision, which may lead to abandoning a promising research area to achieve a recognizable success by the end of the project.
- Even though the proposed future work is adequate and in accordance with the project objectives, it would be good to see the correlation between methanol crossover and catalyst layer cracking before the development of a mitigation strategy, since the cracks are formed in MEAs with Nafion®.
- The overall approach to further work looks good for the four broad categories listed in the presentation. The ternary anode catalyst improvements and durability analysis under practical stack operating conditions are important steps in understanding the benefits of the new anode material. The materials development regarding the membrane and catalyst is focused on the key improvements in durability and performance. For nanostructured catalysts, it would be useful to understand the trade-off of the potential performance improvements with any increases in catalyst cost. It would be useful to ensure that performance benefits were achieved in an MEA. As improvements in the DME oxidation are made, it would be helpful to understand how they could be incorporated into a portable power application that can meet the DOE portable power objectives.

### Project strengths:

- LANL has made excellent progress to date. This project will significantly advance portable power fuel cell capability.
- The project features an excellent team and results. The catalyst synthesis correlates well with theory.
- The team is well organized and capable of developing DMFC membranes and MEAs. The team is composed of research organizations with adequate expertise and resources. Overall, the team is equipped with the knowledge base and resources required for the success of this project.
- This is a strong team that has carried out good basic and applied work to understand the fundamental mechanism for performance improvement and durability.
- The project features excellent work and significant achievements. The project combines fundamental approaches with MEA/fuel cell testing.
- One strength of this project is the excellent make up of the team. The slides contained a significant amount of organized information with highlights pointed out to make the reviewers' job somewhat easier.

### Project weaknesses:

- The team is a combination of a large number of research organizations, which may be a management challenge for LANL.
- The project involves many different approaches. Because these are early fundamental data for some of the approaches, it is hard to see how some of these approaches are going to produce increases in power and energy density to meet the DOE goals, which are expressed at the system-level for portable power.
- Increased clarity on the practicality of some of the approaches is desired.
- The slides were too information dense. Too much detail on each slide made it difficult to follow during the presentation. The slides should contain one or at most two takeaway messages.
- More data are necessary regarding the membrane durability in the presence of higher concentrations of MeOH. In addition, the crack formation mechanism in the MEA catalyst layer needs to be better understood, whether it is caused by the membrane properties, catalyst, or the MEA preparation.

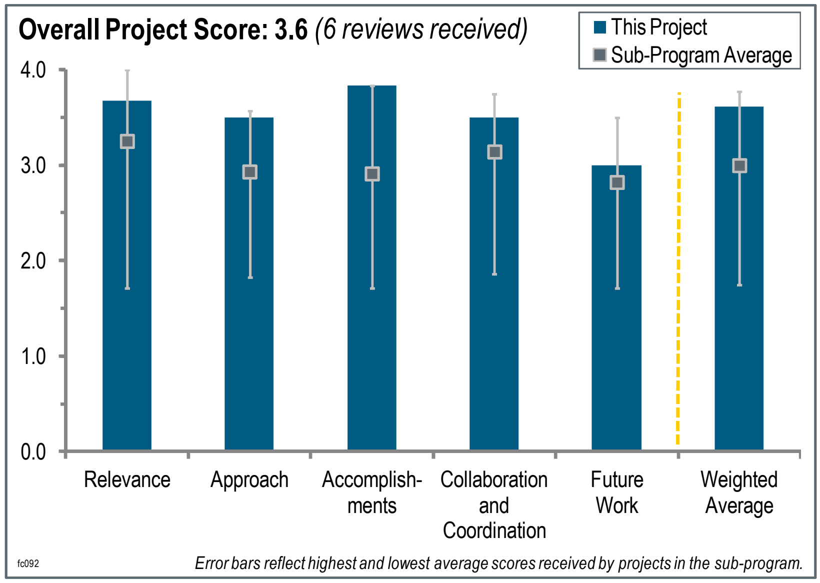
### Recommendations for additions/deletions to project scope:

- LANL should continue to focus on methanol as a fuel.
- LANL should narrow the scope of the project to those paths with the most promise.

- Given the team's strength in direct fuel systems, the team should drill down to the fundamentals of DME fuel systems and determine the viability of the success of DME technology as compared to DMFC technology.
- LANL should carry out work to understand the potential benefits or trade-offs in the performance improvements and costs at the system level.
- Durability is a major concern. LANL should increase emphasis on understanding and mitigating durability effects. The team should also increase interactions with SFC Energy to ensure practicality and progress toward realistic goals and operating scenarios.
- LANL needs to investigate thoroughly the causes of the crack formation in the catalyst layer. It is recommended that Johnson-Matthey Fuel Cells Inc. be involved in the preparation of catalyst layers and the MEA assembly as a parallel path to the MEA preparation procedure developed by LANL.

## Project # FC-092: Investigation of Micro- and Macro-Scale Transport Processes for Improved Fuel Cell Performance

Jon Owejan; General Motors




### Brief Summary of Project:

The core objectives of this project are to: (1) develop a validated transport model including all component physical and chemical properties; (2) disseminate information about the model and instructions for using the model to the public, primarily through the project website; (3) compile data generated in the course of model development and validation to guide model physics, and publish it on the project website; and (4) identify rate-limiting steps and recommendations for improvements to the plate-to-plate fuel cell package.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.7** for its relevance to DOE objectives.

- Understanding transport and transport processes is critical to the commercialization of polymer electrolyte membrane (PEM) fuel cell systems for transportation applications.
- This project legitimately addresses multiple barriers, including performance, water transport, system management, and transient operation.
- It is very relevant to the goals and objectives in the Fuel Cell Technologies Program's Multi-Year Research, Development and Demonstration Plan. This program is important as a basic analytical and design scheme between materials and performance.
- The project is firmly in line with DOE goals and objectives with respect to understanding (water) transport in PEM fuel cells and its effect on performance and durability.
- This project is relevant and the results will be useful to the fuel cell community, but it is not clear that stack manufacturers or automotive fuel cell companies will be able to use these results. There is significant variation in the materials and design of stacks, and the one-dimensional models are not easily transferrable to other fuel cell stack systems.
- This project is well aligned with the DOE Hydrogen and Fuel Cells Program. Of key value is the ability to link changes at the membrane electrode assembly (MEA) structure level for low-cost design concepts to changes in sensitivity to the operating design window, both in terms of absolute performance and current re-distribution at the cell level. This allows industry to conduct the relevant trade-offs to understand technical feasibility and cost of each of the different system components (including the stack).

### Question 2: Approach to performing the work

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

- The approach is almost perfect. Based on the experimental results, MEA performance is well predicted.
- This project employs characterization (which is mostly in situ transport measurements) to develop a down-the channel model.
- This is an ambitious but impressive attempt to measure and model all component resistances and overpotentials in the stack. If successful, this would be the definitive PEM fuel cell model.

- Extensive ex situ characterization and parametric in situ testing support the modeling effort for auto-competitive material sets. Most of the modeling effort is based on actual component data, such as thermal conductivity and component diffusion. The results are posted on a public website and component properties are public.
- The approach, as described, was very clearly laid out with responsibilities for each of the collaborators as well as when deliverables are expected to mature. One barrier that was assumed to have been addressed was "start-up and shutdown time and energy/transient operation." It was not clear from the presentation material how this barrier was being dealt with; indeed, in the supplemental notes it was made clear that durability was not a focus. Transient operation was not described in any detail and neither was the expected impact of transient operation on low-cost designs, although some assumptions could be derived from the detailed assessments made in this body of work. Of particular note was the modular, bottoms-up approach to model development and experimental design where individual component elements could be investigated in the absence of other noise or design factors.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.8** for its accomplishments and progress.

- The experimental measurement techniques are exceptional. A huge database is also helpful for validation. This can be a model case of an MEA design scheme.
- This project has produced a significant amount of data. However, there is so much data and so little description about the data that it is difficult for a person outside the project to understand most of it.
- The model is based on physics and component properties, not fitted parameters. Extensive data are openly provided on components and their properties, and the model accuracy is reasonable. Parameters needing improvement have been identified and are being addressed.
- Components—such as the microporous layer—that are critical in determining the mass transport have not been evaluated and reported. It would be useful for ex situ characterization to be carried out, and details of the techniques and experimental setups should be made public.
- This projected completed baseline validation steps over two current densities and four temperatures and gained a comprehensive understanding of water permeability and film thickness effects on water motion. The project is achieving a comprehensive understanding of platinum (Pt), including loading, roughness, structure, and oxide coverage. The researchers did nice work with understanding water in the gas diffusion layer.
- This project has made great progress so far. The excellent experimental data sets help with model development, which has been progressing well given the timeline of project. Some improvement (see slide 22) would be desirable in the future, but overall the project is on track. It is critically important to underline the importance of structure-property relationships as developed in such an excellent way by this project team.

### Question 4: Collaboration and coordination with other institutions

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

- This project has good, productive collaborations.
- The partners appear to be playing their positions on the team well.
- This project has excellent collaborations with component suppliers, national laboratories, and universities.
- The material supplier coordination seems clear. It appears there are small interactions with the university subcontractors.
- The team consists of automotive fuel cell users, materials suppliers, academic modelers, and characterization experts.

### Question 5: Proposed future work

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

- The future work plan is very consistent with project objectives and deliverables.
- The material set for auto competitive is not justified, especially the choice of 15 wt.% Pt/Vulcan and I/C = 0.7.

- It appears that the project will be completed on time and the proposed future work all looks feasible. Most issues may occur in the two-phase water model.
- There are a few details in the future work section. The work on thin catalyst layers has not been addressed to date. It is difficult to ascertain exactly what type of characterization is going to be done.
- A second data set will be verified and the model will be improved by incorporating more physical component behavior data and specific component models to address known issues.
- The future plan is basically OK. In order for more accurate predictions, other measurement techniques in catalyst layers should be studied. This can be helpful when the model cannot predict experimental data, because there might be other key parameters related to kinetics/mass transport that are not identified yet (e.g., ionomer coverage on Pt, announced by Nissan).

### Project strengths:

- This project is organized, systematic, and executed with engineering rigor.
- This project has good in situ testing diagnostics and correlation to operation.
- This project has a beautiful synergy between experiment and model and an extremely strong team.
- The access to characterization tools and technical data set generation capabilities are strengths to this project. This will lead to the development of good building blocks for other work, once complete.
- The exceptional experimental measurement techniques, quantitative analysis of the correlation between measured effective properties, and overpotentials/I-V performance are all strengths of this project.

### Project weaknesses:

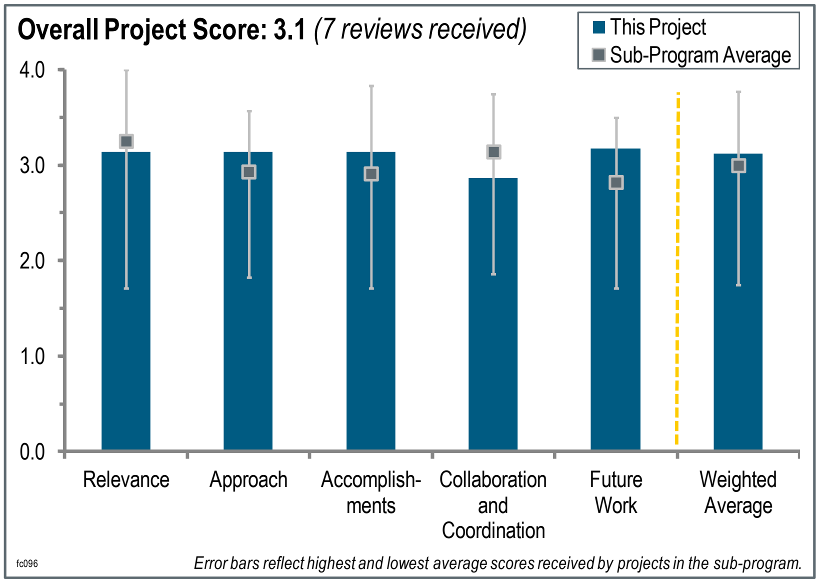
- This project currently has no weaknesses.
- The material set for auto competitive is not justified, especially the choice of 15 wt.% Pt/Vulcan and I/C = 0.7.
- Even at project end there will probably be some regimes of fuel cell operation that cannot be adequately explained by approach, but that is the nature of complex systems.
- There is no real linkage to the actual duty cycle cases, either stated or implied (i.e., there is not enough of a real-world spin on operational sensitivity, because most issues with low-cost MEAs are expected to occur at the extremes of the operation).
- The component characterization appears mainly in testing the transport properties in test stations. There is little in the way of information on how the surface structure affects the transport properties or correlation of fundamental properties to the transport, such as PSD. It is unclear how the photoacid dye information is being used, or what it means. There is a lot of terminology used in this presentation that is not defined, and it is difficult to determine what it is. It appears that the 1-d modeling case has a ways to go before the prediction of HFR is good.

### Recommendations for additions/deletions to project scope:

- This project should include ex situ measurement system setups on the website.
- The researchers should develop the measurement techniques in the catalyst layer.
- The project team easily has the capability to extend the study to include durability/duty-cycle aspects that are vitally important to DOE’s overall objectives. If this is not done in this project, it is important to link this to other groups where durability is a focus.
- Including thin catalyst layers, such as NSTF, and how the transport varies would be a valuable addition to this project. The graphs presented significant data, but there was not enough explanation and what was being measured was not completely understood (i.e., the multitudes of cases on various graphs). More detail is needed to make much of this information meaningful.

## Project # FC-096: Power Generation from an Integrated Biomass Reformer and Solid Oxide Fuel Cell (SBIR Phase III)

Quentin Ming; InnovaTek




### Brief Summary of Project:

Objectives of this project include the following: (1) establish a design to meet the technical and operational need for distributed energy production from renewable fuels; (2) design, optimize, and integrate proprietary system components and balance of plant in a highly efficient design; and (3) demonstrate the technical and commercial potential of the technology for energy production, emissions reduction, and process economics.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.1** for its relevance to DOE objectives.

- This project fully supports DOE Hydrogen and Fuel Cells Program (the Program) and research, development, and deployment objectives.
- Biomass reforming integrated with a fuel cell is doubly relevant to the DOE Office of Energy Efficiency and Renewable Energy's mission. The applicability of the fuel processor technology to fuel cells other than solid oxide fuel cells (SOFCs) is not addressed.
- This project strongly supports the Program's goals and objectives with a focus on the use of non-petroleum renewable fuels such as biomass. There is an emphasis on meeting DOE performance and cost targets.
- This is a relevant project from the point of view of reforming hydrocarbons for use in fuel cells.
- The project appears to be addressing the issues of cost and durability. The project is not supporting the stated objective of shifting from fossil fuels to non-food biomass.
- The project is developing technology to produce electric power efficiently from domestic renewable resources and advancing fuel cell technology without relying on hydrogen ($H_2$) infrastructure.
- The project supports the Program's goal of achieving $H_2$ production from diverse domestic sources with distributed power demonstration by the second quarter of 2018. The project also supports fuel cell goals.

### Question 2: Approach to performing the work

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

- The project focuses on critical issues and system demonstration.
- A systemic, logical approach involving design, review, and confirmation, as well as prototype testing, has been adopted for this project. It is heavily hardware oriented. The approach is very much results directed.
- This is primarily a straightforward engineering and design project to integrate a biomass fuel processor with a fuel cell. The individual subsystems already exist. No scientific breakthroughs are required. Modeling and simulation are effectively utilized. Process economics will be analyzed as part of the project. Transients are not being considered at this time.
- This is a rational engineering project that makes good use of DOE's resources. The focus on natural gas makes a lot of sense. Some pushback was registered during the meeting on this, which is unfortunate. Natural gas is

part of the President's all-of-the-above strategy, and it makes a lot of sense to consider it in the present circumstances.

- The approach to date has addressed cost well, and it is currently addressing performance. Durability has not yet been addressed significantly. The project team should use design for manufacturability much earlier in the design process so that the design starts with these principles in mind instead of trying to rework the design for lower cost later.
- The research is appropriately focused on the critical barriers of fuel processing catalysis, start-up transients, systems integration, and cost reduction. However, SOFCs are capable of internally reforming $CH_4$, and significant system efficiencies could be obtained if they optimized the fuel-processor-SOFC system for only partial external fuel reforming rather than a complete conversion to syngas.
- The overall approach of establishing a design to meet technical and operational needs for distributed energy production from renewable fuels should overcome the barrier of using SOFC power with renewable non-food biomass fuel. The technical objective is good: (1) develop a reformer that generates $H_2$ from non-food biofuels; (2) develop a highly efficient processing design of an integrated SOFC and fuel processor; and (3) prove the technology in a long-term field demonstration with a utility partner. The approach successfully used design reviews and third-party reviews in go/no-go decision points for manufacturing and assembly to lower costs. The design uses an efficient InnovGen fuel processor and Topsoe fuel cells. However, the fuel cell does not have an extensive testing record. The testing time of the 1.2 kW unit was not discussed.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project has achieved reasonable accomplishments, given the budget.
- The team has made excellent progress in system operation demonstration with biomass reformates.
- The project seems to be on schedule. The integrated system did indeed produce 1.2 kW grid power from biofuel and operated stably for about an hour, but no fuel cell performance data was presented.
- Progress has been good thus far, in the first third of the project. Technical milestones have been met. The initial, long-term testing of bio-kerosene reforming has been very successful, and the operation on biofuels producing power is impressive.
- The first-generation system appears to have proven itself out, but the real test will be with the second generation system, especially for durability. Process models were used to assess thermal stresses, which affect the durability, and operation, which affects the performance, of the InnovaTek reformer.
- The researchers are making good progress and have achieved milestones; however, the critical technical milestone (M4) to achieve 40% system efficiency is a few months after the Program's Annual Merit Review (July 2012), and as of now they have only achieved 27.5% (slide 31).
- The researchers' accomplishments are significant. They used simulation and modeling to successfully develop efficient and thermally integrated superior component/system designs with a microchannel heat exchanger and a fuel burner. They also developed an optimized catalyst for biofuel reforming, and fabricated and integrated proprietary system hardware, software, and catalysts. Additionally, they demonstrated 1.2 kW power from bio-kerosene and sent power to the grid for an undisclosed time.

### Question 4: Collaboration and coordination with other institutions

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

- The project features collaborations with several companies and organizations.
- The team comprises a qualified entity for each of the subsystems.
- Collaborations have been excellent, including subcontractors and partners. The involvement and support of student interns from local universities is a very positive aspect of the project.
- The project received good input from the city of Richland. There should be more evidence that Topsoe is engaged.
- Although there are several external partners, it is not clear that InnovaTek is really collaborating with any of them.

- There are a number of good collaborations on fuel processing. However, it might have been preferable to collaborate with one of the few U.S. SOFC companies.
- The project features collaborations with Topsoe Fuel Cell; the city of Richland, Washington; Washington State University; and Pacific Northwest National Laboratory for biofuel energy technology services.

### Question 5: Proposed future work

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

- Strengths of this project include its well-defined future work and work focus.
- The project features a logical, reasoned approach.
- Plans for future work are consistent with and supportive of the original project plan. Revision of the original goals and objective are not necessary, and the remaining technical and cost milestones are adequate to measure progress in the future.
- Analyzing the process economics is important, although the assumption of a 20-year life is questionable. For a continuous power system, this is four times longer than 40,000 hours of operation.
- The proposed future work is appropriate and should build on current performance by decreasing parasitics and improving integration efficiency.
- The planned future work is appropriate: pursue further technology improvements and system optimization, add additional fuel cell collaborators, perform analysis of process economics, and perform field demonstration and long-term operation.

### Project strengths:

- This project’s strengths include its strong, motivated technical team and its supportive collaborators.
- The project’s strengths include its development of useful technology and focus on natural gas.
- InnovaTek has demonstrated integrated system performance that generates power.
- The plans and targets are well defined. Other strengths include the system approach and focus on addressing key issues.
- Strengths of the project include how it does not rely on an $H_2$ infrastructure to advance fuel cell technology, and the resultant efficiency gains.
- The project has strong collaborations and integration and system evaluation components.

### Project weaknesses:

- There is little evidence of tight coupling with the fuel cell provider.
- The team should work with a U.S. SOFC manufacturer.
- The project appears to have abandoned fossil fuel displacement and renewable energy generation in favor of using relatively cheap natural gas as the fuel. It is not clear, though, whether InnovaTek’s natural-gas-fired system would be competitive with an internal combustion engine (ICE) combined heat and power system, much less utility-scale gas turbines.
- The team needs to focus more on integration of the two key components of the system—the reformer and the SOFC.
- It is unclear whether camelina is a sustainable supply of biodiesel. May be possible coking or plugging with a liquid fuel and the microtubular reactor, although an atomizer may solve this problem. The 67% fuel utilization is low and needs to be improved.

### Recommendations for additions/deletions to project scope:

- Perhaps it would be interesting to consider internal reforming in place of the external reformer for some of the biofuels or for natural gas.
- The team should develop a better relationship with a fuel cell company.

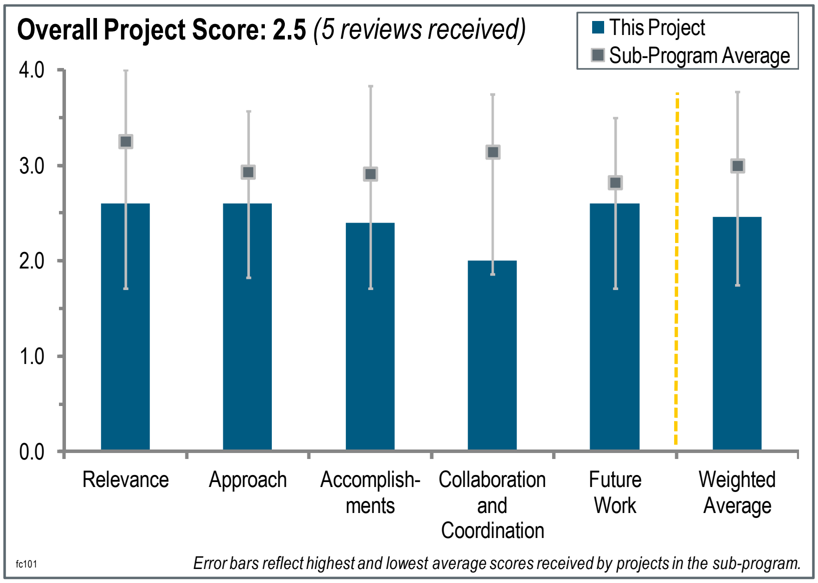
- It would be good to know what market conditions are required for wider acceptance of a biofuel-fired distributed generator, and what the target lifetimes are of such systems, especially as compared to the expected lifetimes of ICE generators.
- The researchers should optimize the fuel-processor-SOFC system for only partial external fuel reforming, rather than complete conversion to syngas.
- The researchers need to use a Versa fuel cell. It has better performance and a better test record.

## Project # FC-101: PEM Stationary Power Plant

Tom Skiba; UTC Power

### Brief Summary of Project:

The project goals are to: (1) investigate the feasibility and value proposition of a 150 kW high temperature polymer electrolyte membrane (PEM) stationary fuel cell operating on natural gas reformate; (2) project durability and reliability of PEM fuel cell components; (3) conduct a preliminary systems analysis of a PEM power plant capable of achieving greater than 45% electrical efficiency; and (4) demonstrate an advanced fuel processing breadboard system capable of delivering a hydrogen - rich, low-CO (less than 10 ppm) reactant stream to the PEM stack.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.6** for its relevance to DOE objectives.

- Demonstrating a 150 kW PEM stationary power facility is highly relevant.
- This project is confusing. It seems very long (started in January 2004) and very expensive ($11.6 million from DOE) for the planned results (only a model).
- The project addresses the technical targets for 2017 listed in table 3.4.6 of the Fuel Cell Technologies Program's Multi-Year Research, Development and Demonstration Plan, and is therefore relevant to overall DOE objectives.
- It would be better if UTC Power could use their own resources to determine whether a 150 kW stationary PEM system is feasible and a good value proposition for the company. It is not clear that this project will have a significant benefit to the DOE Hydrogen and Fuel Cells Program.
- The appropriateness of this system is not completely convincing. UTC Power is already working on a very low-cost stack phosphoric acid fuel cell (PAFC), which is integrated with their systems. The market economics for PAFC systems heavily depend on their ability to generate useful (high-quality) waste heat. In this embodiment, the system cost "might" be marginally lower, because the stack is a lower-cost technology. However, cost may also increase, because this stack technology relies on more reformate cleanup. The system will not generate useful heat and any improvement in capital cost may be lost in lower revenue (no heat output). The market pull needs to be identified before jumping into such a system. The target market is unclear and there are numerous companies that developed PEM combined heat and power systems, only to find marginal sales (even when the product price was subsidized by shareholders and the government).

### Question 2: Approach to performing the work

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

- This project seems like a reasonable approach, although not much is given about the details.
- The technical approach for this system design is impeccable and reflects UTC Power's ability to design and implement systems. The system is still in its early stages of development, and comments can only be made on the system process design.

- The approach in this project is mainly analysis, with some small single-cell tests planned. It seems that the industry has progressed well beyond this for stationary PEM systems, and evaluation of integrated hardware would be of much greater value.
- Investigating the feasibility and value proposition as a first step is a good approach; a go/no-go decision point. It is not clear that all of the milestones remaining can be reached in 2012, given the percentage complete as detailed on slide four. There is no integration with other efforts identified in the presentation and there are no technical collaborations identified on slide 21. While this may be due to intellectual property issues, use of the resources of other entities might help progress.
- The approach of using perfluorosulfonic acid (PFSA) membranes and trying to generate steam for the reformer from the stack is questionable. The energy penalty associated with compressing the low-pressure steam will be significant, and capital and maintenance cost for the steam compressor will drive up the cost of ownership. A better approach would be to use a membrane capable of operating significantly above 100°C, but this would probably have to be something other than PFSA.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The system design has been performed, and technically the system is feasible.
- There do not appear to be a lot of accomplishments and progress for a project that began in 2004.
- The project started in 2004, and it appears the technical feasibility is the only milestone achieved in eight years. The progress has not been addressed in the brief, and it was difficult to address the accomplishments and progress. There were no timelines provided upon which to gauge progress.
- This project seems to have had many stop-starts throughout its history, and the continuity of results reflects that. The data and plots provided in the poster were pretty basic analyses and could be accomplished with much less investment than the cost of this program. Ignoring this for a moment, the results themselves do not give high confidence that the system proposed can meet the technical targets.
- The project has not been running very long since reinitiating. The attempt to project a membrane lifetime of 23,000 hours based on a few hundred hours of testing is not credible. The feasibility study seems to have led to dubious possible system designs (recompression of low-pressure steam).

### Question 4: Collaboration and coordination with other institutions

This project was rated **2.0** for its collaboration and coordination.

- No partners were listed.
- There has been no collaboration reported.
- No collaborations or interactions are indicated.
- UTC Power might benefit from collaboration with partners that have expertise in compressors, but no collaborations are strictly needed for this project.
- As a system designer, UTC Power should be communicating and catering to its market. It is unclear who that is, and what the “voice of the customer” would ask for, in light of other UTC Power product offerings.

### Question 5: Proposed future work

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

- The future work looks reasonable.
- The future work is appropriate, though it seems like a lot to get done between now and December 31, 2012.
- The work on desulfurization is valuable to the industry; however, the plan shown on slide 20 is unclear and lacks crisp, well-defined deliverables.
- There seems to be a lot more future work required than just developing an advanced fuel processing system. The project plan is not clearly defined; it looks like the researchers will need an extension.
- UTC Power would march forward and demonstrate the system; however, this reviewer’s industrial experience indicates that UTC Power needs to step back and examine its target market and target system specifications.

### Project strengths:

- This is a very technically and scientifically strong team.
- UTC Power has significant experience with the PEM fuel cell system.
- Developing a high-temperature PEM stationary system is a difficult challenge. A systematic approach to the various project elements is a good plan, although it is difficult to see this from the briefing.
- This project has no strengths.

### Project weaknesses:

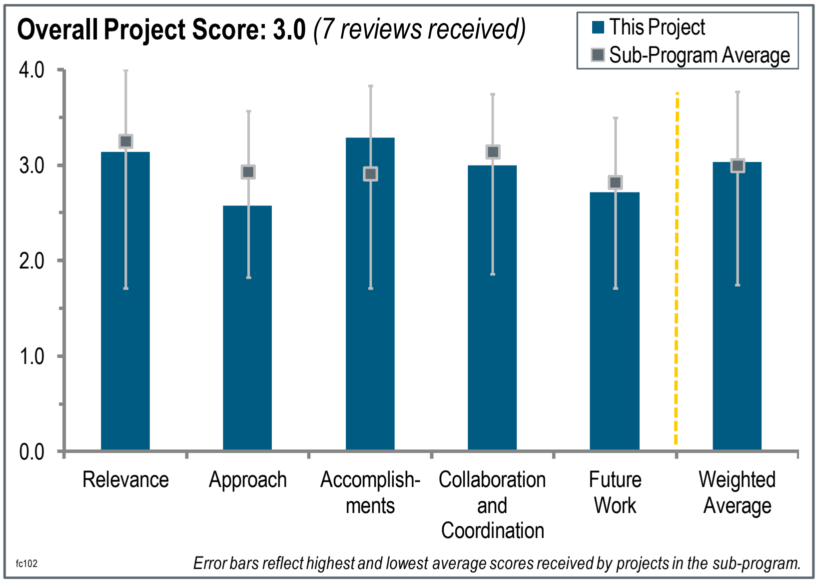
- This project needs market analysis (or needs to show existing market analysis).
- This project does not seem like it has accomplished a lot for the money that has been spent.
- This project has an unbalanced approach (too heavy on analysis, not enough testing), discontinuous history, unpromising results, and a low value-to-cost ratio.
- The poster did not convey this project's progress, so it is difficult to judge. The progress of this project appears poor, given the time and budget invested; the project started in 2004. It seems that technical feasibility is the only milestone achieved in eight years.
- This project is not exploring a new application space. PEM fuel cells have been around for so long, and it is unclear why UTC Power is deciding now to investigate this stationary application. It may have made more sense back in 2004 when the project originally started than it does now.

### Recommendations for additions/deletions to project scope:

- Close this project as soon as practical.
- Because the project end date is December 31, 2012, it does not make sense to change the scope with so little time remaining. This project should not be extended beyond 2012.
- The researchers need to show what the market pull is for this type of system; how big the market is; and if the system makes sense, even if it accomplished $3,000/kW. It is unclear whether this system can offer something more than its big PAFC brother. If this system had a good market pull, then it seems unnecessary for DOE to provide funding for UTC Power to pursue it. UTC Power should spend more effort on this system with its own funding.

# Project # FC-102: New High Performance Water Vapor Membranes to Improve Fuel Cell Balance of Plant Efficiency and Lower Costs

Earl Wagener; Tetramer Technologies, LLC




## Brief Summary of Project:

Tetramer aims to design and develop high-performance, low-cost water vapor membranes for cathode humidification in fuel cells. Technical objectives are to: (1) demonstrate a water vapor transport membrane with >18,000 gas permeation units (GPUs), (2) develop a water vapor membrane with less than 20% loss in performance after the General Motors (GM) stress test, (3) limit the crossover leak rate to less than 150 GPU, (4) design temperature durability of 90°C to excursions of 100°C, and (5) limit the cost to less than $\$10/m^2$ at medium volumes.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.1** for its relevance to DOE objectives.

- Low-cost water vapor membranes to reduce the cost of the humidification system/fuel cell electric vehicle balance of plant fit well with the overall DOE objectives.
- The development of high-performance materials for humidifiers is a very important topic to be addressed to enable the introduction of fuel cells.
- The project is focused on one narrow aspect of fuel cells—namely, membranes to control hydration. While this is very important, it is not as broad as some of the other DOE-funded activities.
- The project will help to achieve better water stack management with existing materials. DOE is investing in new ionomers for hot and dry operation, so it is not clear that a humidifier will always be needed.
- Improved membranes for water recycling in polymer electrolyte membrane (PEM) fuel cells lead to cost and durability improvements. Current membranes exhibit adequate performance, but they are expensive. A lower-cost membrane for water management would provide a modest cost reduction.
- This work is pertinent to hydrogen (pure or reformate) and oxygen (from air) PEM fuel cells that need hydration, which is the PEM technology presently favored for automotive fuel cell power sources. This technology could help reduce the size and weight of this kind of fuel cell. This technology is not critical for high-temperature (HT) PEM fuel cells and solid oxide fuel cells.
- It is a bit unclear what the target truly is, and how it relates to DOE objectives. It is also unclear if the membrane is for HT application, humidifiers, or fuel cells. The project seems related to novel membranes with low cost.

## Question 2: Approach to performing the work

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

- This work was a short, six-month Phase I, and under those constraints the principal investigator (PI) demonstrated a very efficient and focused effort to come up with a feasible membrane that shows great potential.

- It is difficult to evaluate the approach because it is proprietary. The reputation and experience of the researchers does foster a sense of confidence that they know what they are doing.
- It is hard to really describe the approach. Targets are clearly set, but how Tetramer Technologies is trying to achieve these targets is a mystery.
- It is not clear what criteria are used to drive material development. The approach seems to be exclusively experimental, based on trial and error. It is recommended to increase analytical support to experiments and to make it visible in the course of the next reporting period.
- This approach of using a water permeation membrane based on Henry's law and molecular void space is reasonable and desirable, as opposed to using inefficient and bulky mechanical humidifiers. It is hard to say that this approach is outstanding, because the authors could not share the chemistry of the membranes due to intellectual property issues. This work should be continued if the chemistry makes sense to "those in the know." The authors should file their patents or patent disclosures before next year so progress can be reviewed fairly and objectively.
- It is not clear whether allowing a huge amount of water through the membrane will help issues beyond water management. The materials presumably swell heavily and may have compromised mechanical integrity when wet.
- The approach seems fine, but without knowing anything about the membrane chemistry it is hard to judge the approach. The focus on gas permeation is a bit confusing with regard to whether it is a key metric or not. Also, it would be interesting to know about liquid water transport.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The progress in the development of the work has been very good. Degradation rates need to be decreased; however, the candor of the PI was appreciated.
- These early results show achievement of critical flux rates and lifetime under accelerated protocols.
- Comparing the conductivity and IV performance of Tetramer Technologies membrane versus Nafion® 1000 is very competitive. The project is making steady progress toward gas permeation targets based on various generations. Durability, however, still has room for improvement.
- The team has made good progress in the area of performance. Significant challenges still have to be addressed in the area of durability. The presenter mentioned that seven different methods have been identified to reduce degradation, but no details were provided.
- This is new work, and the fact that the water permeation is better than in Nafion® for 200 hours is a good start. A key issue is to resolve the purification of materials, as the authors themselves stated, so a rational design of experiment can be done and discussed next time.
- The researchers have met the gas permeation goals. It is hard to comment on the accomplishments when the membrane chemistry/morphology is not revealed at all. The durability is very poor; it is good that Tetramer Technologies has identified several methods to mitigate the problem, but because these were not discussed, it is very hard to comment.
- The team accomplished good results in terms of permeation and conductivity. However, not all humidity ranges and temperatures were reported. The researchers seem to be reaching their milestones, although some more data about the experimental conditions is necessary.

### Question 4: Collaboration and coordination with other institutions

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

- High-level partners are in place to build and evaluate prototype modules when the down-selected materials take place. At this time, no real interaction has occurred.
- The collaboration with GM is clear, and the roles of Ballard and Membrane Technology Research will become more evident in the following phases. In general, the team is well composed with two strong industry representatives.
- Most of the collaboration to date has been with GM. Phase II, if granted, would invoke additional partners. However, the GM collaboration has proved important in guiding the desired results.

- The collaboration appears to be validation testing from an original equipment manufacturer (OEM). Few details were provided.
- The authors are collaborating with GM and Ballard, which are leaders in this PEM technology and can guide the team and validate if the team is making significant impact.
- All collaborators are industrial—either component manufacturers or OEMs. It would be appropriate to include a national laboratory partner.
- For the size of the project, there is good collaboration, although independent testing of the membrane is expected.

### Question 5: Proposed future work

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

- The proposed future work is hard to evaluate due to the proprietary nature of the project.
- Although more details on the future work would have been beneficial, the plan seems to address the major challenges. It is aimed at validating Phase I findings in real environments.
- The future work is dependent on a Phase II. Some issues to consider would include the ease of scaling up beyond the sheet casting that the PIs discussed, and developing a packing geometry appropriate for automotive applications.
- The authors are correct, and future work should include using a known structure to develop a new, better one, and scaling up for prototype testing. The researchers should include purification of a monomer for optimal synthesis (they said this in the body of the presentation), and also file patents so they can discuss the work in more technical detail.
- Scale-up of the films is highly desirable for testing at component suppliers and OEMs. It is still not clear whether the trade-off between performance and durability can be achieved when the films barely reach the performance target.
- The project plan seems good, but it is tough to say because gains in durability, etc. will depend on chemistry. No specifics were given about other characterization tasks.
- The proposed future work is: "Using structures developed in Phase I, fabricate membranes and optimize tradeoff in performance and durability. Scale up to provide partners with membranes for prototype testing." There is no way to gauge what that actually means.

### Project strengths:

- The project's strength is the low cost of the membrane.
- Tetramer's strong partnership with industry members is an area of strength.
- The team's strong knowledge of separation membranes and familiarity with the chemistry allowed for quick progress to achieve the target water flux goals.
- The project's strength is the world-class polymer scientist leading the team.
- This is a strong academic and industrial team that has all of the resources to achieve its goals. The academic team is affiliated with Clemson University, a university with proven know-how for achieving new, high-quality membranes. The team should leverage the "Clemson connection" as much as possible.
- The novel polymer chemistry is an area of strength for this project.
- The supposed low-cost membrane that performs as well as Nafion® is a strength of this project.

### Project weaknesses:

- It is unclear what the technology is about.
- The project lacks an analytical approach to accelerating the experimental findings. The roadmap to identify the trade-off between performance and durability is unclear.
- The highly proprietary nature of the project makes it very difficult to evaluate. There are no reports or publications to even assess the polymer chemistry used.

- It is not clear how to improve durability and optimize the membrane for permeability, because the composition and structure were not discussed. The researchers should file patent disclosures as soon as possible, or else they will be inhibited to discuss the work and will get limited help from the outside team.
- The approach is possibly too empirical and needs scientific input from a university or a national laboratory.
- The chemistry is not specified, so it is hard to say what is going to happen with durability, expected cost, etc. Some justifications are needed for the specific experiments chosen to be metrics for the membrane targets.

### Recommendations for additions/deletions to project scope:

- The project scope is adequate.
- Humidifier housing can play a significant role in defining performance and durability. The team should investigate this aspect as part of the following phase.
- Recommendations include funding for Phase II and a focus on the packaging/scale-up.
- The researchers should include purification of a monomer for optimal synthesis (they said this in the body of the presentation), and also file patents so they can discuss the work in more technical detail.
- The team should make sure that all DOE membrane metrics are being met or examined, and test other transport properties under a whole range of conditions.

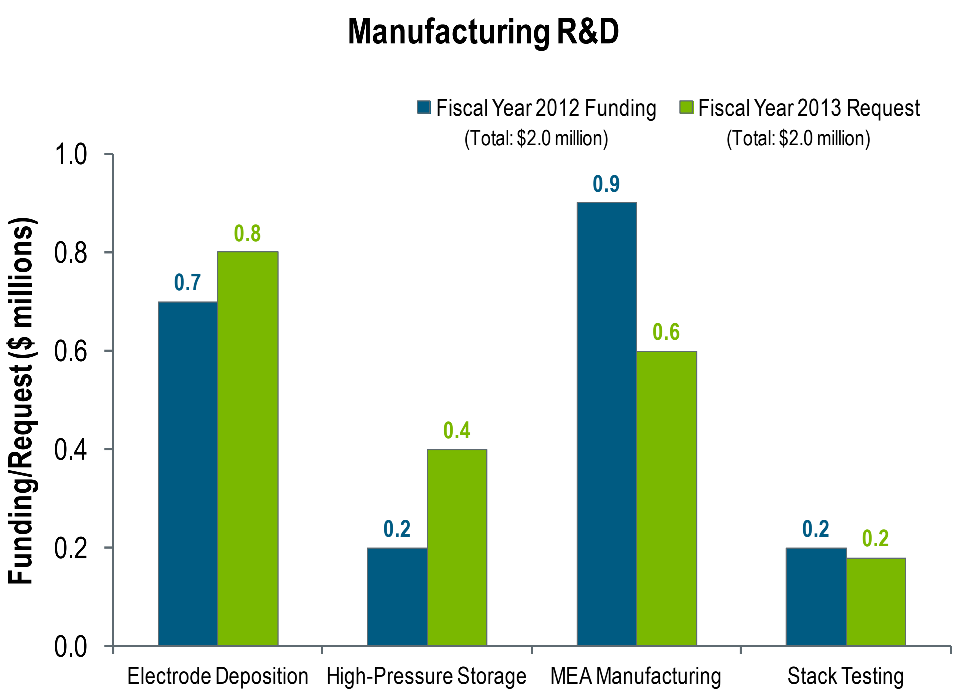
# 2012 — Manufacturing Research and Development (R&D)
# Summary of Annual Merit Review of the Manufacturing R&D Sub-Program

## Summary of Reviewer Comments on the Manufacturing R&D Sub-Program:

The Manufacturing R&D sub-program was judged to be well managed and well executed. Reviewers stated that the overall goals and objectives of the sub-program were clearly defined and that the projects in the sub-program were highly relevant to these goals and objectives. Reviewers also stated that the manufacturing sub-program projects are rather mature, well-conceived, and are delivering quality results. Reviewers noted that the issues and challenges that remain have been clearly identified and a plan exists with the only limitation being availability of funding. In fiscal year (FY) 2012, six manufacturing projects were reviewed. These projects addressed fuel cell membrane electrode assembly manufacturing, fabrication of catalyst-coated membranes, fuel cell stack in-line testing, and manufacturing of high-pressure vessels for hydrogen storage. Reviewers noted that there were no projects for low-cost, high-volume manufacturing of gas diffusion layers (completed in FY 2011) and no projects to facilitate high-volume assembly of stacks.

## Manufacturing R&D Funding:

Funding for the Manufacturing R&D sub-program was $2 million for FY 2012 and $2 million was requested for FY 2013. The FY 2013 request level funding will continue existing manufacturing R&D projects. The ultrasonic membrane electrode assembly (MEA) sealing project has been completed.



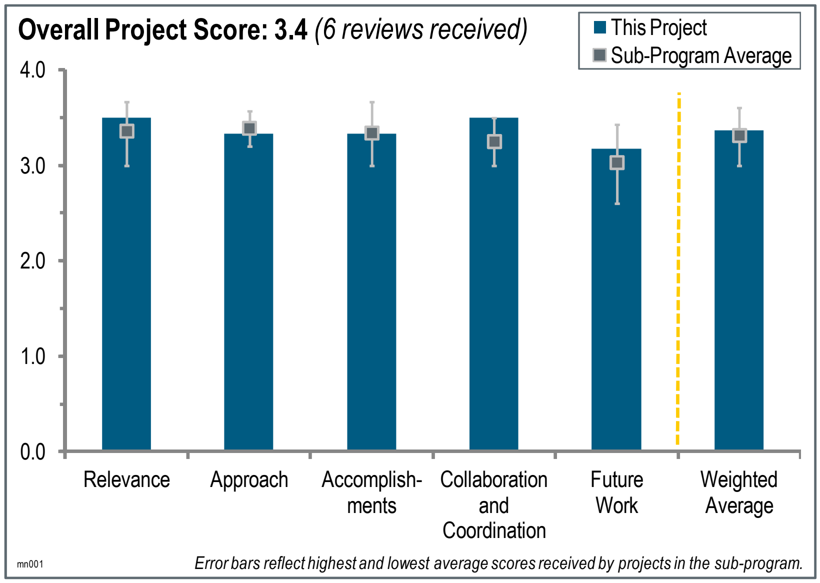

## Majority of Reviewer Comments and Recommendations:

Six Manufacturing R&D projects were reviewed and the maximum, minimum, and average scores for the projects were 3.4, 3.0, and 3.3, respectively. All projects were judged to be highly relevant to the DOE Hydrogen and Fuel Cells Program's activities, with very good technical approaches. Project progress and accomplishments were judged to be extremely good. Project teams were judged to be strong; participation and contribution from industry partners was judged to be useful and coordinated. For most of the projects, reviewers felt that more details needed to be presented for future work.

The highest ranked projects (3.4) were considered by the reviewers to be highly relevant, with an excellent approach, outstanding accomplishments, and strong technology transfer and collaborations. The reviewers wondered if the principal investigator of the project with the lowest score (3.0) fielded sufficient test articles to provide an adequate number of samples to achieve statistical significance.

## Project # MN-001: Fuel Cell MEA Manufacturing R&D

Michael Ulsh; National Renewable Energy Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) evaluate and develop in-line diagnostics for membrane electrode assembly (MEA) component quality control (QC), and validate in-line; (2) investigate the effects of manufacturing defects on MEA performance and durability to understand the accuracy requirements for diagnostics; and (3) integrate Lawrence Berkeley National Laboratory's (LBNL's) modeling to support diagnostic development and implementation.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- The National Renewable Energy Laboratory (NREL) works to improve manufacturing and QC of Nafion®-based MEAs and gas diffusion layers (GDLs). The emphasis appears to be on measuring and identifying defects, which is a good thing for a national laboratory to be doing.
- This activity is relevant in the overall scheme of things. The task consists of evaluating and developing in-line diagnostics for MEA component QC.
- This is a difficult project to get industry buy-in; it is difficult for companies to share their proprietary QC methods and their yields because yield goes into all cost models. This project seems to be doing a good job of walking that fine line.
- This project is very relevant to the goals and objectives of the DOE Hydrogen and Fuel Cells Program (the Program). This project specifically addresses in-line QC and defect evaluation, both of which are critical as manufacturing volumes increase.
- This project is focused on QC related to fuel cell manufacturing. It is very relevant to the Program. Defect identification is important to the economics of manufacturing fuel cell soft goods.
- MEA cost-reduction efforts are significant opportunities for polymer electrolyte membrane (PEM) fuel cell cost reduction in both the near and long terms.

### Question 2: Approach to performing the work

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

- This project is developing various diagnostics that can be used in manufacturing, including at least one diagnostic for all of the internal components (i.e., membrane, GDL, electrode, MEA). NREL is demonstrating the applicability of all these techniques on a manufacturing line speed. This work should be applicable to multiple types of manufacturers. They apply to PEMs and solid oxide fuel cells (SOFCs).
- The approach expounded upon appears to be rational. It would difficult to recommend an alternate approach that would demonstrate similar degrees of success.
- The approach is good, but this reviewer would like to see a broadening of the scope and/or future work to look at further facets of the manufacturing from assembly down to the catalyst level. Understanding these capabilities will give guidance to DOE where the gaps are and what the ultimate limits are in cost with manufacturing scale-up.

- The overall approach of this project has been good because it has had input from a variety of component manufacturers. Most common defects can be detected for the components of the MEA through the methods developed in this project. In addition, the segmented cell analysis can provide key information about the required manufacturing tolerances for these components, which will have a direct impact on cost.
- The approach is to develop and verify several diagnostic techniques that have the potential to be implemented in manufacturing lines. The potential cost savings for implementing any of the techniques is not addressed. An assessment of the minimum size defect that impacts performance needs to be determined. Testing with the segmented cell has shown that currently applied manufacturing tolerances of 2% of PTFE content are sufficient; a 4% difference did not affect cell performance. Similar analysis needs to be completed for the other diagnostics.
- The project's approach to the efforts are well thought out and planned and the effort is obtaining valuable direction from industry.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- NREL showed a web-line demonstration of optical and IR/DC diagnostics. The IR/RFT measurement appears to require significant work before it can be applied to a web-line.
- The progress on this project has been impressive to date. The detection level is excellent. This reviewer questioned what happens at higher speeds and what the maximum feed rate is, because there may be transient heat transfer issues.
- NREL has made excellent progress in systems for both coating quality and pin-hole detection.
- The results from this project so far are excellent. The optical and infrared diagnostics have been demonstrated at speeds and tolerances required for near-term component manufacturing. Significant progress has been made to improve the performance of the reactive flow-through technique. The initial segmented cell data are promising and should be helpful in determining the impact of defects both at the beginning of life and as the MEA ages.
- Reasonable progress is evident even with a budget reduction. The IR/DC diagnostic can be used to detect all scratches (up to 2 $cm^2$) and surface cuts (except those in the direction of motion) at line speeds up to 100 ft/min. In addition, invisible shorting defects were detected at speeds up to 30 ft/min. The IR/DC diagnostic appears to be the furthest along in development. An understanding of the detection limits needs to be developed and the minimum size defect that impacts performance needs to be established.
- Go/no-go decision metrics need to be established for the reactive flow technique, and the value of further work in the segmented cell needs better justification.
- NREL made good progress with the IR/DC diagnostic. Cuts and scratches in the direction of travel are problematic with this approach. Further characterization is needed for the IR/RFT approach to implement in-line.

### Question 4: Collaboration and coordination with other institutions

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

- Materials from a number of sources have been used in the project.
- Because the presenter did not show any results from the LBNL modeling effort, it is hard to know how that effort is helping the project.
- The list of collaborators is impressive.
- It seems that NREL is tied in to many of the major players. This reviewer suggests that NREL also assess the potential impact on the assembly process and, at the other end, develop techniques to look at particle-size distribution for catalysts. The collaboration on this project appears to be very good, involving key players in both industry and academia.
- Industry collaborations are good, and industry input is used to guide the program in fruitful directions. It is not clear if the funding for specific partner studies is provided by the partners or if the results will be of benefit to the broader fuel cell community. The value that the funded partners bring to the project is not very clear.
- The project incorporates a number of useful collaborators from both industry and academia.

### Question 5: Proposed future work

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

- Future work includes segmented cell studies, which are hard to justify in a manufacturing program. The PI stated that NREL will "perform aging studies to determine if failures develop at defect locations"; while the study may be interesting, it appears to be outside the "jurisdiction" of the Manufacturing sub-program.
- The proposed future work appears appropriate.
- The proposed future work appears to be focused in the right direction, but it lacks clarity. Additional details about other diagnostic techniques being considered would be helpful.
- The proposed future work plan lacks prioritization. It is unclear where limited resources can be best applied to achieve success for the most promising approaches. The work in the segmented cell needs much better justification before being carried forward.
- The project is incorporating further defect impact studies and cost-benefit analyses, which are essential for assessing the utility of various detection methods. Further detail regarding schedule and success metrics would be helpful.

### Project strengths:

- The project's strength is seen in its accomplishments so far.
- The project's strength is its focus on developing diagnostics for high-speed manufacturing processes.
- NREL is making good progress in a number of defect detection and impact areas and is expanding into other fuel cell technologies (e.g., SOFCs).
- The project strengths include using commercially available hardware from other industries for this specific application, such as simple transient heat transfer techniques with high-speed thermal imaging. This reviewer expects that the feed speed will be limited by variability in the component thermal capacitance and thermal conductivity and that estimates of maximum feed speed might be possible by analyzing the acceptable variability of these properties as compared to the resolution limits of the IR camera response.
- This project has clearly demonstrated the viability of the in-line detection tools under commercial operating conditions. There has been excellent synergy between NREL and its industrial partners to develop methods to meet industry needs. The addition of the segmented cell technology may provide critical data about the impact of specific defects on the performance and durability of fuel cells.

### Project weaknesses:

- It appears that the previously noted deficiencies have been resolved.
- The project's weakness is inherent in having an institution develop QC equipment for other companies while never being able to see the other companies' processes or be manufacturers themselves. Gap assessments are what is needed in judging technologies; for example, it is unclear if core-shell catalysts could ever be a manufacturable technology and, if so, at what cost.
- Although there has been great work done to demonstrate production speed, there has been no effort spent on examining production lengths. It is not clear if NREL could collect data on hundreds of meters of material and maintain accuracy and track defect locations effectively, or if there are other possible techniques that are already under consideration.
- Defects that do not cause an increase in temperature upon DC excitation will likely not be identifiable with this technique. The techniques have been developed and tested using induced defects. How these defects correlate with actual manufacturing defects is not clear.

### Recommendations for additions/deletions to project scope:

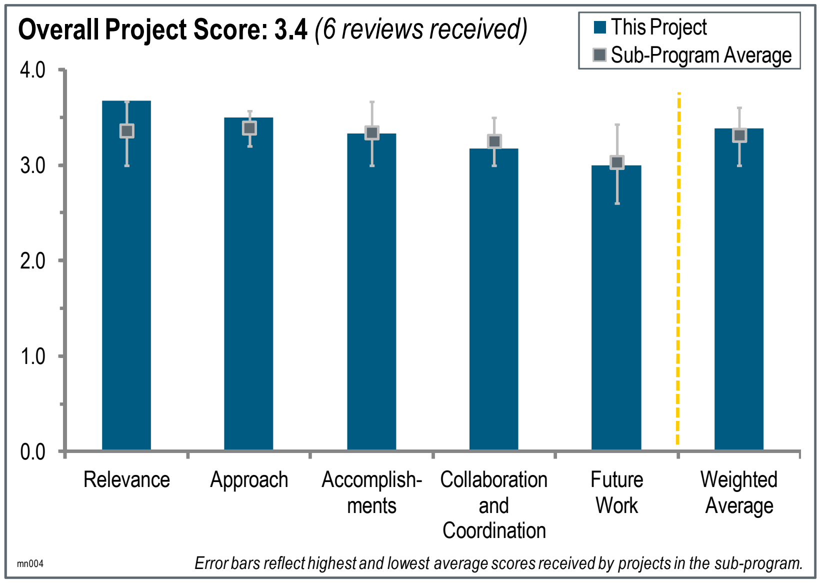
- This research should continue – it is nice work.
- Prioritize future work to account for the uncertainties in funding and to prepare to narrow the focus to the most promising approach.
- Segmented cell measurements can be a valuable tool for many purposes. However, for this project it seems to have limited applicability, as it could never be considered a manufacturing diagnostic. Aging studies using the

segmented cell should be deleted, because those studies are not part of a manufacturing study, and should be done under a durability program. This project should continue its development and feasibility assessments of in-line measurements for each of the components.

- Devising a system to do inspection of GDLs would be most helpful in determining caliber; uniformity of hydrophobic coatings and microporous layer coatings could be next possible interest areas. Some modeling of potential airborne defects and clean room requirements versus defect count and subsequent yield reduction might be nice to share with industry and those wanting to determine ultimate MEA cost.
- It would be helpful to understand how the location of the defect influences performance in the segmented cell. It may also be helpful to consider different flow-field geometries for segmented cell analysis. Some effort should be spent to determine if any of these tools could also be used for measuring the uniformity of critical component properties. There may also be some benefit to demonstrating some of the more developed techniques on an actual commercial production line. A prioritized list of defect types and sizes from each supplier may be helpful for coordinating the segmented cell analysis.

## Project # MN-004: Manufacturing of Low-Cost, Durable Membrane Electrode Assemblies Engineered for Rapid Conditioning

Colin Busby; W.L. Gore




### Brief Summary of Project:

The overall objective of this project is to develop unique, high-volume manufacturing processes that will produce low-cost, durable, high-power-density 5-layer membrane electrode assemblies (MEAs) that minimize stack conditioning. Goals will be achieved through modeling and experimenting with different configurations, including reinforced, 3-layer MEA layering, 5-layer heat and water management, and exploring a new 3-layer MEA process layering of cathode, reinforced membrane, and anode layers.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.7** for its relevance to DOE objectives.

- W.L. Gore (Gore) works to develop high-volume manufacturing for 5-layer MEAs relevant to automotive volumes.
- High-volume MEA and fuel cell stack manufacturing processes are critical to fuel cell affordability. Developing a direct coating process for the catalyst addresses a major cost driver in MEA manufacturing. Reducing the number and cost of coating passes will directly result in lower-cost MEAs. Minimizing stack condition time addresses a major fuel cell stack cost driver.
- The Gore project fully addresses one of the major challenges facing fuel cell commercialization, namely, cost. The process being developed by Gore is consistent with achieving the DOE cost target of $9/kW MEA in 2017.
- This project can support several DOE fuel cell research and development goals, but it is also a project with some risk.
- The development of a direct-coating fabrication process is essential to enabling the needed cost reductions, thus this project is a perfect fit.
- Reducing the cost of MEA production is an important function. The improvement has to be measured in terms of cost savings to the overall cost of the MEA, which would include materials costs and manufacturing costs. This reviewer questioned what percent of improvement is anticipated.

### Question 2: Approach to performing the work

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

- The elimination of backer materials, reduction of coating passes, minimization of solvents, and direct coating of membrane are all excellent approaches. It would be nice for more information to be shared, for example on slide 6, such as the environmental control required, and the line speed that is possible and has been demonstrated.
- The principal investigator (PI) has approached this work in a systematic, logical manner with appropriate go/no-go criteria at key milestones. The approach to reduce MEA costs and optimize durability by examining mechanical durability/power density trade-offs of the 3-layer construction is a very sound approach. Modeling of

mechanical stress and heat/water management (University of Delaware [UD] and University of Tennessee, Knoxville [UTK]) are both important aspects to achieving success in addressing cost and durability barriers.
- The approach to reducing MEA costs is to reduce the cost of intermediate backer materials, reduce the number and cost of coating passes, minimize solvent use, and reduce conditioning time. Gore has proposed and tested a number of innovations in the manufacture of MEAs that show promise for achieving the project goals. Testing at UTC Power will offer an unbiased evaluation of the success of this approach. The utility and added value of the two modeling efforts are rather unclear. Lifetime modeling of MEAs manufactured with the new process would be of value if it could take manufacturing defects into account.
- The approach combines the MEA low-cost backing fabrication method and modeling of the MEA and 5-layer structures to meet the project goals. These three efforts do have the potential to, in combination, develop a new process for low-cost, durable MEAs and 5-layer structures, as well as reduce stack conditioning time, but it seems that the risks of producing prototypes that meet all the goals are high, and the risk is not assessed.
- The approach nicely combines optimization through modeling with experimental validation versus reference targets.
- The approach is reasonable; it builds and expands the traditional manufacturing method. There was no discussion of scrap rates associated with the different forms of manufacturing.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project shows progress in the performance of direct-coated cathodes. However, there is little information about the process; thus, the only project that benefits from the developments is Gore.
- The cost model indicates that a 25% reduction in MEA cost is achievable and results support that estimate. This is primarily due to eliminating the use of backer material—for both membrane and electrode coating—and reducing scrap. Four sub-processes were eliminated in 3-layer roll-good finishing operations, which will favorably impact cost-reduction efforts. The mechanical modeling effort by UD appears to be of utility to Gore in understanding the impacts of potential manufacturing process changes. UTK work also appears to be of utility to Gore (5-layer heat and water management modeling).
- Progress has been good. Gore's state-of-the-art 10-µ membrane has been incorporated into the primary process path. The performance of the new MEAs surpasses that of their current commercial membrane. Gore developed a process waste map that identified areas for reducing waste in the manufacturing process. Most of the improvements have been realized in the past year. Gore was forced to find additional backer material suppliers when the current supplier discontinued the incumbent product line. The team identified two low-cost candidates and successfully coated the most promising backer in a 30-cm-wide, high-speed-capable, roll-to-roll coating and drying process.
- While the modeling is almost 100% complete and looks promising, the experimental validation is still incomplete. The addition of the 10-µ membrane from Gore is a valuable addition.
- Gore’s progress was obviously hampered by the material supplier issue; however, the company appears to have identified and validated an appropriate substitute. Progress by Gore’s partners, UD and UTK, appears significant. Extension of the project timeline was appropriate as there is too much invested not to see the outcome of the project.
- This project showed a high dependence on modeling to predict properties. It is not clear what all the parameters input into the model would be. This reviewer questioned if this is an empirical mode. The approach appears to be yielding proper results.

### Question 4: Collaboration and coordination with other institutions

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

- The project shows the modeling by UD and UTK, but there is no evidence that any of this is actually being applied to anything. To date, there has not been any stack testing shown by UTC Power.
- Both UD’s and UTK’s collaboration with Gore is positive. Both of these institutions will benefit from working with an MEA original equipment manufacturer such as Gore. Based on some of the questions posed to the PI, it does not appear that the models being developed by UD and UTK will be generic enough to be of utility to other

MEA suppliers. Working with the National Renewable Energy Laboratory (NREL) on in-line quality control provides good collaboration and coordination of results. Using UTC Power for stack testing demonstrates good collaboration.
- Collaboration with the project partners is good; their roles are detailed. It appears that the two modeling efforts are nearly complete from the schedule presented. It is not clear if the models will have utility for the larger fuel cell community. They appear to be specific to Gore.
- It appears that the three partners collaborating to date (Gore, UD, and UTK) are collaborating well and their contributions are very complementary and necessary for success.
- Gore has developed good quality collaborations and the project builds on the results of the inputs for the partners.

### Question 5: Proposed future work

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

- Future work on the multilayer backer on a wide, high-speed, roll-to-roll is a good direction. There is a lack of integration of the other partners' work into the Gore work.
- Gore has executed this project in a logical and systematic manner with few disruptions caused by itself or its team. Gore describes a low-cost MEA scale-up process for its next step, which is a sign of a successful project to date. Future work discussion indicates a strong desire for Gore to implement the new processes proven in this effort on its production floor.
- The future work plan appears to be on track to reach the go/no-go decision point in August 2013, which is to achieve a high-volume cost of $9/kW. There is not much information presented on the break-in period and why it needs to be on the order of two hours. It is not clear if there is a path to eliminating the break-in period.
- The lion's share of the physical development remains for future work in the multiple steps involved in the low-cost MEA and gas diffusion media optimization and MEA conditioning (slides 19 and 20). The risks are likely high and their mitigation or alternatives are not evident in the future work statements.
- The program appears to be well structured and planned for the future.

### Project strengths:

- Gore's developments look good for manufacturing.
- Elimination/reduction of backer material will realize significant cost savings in MEA production. Reduction in the number of coating passes will also reduce MEA costs. Durability of MEAs and stacks will be increased as a result of a better understanding of mechanical durability and power density trade-offs. UD and UTK are good partners for Gore in this effort, as are NREL and UTC Power (partners to a lesser degree). A 25% reduction of cost for a 3-layer MEA is impressive. The PI demonstrated strong leadership and focus throughout this effort and significant progress was achieved.
- The strength of the project remains Gore's experience and process knowledge.
- The strengths of this project are UD and UTK's modeling capability and Gore's ability to innovate in MEA processing and reinforced membranes.
- The relevance of the project makes it very important. The approach is well defined and the partners are appropriate. Incorporation of process control research, although not necessarily a defined project task but evident through collaboration with NREL, should increase the value of the final deliverables.
- The most important program strengths are the experience and database of Gore.

### Project weaknesses:

- It is unclear how the UTK modeling effort relates to the rest of the project, and how this portion of the project is relevant to a manufacturing project. Similarly, the UD modeling does not seem to apply to a manufacturing project. At one point, the presenter mentioned a four-hour break-in as a target. That seems exceedingly long for transportation applications. The target should be substantially less than that.
- Results are not likely to have technology transfer beyond Gore.
- The project's weakness is its risk assessments.

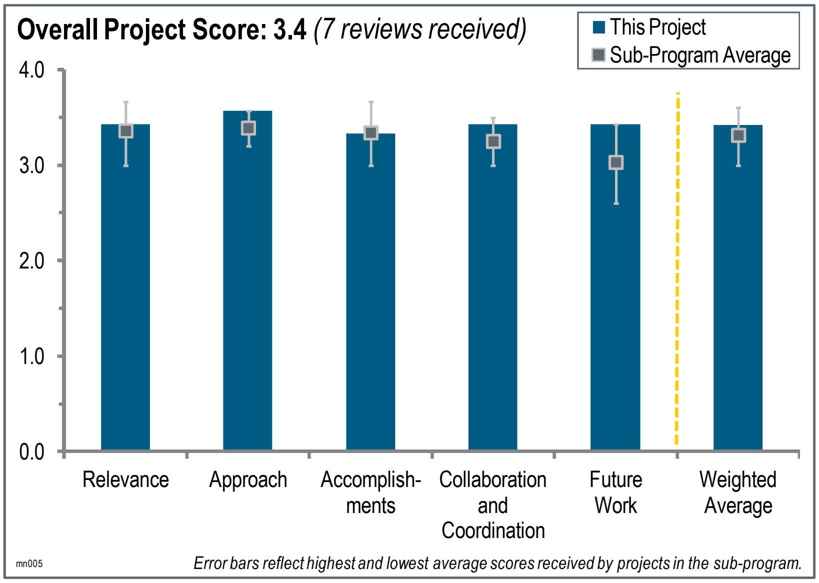
- The empirical results used in the modeling make the models dedicated to Gore and not beneficial to other MEA manufacturers.

### Recommendations for additions/deletions to project scope:

- Apply the modeling effort to new materials and provide enough information so that other projects benefit by the knowledge produced by this project.
- Expand the model work to include other MEA manufacturers' databases.
- At the end of the project, Gore should project when these 3-layer MEAs will enter their product line.
- One reviewer felt this was a great project and had no recommendations.

## Project # MN-005: Adaptive Process Controls and Ultrasonics for High Temperature PEM MEA Manufacture

Dan Walczyk; Rensselaer Polytechnic Institute




### Brief Summary of Project:

The high-level objective of this project is to enable cost-effective, high-volume manufacture of high-temperature (160°C–180°C) polymer electrolyte membrane (PEM) membrane electrode assemblies (MEAs) by: (1) significantly reducing MEA pressing cycle time through the development of novel, robust ultrasonic bonding processes for high-temperature (160°C–180°C) and low-temperature (<100°C) PEM MEAs; and (2) achieving greater manufacturing uniformity and performance through (a) an investigation into the causes of excessive variation in ultrasonically and thermally bonded high-temperature MEAs using more diagnostics applied during the entire fabrication and cell build process and (b) development of rapid, yet simple, quality control measurement techniques for use by industry.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- This work is right on target with what the DOE Manufacturing sub-program should be sponsoring.
- This project is important and addresses important aspects of increasing the production rate for MEAs.
- This project conforms to the overall objectives of the DOE Fuel Cell Technologies Program (FCT Program). If successful, this project could drop the bonding time of the electrodes by an order of magnitude while increasing the yield and reducing tooling costs.
- This project addresses high-volume manufacturing and quality control for the manufacture of phosphoric-acid imbibed polybenzimidazole-type MEAs.
- Automation is a key to cost reduction, as is adaptive manufacturing. This effort is relevant to DOE objectives. A cost model for scale production needs to accompany this work to ensure that a business case is demonstrated, and Rensselaer Polytechnic Institute (RPI) and its team ensured that a cost model and design guidelines are included. Ultrasonic sealing is a less energy-intensive and faster process that can give equal or better performance to thermal pressing. It is very relevant to DOE objectives to look at MEA manufacturing using ultrasonic sealing for both energy reduction and cost reduction purposes.
- Ultrasonic bonding for MEA fabrication has the potential to reduce MEA manufacturing costs. It is not yet certain if this will lead to the improvement of quality and performance.
- This project needs to prove the cost savings.

### Question 2: Approach to performing the work

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

- The project has a well-thought-out plan, incorporating elements of experimental design, testing, and cost analyses.

- This project has a strong creative approach. RPI is a national leader in developing a unique manufacturing methodology for fuel cell systems.
- Comparing different bonding technologies and understanding the structure differences could be valuable to different types of electrode manufacturing. Just from a time restriction, the project should concentrate on ultrasonic bonding, not thermal bonding.
- The technical approach follows a logical and systematic path with go/no-gos placed at appropriate program milestones. Ultrasonic sealing was not only applied to high-temperature PEM MEAs, but also low-temperature MEAs, where much of the PEM manufacturing currently exists. The principal investigator's (PI's) problem statement spoke about the mismatch between variances in MEA components and only a standard MEA manufacturing process—and the need to better understand their interrelationship. There does not appear to have been a lot of work in this area, as it is mostly consumed with the ultrasonic sealing novel process. This effort has a well-designed experimental approach. Teaming with BASF is a real plus because it maximizes the opportunity for technology transfer. Having Ballard on the team is also a plus. The royalty-free license for the RPI work allows for further dissemination to other MEA manufacturers.
- The detailed program approach appears to be sound and well thought out. Scale up to a commercial-size platform and use in a field application when both thermally and ultrasonically bonded MEAs are operating in the same stack and are being exposed to the exact same semi-controlled field conditions seems to be missing from the plan.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Progress has been great and the potential savings are outstanding.
- Voltammetry does not seem like a reasonable manufacturing quality control test. It is unclear why the different processing methods would affect the platinum particle size.
- There is limited use of diagnostics that give a good indication of how the manufacturing process works. Scanning electron microscopy cross-sections, radiography, or X-ray tomography could all add value to this project.
- The project accomplished what it set out to do: demonstrate the viability of ultrasonic sealing of MEAs compared to existing thermal processes.
- RPI saw little value in continuing with adaptive process controls; the company took the initiative to contact DOE and request that the remaining effort be solely focused on ultrasonic sealing.
- The business case via cost model will inform industry adopters of this technology and help them make an informed decision. A design guide for ultrasonic testing is an important "leave behind" that RPI is undertaking.
- Significant energy reduction, cycle time, and sealing time reductions were demonstrated compared to conventional process.
- The progress reported, including the quality assurance checks, is impressive. It is not clear if there is work to resolve the cause of the performance difference between the thermally bonded and ultrasonically bonded MEAs. It was also unclear if a hydrogen ($H_2$) pump wasvrun on both types of MEAs and a new calibration curve generated, which would bridge the performance gap.
- There has been reasonable progress in testing and validating ultrasonically bonded MEAs. Further work is required to resolve influences due to bipolar plates in testing, as well as characterization of mass transport performance deficiencies. Cost results should be expressed relative to total MEA and/or stack cost. Cost analyses for lower production volumes would be nice to see.
- RPI has improved the rate of production by 15-fold. The researchers are conducting analyses to prove production without defects, i.e., quality control. They are testing stacks to prove their system performance. They are also looking at low-temperature PEM MEAs; the approach there was good, but low-temperature MEA are not optimized for ultrasonic bonding. This work is demonstrating a cost reduction for ultrasonic sealing.
- This reviewer wondered the rate is when the system is scaled up.

### Question 4: Collaboration and coordination with other institutions

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

- The collaboration appears to be appropriate for this type of project.

- Participation and contribution from industry partners has been useful and coordinated.
- This project has proper representation from all possible players, which is a nice balance.
- This project has a good collaboration team that covers the breadth of high-temperature to low-temperature PEM fuel cells and is working with a quality control group.
- There is good collaboration between BASF and RPI. It is unclear what the National Renewable Energy Laboratory (NREL) and Ballard have provided.
- The royalty-free license will maximize collaboration and coordination with other MEA original equipment manufacturers.
- The RPI Center for Automation Technologies and Systems program has a short but strong history of technology transfer, including the nanotechnology and integrated circuit manufacturing areas.
- Other collaborators such as NREL, UltraCell, Ballard, PMD Manufacturing, and Arizona State University make information dissemination more likely.
- Coordinating with DOE on the need to focus remaining resources more on ultrasonic testing was positive.
- It was not clear how much the results address BASF-only processes and whether or not they might be transferrable to other MEA suppliers, especially for low-temperature PEM fuel cells.

### Question 5: Proposed future work

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

- The proposed follow-on work is appropriate. A little more specification on the steps would have been nice.
- Future work efforts are appropriate and will hopefully address some anomalies that have arisen in validation testing.
- The use of diagnostics and thermal characterization could be valuable in understanding the manufacturing differences. This project should concentrate more on this aspect rather than the 10-cell stack testing. Diagnostics during the MEA build process could also be valuable.
- More testing is critical to informing both the cost model and the design guidelines. RPI will address the issue with larger MEA (140 $cm^2$) problems before this project is over.
- Design guidelines for ultrasonic testing will be very valuable for further demonstration of this technology by other entities, as well as the BASF production facility.
- The path forward seems logical, but the project is 85% complete and with only four months remaining, it may be tight to fit everything in. If additional funding was needed and could be provided to ensure completion, then this reviewer recommends that DOE continue support. This is especially important because the low-temperature application looks promising, but further research needs to be done to investigate and potentially develop a plan to mitigate, if possible, the performance degradation at higher current densities.
- W.L. Gore needs to predict the production rate and number of production lines required to make a 40 MW fuel cell power station.

### Project strengths:

- The strengths are the knowledge and dedication of the project members.
- The effort is well designed and executed, and it is generally making good progress.
- The project has been well coordinated and executed, and all partners are actively engaged. The accomplishments have been outstanding and the payback potential appears huge.
- This project has improved its approach to bonding of MEAs. The methodology appears specific to membrane fuel cell systems.
- The project's strength is its design of the experiments in making a sound comparison between thermal and ultrasonic testing. The project also has a good team established to include RPI as lead, BASF (the MEA manufacturer), Ballard (the stack manufacturer), UltraCell (the system manufacturer), and NREL.
- The project team did not try to do too many things and just focused on a comprehensive evaluation of ultrasonic sealing versus thermal sealing and producing comprehensive results.
- RPI demonstrated agility in seeking to reduce the adaptive process controls thrust and to increase the ultrasonic thrust.

### Project weaknesses:

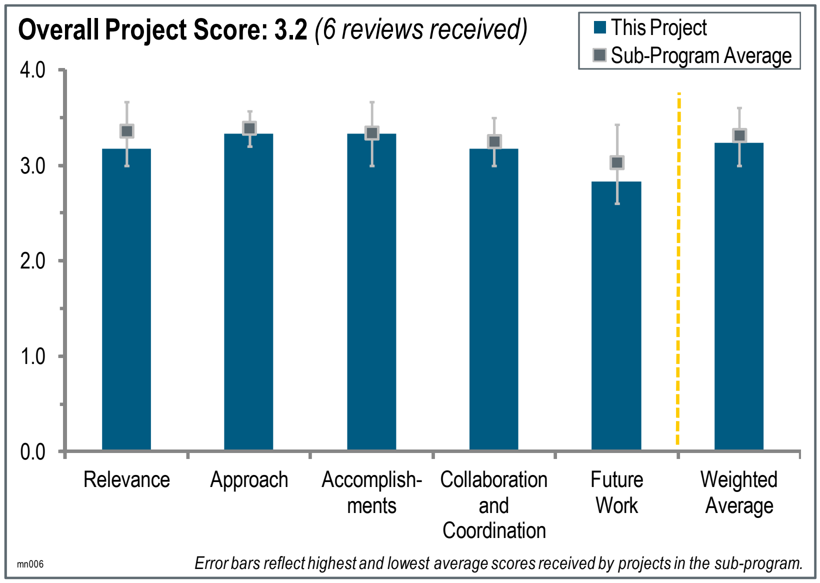
- The performance of these MEAs is low; much lower than typical $H_2$ MEAs. To be competitive with other types of MEAs supported in the FCT Program, the electrode layer cost must be significantly lower; however, it is the opposite. It is unclear why, at this point in time with this technology, this work is addressing the high-volume manufacturing of these MEAs, as opposed to increasing the performance and reducing the electrode cost so that they are cost competitive. The performance of the low-temperature MEAs is well below the standard MEAs that are commercially available. There are many other presentations at the DOE Hydrogen and Fuel Cells Program Annual Merit Review, for example. This performance needs to be brought to competitive standards before the manufacturing technique optimization can be considered valuable.
- This project is somewhat limited in scope. For example, commenting on reducing from 30 seconds to 3 seconds part of the MEA manufacturing process, the PI states that the "dominance of heat treatment may diminish the importance of the ultrasonic method."
- No major weaknesses, with the possible exception of the suggested future work, were noted. The perceived weaknesses can be easily corrected by the project members.
- Cost impacts need to be expressed at a level higher than the specific process.
- The project's weakness is that, compared to phosphoric acid fuel cells (PAFCs), the MEA production rate would yield the same data, because PAFCs are not based on membrane design.

### Recommendations for additions/deletions to project scope:

- This reviewer suggests publishing the costing data referred to by the presenter.
- The project is the cart-before-the-horse; these MEAs are not close to being competitive with Nafion®-based MEAs, thus until the performance/cost is improved, improving the manufacturing of these MEAs will show limited benefit to the FCT Program.
- The DOE Hydrogen and Fuel Cells Program should be funding improved MEA designs with reduced electrode loadings before it funds improved manufacturing of the technology.
- Only a small portion of this project deals with low-temperature MEAs. That portion should become the main emphasis of the project.
- Thermal bonding of MEAs taking 30 seconds should be eliminated from the project.

## Project # MN-006: Metrology for Fuel Cell Manufacturing

Eric Stanfield; National Institute of Standards and Technology




### Brief Summary of Project:

The objectives of this project are to: (1) identify and evaluate the capability and uncertainty of commercially available non-contact, high-speed scanning technologies for applicability to bipolar plate manufacturing process control, and (2) develop, demonstrate, and optimize the system's ability to measure thickness and variation-in-thickness, which can then be used to assess stack parallelism.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- The non-contact dimension sensor and the scatterometry measurement systems can be very important in reducing manufacturing costs and enabling improved quality for both bipolar separators and catalyzed membrane electrode assemblies (MEAs).
- The goal of this project is to provide bipolar plate manufacturers and designers with the data necessary to make informed tolerance decisions to enable reduction of fabrication costs. This clearly is relevant to DOE objectives. Additionally, the fact that the National Institute of Standards and Technology (NIST) has adjusted the objective based on future industry input supports the relevance and the ongoing efforts to support industry. The fact that Ballard, FuelCell Energy, and UTC Power are interested in these technologies further validates its relevance.
- The objective of one of the projects is to provide bipolar plate manufacturers with a high-speed, automated approach for process control dimensional inspection. The other project is focused on an automated high-throughput process inspection of platinum (Pt) loading. Both topics are very relevant to DOE's efforts to bring fuel cell technology one step closer to widespread adoption by lowering the cost of manufacturing through better process control.
- Both aspects of this project do not adequately characterize how advances made in the effort could support reaching DOE objectives.
- The methodology fits well into establishing quality control (QC) data. The project does not appear to be applied to an existing bipolar plate manufacturing process, so it is not clear if this will or can be applied to continuous production technology.

### Question 2: Approach to performing the work

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

- It is difficult to identify a better prepared and higher skilled awardee than NIST to execute this project.
- The approach to generating this database is to use a statistically based design-of-experiments and fabricate experimental "cathode" side flow-field plates with various well-defined combinations of flow-field channel dimensional variations. Then, through single cell fuel cell performance testing using a robust protocol, NIST will quantify the performance effects, if any, and correlate these results into required dimensional fabrication tolerance levels. This is a sound engineering approach.

- The approach with bipolar plates (P2 project) is to identify suitable non-contact measurement technologies with 12–50 µ resolution and demonstrate the ability to measure plate thickness to enable overall fuel cell stack parallelism to be assessed during manufacture. Having the capability for a high-speed measurement system will be important eventually for high-volume manufacture; this capability is not currently available commercially. It is not so critical now, when fuel cells are manufactured at low rates.
- The objective of the P3 subproject is to evaluate the optical scatterfield metrology tool's sensitivity to Pt and Pt alloy catalyst loading and various defects such as pinholes that can be characterized by other methods such as x-ray fluorescence and scanning electron microscopy. Successful development would provide MEA manufacturers with an automated, high-throughput technique for determining Pt loading with greater sensitivity (0.01 $mg/cm^2$) than currently available.
- The approach is simple in that it includes the steps that are typically necessary to alter or design new optical metrology methods that, in this project, overcome barriers in MEA and separator surface measurement speed and accuracy. NIST still needs to manage large sets of raw two-dimensional data and to develop routines for analysis.
- The non-contact plate sensor approach is adequate for validating the sensor technology. It is not clear how a validated technology improves stack cost or performance. The approach for the optical scatterfield portion of the project is not adequately explained. This reviewer is left to try to infer it from the accomplishments.
- This project's approach to obtaining measurements is outstanding; however, the approach to integrate with existing manufacturing production line is poor.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- NIST delivered another year of outstanding progress and accomplishments. This reviewer had high expectations for NIST and felt that they delivered at that level of expectation.
- The accomplishments for the P2 project are reasonable, given the small amount of funding. NIST demonstrated a dual opposed-probe configuration to measure thickness and variation in thickness with less than 10 µ accuracy. Error in measurement is currently being evaluated.
- The optical properties of Perylene from 3M were determined to provide model parameters that enable a realistic scattering model for the 3M nanostructured thin film (NSTF) catalyst coated membrane (CCM) to be developed. Preliminary simulations for quantifying the effect of roughness on CCM scatterfield measurements were completed, but conclusions are still being determined. The optical design of a large aperture system prototype for fuel cell Pt loading measurements was completed.
- The opposed probe for plate thickness and profiling builds on the earlier accomplishments of the single-sided probe, and progresses toward a complete high-speed dimension metrology system for bipolar plates.
- The plate thickness measurement effort is showing useful results. It is not clear how important calibration (both initial and continuing) is relative to producing consistent measurements. More detail regarding the utility of the approach with increasing line speeds would be helpful.
- The scatterfield metrology effort shows lots of results, but it is not yet clear how the results will lead to improved QC and reduced component cost. It is also not clear how scatterfield metrology can be transitioned to an industrial environment.
- The accomplishments are very good. NIST has demonstrated the performance of the QC method and has developed the interest of industry. Good progress is being made in the measurement of electrode surfaces using ellipsometry.
- The accomplishments to date are appropriate for the effort. The results appear to be outstanding.

### Question 4: Collaboration and coordination with other institutions

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

- Collaborations with a number of plate manufacturers are leading project P2 in fruitful directions. W.L. Gore (Gore) and 3M are interested collaborators on the P3 project and provide samples of material sets for development of the scatterfield technology. Discussions are underway with a leading semiconductor toolmaker for future collaborations to prototype the scatterometry tool. NIST is collaborating with Los Alamos National

Laboratory (LANL) to provide an optical technique capable of nanoscale materials characterization based on scatterfield technology.
- This project demonstrates an interaction with LANL, 3M, and Gore, but this reviewer is unsure of the degree or quality of interaction or contribution. Certainly the future work with industrial collaborators' membranes and MEAs is important. Working with Treadstone on the metal plate metrology will provide a realistic basis with industry for the bipolar plate manufacturing.
- The plate thickness measurement effort collaboration seems to largely consist of samples provided by a few companies. While there are a few current collaborators for the scatterfield metrology effort, others are tentative or even speculative (proposed work participants).
- NIST has identified companies with interest and companies that they are submitting proposals with, but they have not directly applied the technology to an existing production or pilot plant production facility.
- The collaboration and coordination is appropriate and appears to be growing.
- This work displays extensive collaboration with industry leaders in materials.

### Question 5: Proposed future work

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

- According to the schedule, the project is nearly complete. It is not clear just how far the two projects can proceed with the available funding. Proving the usefulness of these techniques in actual practice will likely require additional resources. The research team should consider partnering with the National Renewable Energy Laboratory.
- The main project will be bringing the opposed probe thickness and profile device to a practical reality by optimizing its accuracy. This builds on the progress made on the single probe system.
- The sub-project future work plan can progress to building a prototype set of instruments for MEA and membrane measurements of surface and catalyzation. This may be a large increment from the current project status and difficult to achieve. If achieved, however, it will result in a valuable tool set for manufacturing metrology.
- Future work efforts identified are appropriate for further validation of the technologies. It does not appear that there is enough time or budget remaining to complete the identified future work. The work is lacking any assessment of the usefulness relative to quality improvements or cost reductions.
- The proposed improvements and understanding of the QC technology are very good to outstanding. However, it is a concern that the two techniques discussed have not been integrated with a fabrication line.
- One reviewer felt that the proposed new work appears appropriate.

### Project strengths:

- The highly qualified and experienced team at NIST is successfully collaborating with numerous organizations to prove the viability of the two approaches. It provides manufacturers with established processes and procedures for determining measurement standards.
- The researchers are very competent and have a strong knowledge base.
- The project's strength is its analytical approach.

### Project weaknesses:

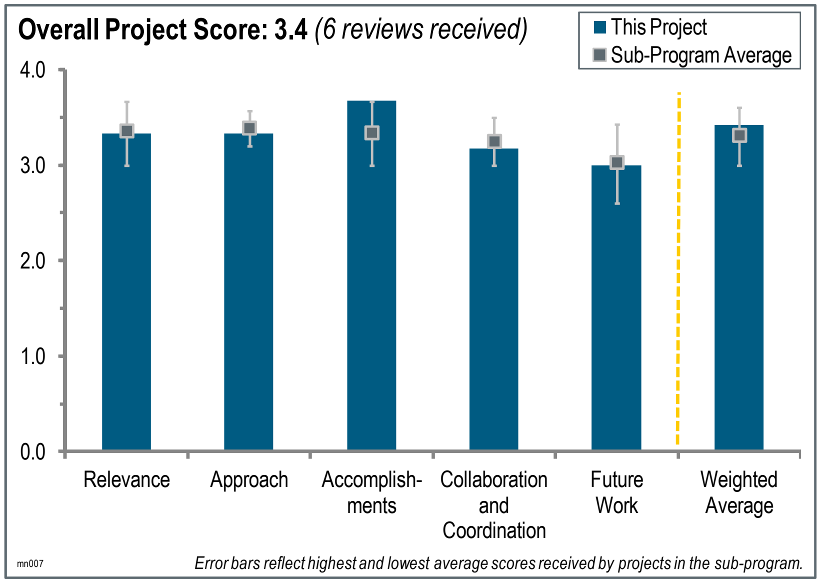
- A rigorous comparison of techniques in commercial practice with those being developed by NIST is missing. This comparison could provide stronger justification for developing these measurement techniques and for determining the impact on manufacturing cost savings if they are successfully implemented in practice.
- This project remains too focused on things one could do and not focused on whether one should do them.
- A stronger partnership with a fabricator is needed.

### Recommendations for additions/deletions to project scope:

- Continue the excellent work.

## Project # MN-007: High Speed, Low Cost Fabrication of Gas Diffusion Electrodes for Membrane Electrode Assemblies

Emory De Castro; BASF




### Brief Summary of Project:

The overall objectives of this project are to: (1) reduce the cost of fabricating gas diffusion electrodes (GDEs), especially for GDEs used for combined heat and power (CHP) generation; (2) relate manufacturing variations to actual fuel cell performance to establish a cost-effective product specification within six-sigma guidelines; and (3) develop advanced quality control (QC) methods to obtain a threefold throughput increase on full width and length cloth, and to expand efforts on non-woven paper.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- This project is an integral contributor to the success and viability of high-temperature polymer electrolyte membrane (PEM) technology.
- This project focuses on reducing the fabrication costs of GDEs for high-temperature PEM applications, which is relevant to the DOE objectives in the manufacturing session.
- The project's objectives are well rounded and directly support the Fuel Cell Technologies (FCT) Program and its objectives related to membrane electrode assembly (MEA) cost reduction and quality improvement.
- Cost reduction is critical and relevant to the development of this technology. This reviewer questions what portion of the cost is platinum (Pt) and if it is more important to reduce this cost.
- This project clearly meets the DOE objectives for lower-cost, higher-performing components.
- BASF works to improve manufacturing and quality of GDEs for CHP.

### Question 2: Approach to performing the work

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

- One reviewere felt that GDEs have a limited role in the FCT Program, with few developers working on them, and few developers working on polybenzimidazole (PBI)/phosphoric acid.
- The in-line QC/in-line Pt measurements are good advances.
- The overall approach of this project is solid, focusing on increasing throughput by improving line speed and reducing the required number of coating passes.
- The work approach incorporates aspects of modeling, experimental design, and testing in a clearly described work flow with decision points.
- The approach is given in very broad terms -- a stronger discussion with more details would have been preferred. If a catalyst cannot be directly applied to the membrane, then attachment to the gas diffusion layer (GDL) approach is logical. It is not clear why BASF needs a microporous layer (MPL) and how a three-dimensional catalyst layer would be connected to the electrolyte. This needs further discussion.
- The approach is well thought out and methodical. This is as expected, based on the principal investigator.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.7** for its accomplishments and progress.

- This project has demonstrated a decrease in manufacturing time and identified various problems with catalyst inks that have been solved to increase the manufacturing rate.
- This project provides potential for substantive cost savings in labor and Pt loading.
- There was a significant amount of progress in this project compared to last year. A fourfold increase in throughput rate, an order of magnitude reduction in defects, and a fivefold reduction in variation of Pt loading are very impressive accomplishments. There was a 75% cost reduction attributed to reducing labor hours, which is a significant achievement for this project as well.
- BASF has achieved appreciable increases in process capacity while maintaining performance. The labor savings identified were significant but should be characterized as a function of stack cost.
- BASF has shown an improvement in throughput. The production rates should reflect these improvements. It is unclear what the cost benefit for this improvement is and why the team needs an MPL for a non-woven material. This reviewer also wondered if the MPL is carbon or graphite.
- The progress and accomplishments are impressive. The learnings to date, if only applied to older processes, would still warrant the expenditures.

### Question 4: Collaboration and coordination with other institutions

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

- The collaborations and coordination with other investigators are limited, as should be expected based on the proprietary nature of some of the initial processes being evolved.
- ClearEdge Power and the National Renewable Energy Laboratory (NREL) are strong additions, and it appears that there is a good amount of collaboration on this project.
- The two collaborators appear to be reasonably engaged and coordinated.
- This project is working with two good organizations: NREL and ClearEdge Power.
- BASF is providing GDEs to NREL for their project. ClearEdge Power is validating the materials.

### Question 5: Proposed future work

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

- Work on the non-woven back is a step in a positive direction, and woven materials are not being considered by most original equipment manufacturers.
- Developing a one/two pass application process will be the most likely candidate for commercialization.
- The future work appears appropriate, as far as it goes. It would be hoped that the new materials would be subjected to field trials and benchmarked against current product lines for performance and durability.
- The proposed future work appears solid, but is not well defined. It is unclear what is considered "production level"; what the plan is for achieving two or fewer passes for both anode and cathode GDEs; what risks are involved with these targets; and how these risks are being addressed.
- One task remains and the future work is adequately defined. Further detail regarding schedule would be helpful.
- It is  not clear if a 30% reduction in material and labor is a significant number. This sounds like a good objective, but it needs some justification.

### Project strengths:

- There has been very significant progress made in increasing throughput and reducing fabrication costs in this project. Additional improvements in quality should help further reduce the total cost of the GDE.
- The strengths are the knowledge, technical capabilities, and dedication of the project members.
- This is a well-conceived and -executed project that is exceeding its objectives.
- The project's strength is its very experienced group.

### Project weaknesses:

- The project has limited applicability for other programs and manufacturers. There are few developers of PBI/phosphoric acid fuel cells and few developers using GDE technology. Thus, this project is mostly only applicable to one company.
- The path forward lacks clarity and there is no reference to how close the throughput rate is to being ready for commercial production. Additional information about the cost savings due to improved product uniformity would also be helpful in assessing this project.
- It is not clear if the team fully understands the impact of carbon corrosion on their electrodes. Manufacturing may be improved, but the stability of the system may not be improved.

### Recommendations for additions/deletions to project scope:

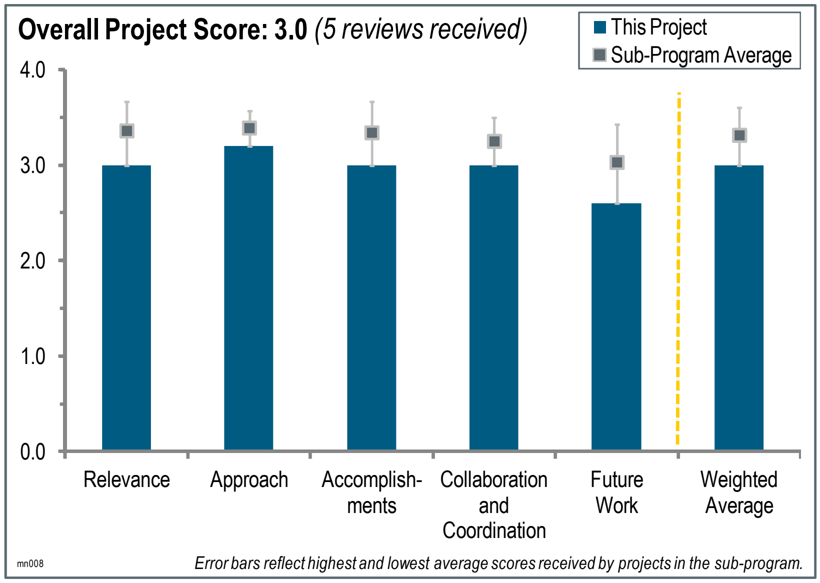
- The project seems to be on the right track and does not require any major changes to the overall scope. It may be helpful to have more clearly defined goals for the next portion of the project, however.
- The project should require durability testing of the new catalyst layers.
- Continue the good work.

## Project # MN-008: Development of Advanced Manufacturing Technologies for Low Cost Hydrogen Storage Vessels

Mark Leavitt; Quantum Fuel Systems Technologies Worldwide, Inc.

### Brief Summary of Project:

The objective of this project is to manufacture Type IV hydrogen ($H_2$) storage pressure vessels utilizing a new hybrid process with the following features: (1) optimized elements of advanced fiber placement (AFP) and commercial filament winding (FW) and (2) improved understanding of polymer liner $H_2$ degradation. The project will develop a manufacturing process with lower composite material usage, lower cost fiber, and higher manufacturing efficiency.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- Quantum is developing new hybrid Type IV $H_2$ storage tanks, which require less carbon fiber (CF) than conventional filament wound pressure vessels. The work is relevant to DOE's goal of reducing the cost of onboard $H_2$ storage systems. The project aims to improve understanding of the compatibility of $H_2$ with Type IV high-density polyethylene (HDPE) liner through testing in a 4,000 psi $H_2$ environment (tests done by Pacific Northwest National Laboratory [PNNL]).
- This continuing project addresses improved design and manufacturing methods for high-pressure compressed $H_2$ gas storage vessels. While proposed modifications are predicted to give an approximate 22% reduction in the baseline tank weight for Type IV tanks, any cost savings seem to be minimal (approximately 5%, at best) and there is virtually no positive impact on volumetric capacities of these vessels. This effort is to include characterization of the polymer liners with high-pressure $H_2$ gas, including permeation and degradation effects.
- This project addresses the key issues of cost and weight of $H_2$ pressure vessel storage for vehicles. This can add leverage to high-pressure storage as an early enabler to fuel cell electric vehicle penetration.
- Developing innovative manufacturing techniques for forming tanks will be very useful for improving $H_2$ storage systems. Fibers with windings, lower-cost materials, and higher manufacturing efficiency are all important. However, because it appears that most of the cost savings comes from fiber cost reductions, it is unclear if the rest of the effort is worth all the expense for the rest of the project.
- The research goals as pursued directly address DOE Hydrogen and Fuel Cells Program goals. However, while the work performed is excellent and meets the objectives stated in the presentation, it did not specifically target issues with conformal vessels. However, this reviewer believes the work as performed should come before more advanced studies on conformal vessels.

### Question 2: Approach to performing the work

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

- The hybrid AFP/FW scheme reduces total CF usage by as much as 22% as compared to FP alone. Additional incremental cost reduction is achieved by using lower-cost fiber on the outer layers of the AFP/FW tank. While the combined approach has the potential to reduce the cost of Type IV tanks by about 10%, it cannot bring down the system cost sufficiently to meet the DOE cost target. The cost reduction is not in direct proportion to

reduction in CF usage because of the higher cost associated with AFP manufacturing. In situ testing is carried out at PNNL to characterize $H_2$ compatibility with the HDPE liner.
- This project displays a well-organized, mixed approach to relevant challenges for $H_2$ storage.
- The Quantum/Boeing team would reduce the cost of Type IV tanks by optimizing CF placement and the winding process to minimize the amount of expensive high-strength CF, while maintaining strength to pass performance requirements. PNNL was to characterize candidate polymer liner materials for $H_2$ compatibility better, but it was not clear whether improved options would be found.
- The approach is clever and straightforward. While the elements of the approach are excellent, an argument should be provided that makes it clear that more testing to improve statistics is not needed.
- Most of the effort is focused on developing techniques to integrate less expensive fibers with hybrid tanks. The efforts are appropriate.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- In collaboration with Boeing, this project has built and tested several additional vessels to validate the hybrid AFP/FW design for Type IV pressure vessels. In addition to burst tests, cycling tests were conducted (vessels 8 to 11 were presented at the DOE Hydrogen and Fuel Cells Program Annual Merit Review, but not included in the submitted slides).
- Improvement was made to reduce marcelling and wrinkling in end dome plies and reduce peak stress at the AFP/FW interface.
- PNNL tested the compatibility of polymer liners with $H_2$ at 4,000 psi. Initial sample results showed a decrease in liner modulus under high $H_2$ pressure.
- Issues with analysis software for designing tank configurations led to tanks rupturing at pressures just below the requirement level when CF contents were decreased. The project team has not yet demonstrated a prototype type vessel that would pass pressure tests. Boeing implemented IR heating during the fiber wrapping process that produced improved vessels, although not all problems with wrinkling of fibers were addressed or resolved. It looks like more processing refinements are still needed. Only limited data were obtained at PNNL on the $H_2$ compatibility of HDPE polymer liner materials, and it looks as though this effort has been de-scoped from the project.
- The reduced marcelling and reduced CF use, combined with IR heating, all contribute toward DOE cost and weight goals. The cycle and burst testing helped in indicating realistic progress, but this is initial testing.
- A lot of work has been performed with hybrid vessel integration with lower-cost fibers, testing, modeling, and process development, which points to meeting the goals.

### Question 4: Collaboration and coordination with other institutions

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

- It appears that interactions between Quantum and Boeing have led to modest improvements in the quality of the wrapped fibers and tank walls, although the vessels still do not satisfy safety requirements. Efforts were made to adapt manufacturing processing with designs. It is not clear how PNNL's work on polymer liners correlated with the other team members, and there does not seem to have been other significant roles played by PNNL.
- This project displays great collaboration with Boeing and PNNL, which leverages the capabilities of each partner.
- All three organizations mesh well in this project. This project displays good collaboration and coordination, playing on the team's strengths.
- This project has strong collaboration with Boeing and PNNL.
- The team consists of industry players whose strengths appear complementary.

### Question 5: Proposed future work

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

- The project is ending in March 2013, and the proposed future work is proper to wrap up this project. Testing to national standards is a prerequisite to meeting U.S. Department of Transportation and Society of Automotive Engineers standards. Cyclic tests to collect fatigue data for T700 CF will be a valuable addition to the database for use by modelers as well as tank manufacturers.
- A complete cost analysis of the hybrid vessel designs is within the scope of this project.
- The next step seems to be improving the finite element computer modeling to yield better designs needing less CF to meet mechanical requirements that will be checked by pressure and burst testing. The project is unlikely to improve either mass or volume properties substantially, and even an approximate 20%–25% reduction in costs will not allow these Type IV tanks to meet the cost targets. Significant cost saving can only come from using very inexpensive and high-strength alternative materials that are outside of the current scope of this project.
- Continued production of vessels with a hybrid of carbon and lower-cost fibers, followed by cycle and burst testing, is a major element in the future plans and is critical. The continued, incremental manufacturing improvements in things such as tensioner controls will also help in cost reductions.
- The proposed future work to upgrade in-house computer models and to design and build vessels with baseline and low-cost fibers on hybrid is reasonable.
- Elements of the proposed work are good. However, the comment about increasing the statistical sample should be considered.

### Project strengths:

- The project's strengths are (1) its substantial experience in Type IV tanks and CF winding, (2) the fact that the company is one of leading manufacturers of Type IV tanks for commercial application, and (3) leveraging advanced proprietary technology from Boeing to advance the project goal.
- A coordinated effort between commercial Type IV storage vessel manufacturers and an aerospace technology company with expertise in CF manufacturing offers the possibility for developing less-expensive compressed gas $H_2$ storage systems.
- The effort is using a lot of existing capabilities to optimize $H_2$ storage tanks with lower-cost fibers. It appears the project should be able to meet the goals.
- The project's strengths are that it is well organized, makes continuous progress, and has a complementary team of partners.
- The concept that the approach addresses is an excellent one.

### Project weaknesses:

- This reviewer wonders whether sufficient test articles were fielded to provide an adequate number of samples to achieve statistical significance. Vessel burst test results were compared to modeling and a limited number of vessels whose construction was not identical. Construction was changed during testing in an attempt to address an apparent weakness. This sort of approach suggests that there is a risk of not establishing a good baseline. During the question and answer period, several questions touched on this issue, but the presenter was confident that the modeling was sufficiently accurate to back the experimental results. However, it should be noted that in one case a need was felt to improve the model.
- Even though the team may be able to optimize fabrication processing using their stated approach, it cannot overcome the cost issues with conventional high-strength CF. Any improvement in manufacturing will still have only limited benefits. There was no follow through on the polymer line characterizations or alternative materials being assessed.
- It would be more interesting to see such large efforts be used to identify potential routes that could substantially improve tanks and reduce costs associated with tank design and manufacturing, rather than relying on decreasing fiber costs as the main way to decrease costs.
- While a 10% cost reduction is welcoming, it is unlikely that more significant cost reduction (30%–50%) can be achieved with the hybrid designs and utilization of lower-cost fiber for outer layers.

### Recommendations for additions/deletions to project scope:

- Quantum should document and show more clearly the rupture regions of pressure-tested tanks, determine whether failures are due to design inadequacies or manufacturing issues, and clarify whether there will be any additional assessments of the polymer liner materials.
- Consider conducting a modeling effort that considers, among other things, the diffusivity, solubility, and permeability of $H_2$ in polymers to guide efforts on polymer liners.
- The project should focus on designs and processes that could substantially reduce costs.
- All the data collected during the life of this project should be published or made available to DOE.

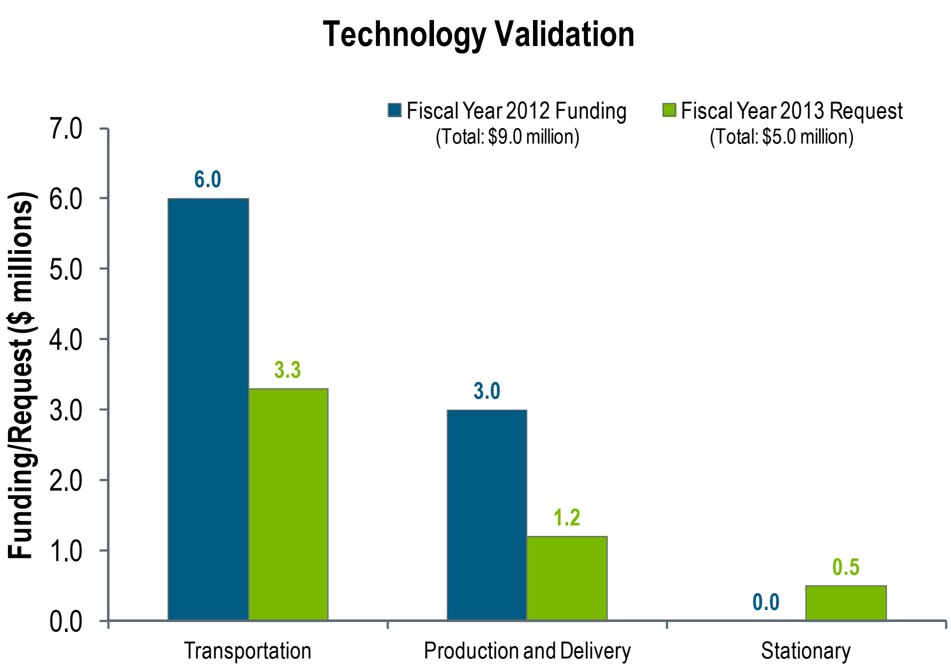
# 2012 — Technology Validation
# Summary of Annual Merit Review of the Technology Validation Sub-Program

## Summary of Reviewer Comments on the Technology Validation Sub-Program:

In general, the reviewers believed the sub-program area was adequately covered. Important issues were identified and progress was clearly presented. Projects included in the sub-program were effectively summarized, and highlights presented. Plans were identified for addressing issues and challenges. The Technology Validation sub-program is in transition. Important elements of the sub-program's future portfolio will be determined in large measure by awards resulting from current Funding Opportunity Announcements (FOAs). The FOAs focus on light duty fuel cell electric vehicle validation and hydrogen refueling station performance. The reviewers believed the sub-program to be focused and managed fairly well, noting the transition in management. However, overall the sub-program is effective in addressing U.S. Department of Energy's (DOE's) Hydrogen and Fuel Cells Program's (the Program's) needs. A reviewer notes that some activities were not in good alignment with the sub-program's goals.

## Technology Validation Funding by Technology:

The funding portfolio for Technology Validation will enable the sub-program to continue to collect and analyze data from fuel cells operating in transportation and stationary applications, as well as hydrogen production and delivery technologies. Data from fuel cell buses, forklifts, and backup power systems will continue to be evaluated. In addition, analysis of new hydrogen refueling stations in California and the Northeast will be included in the data collection activities. The fiscal year (FY) 2012 appropriation was $9 million. The Learning Demonstration ended early in FY 2012, and in late FY 2012, new project awards were announced for collecting hydrogen refueling station data and validating advanced refueling components, which include a high-pressure electrolyzer and increased-capacity hydrogen storage for stations. Also in late FY 2012, a fuel cell vehicle data collection funding opportunity closed. Selections are anticipated in early FY 2013, but the projects will also be funded using FY 2012 funding. These new projects will be the main emphasis of the sub-program. The FY 2013 request of $5 million is subject to congressional appropriations.



## Majority of Reviewer Comments and Recommendations:

The reviewer scores for the four Technology Validation sub-program projects that were reviewed had a maximum of 3.8, a minimum of 2.3, and an average of 3.1. A key strength identified by reviewers in all of the Technology Validation projects was the excellent participation from collaborators, which has been critically important to the success of the projects. In addition, the projects supported the major goals of the Program and provided valuable information to the participants.

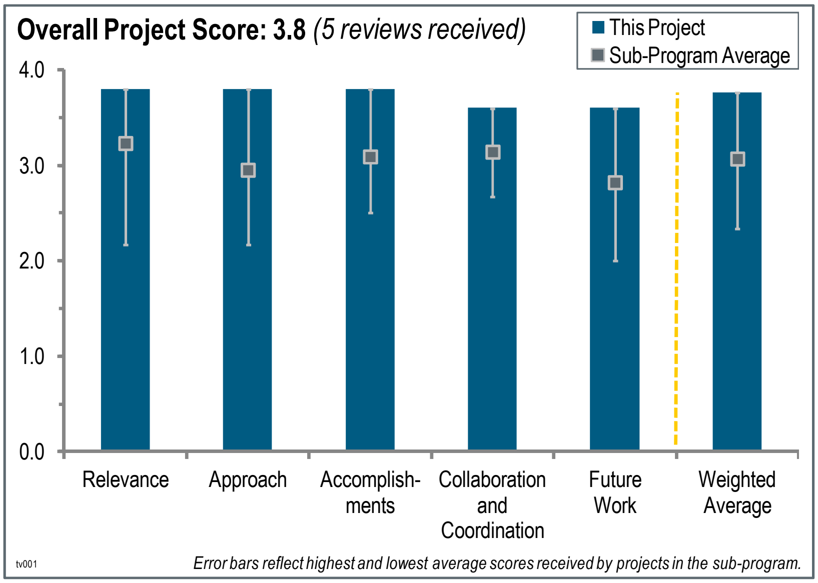
Reviewers observed that the National Renewable Energy Laboratory's approach for collecting, securing, and analyzing data is well-established and has been expanding to other applications such as material handling equipment and backup power. Reviewers recommended the continuation and expansion of the Controlled Hydrogen Fleet and Infrastructure Analysis activity because of its vital importance for informing decision-makers for public and private investments in hydrogen and fuel cells for transportation. Continued tracking of technology status is important for capturing the next generation of vehicles as they become commercial. The importance of fuel cell buses was broadly acknowledged. The principal investigator for the fuel cell bus evaluations was applauded for her excellent collaborations with transit agencies, developers, and others. It was recommended that fuel cell buses be compared to other hybrid buses; that warranty repair costs be included in the repair costs; and that more be done to compare buses of similar age, size, and service conditions. The merits of grid support in frequency and response to wind in the Wind to Hydrogen project were highlighted, but the technology validation project needs to focus on technology validation goals and have a more open process for bringing in equipment to validate. The Florida Hydrogen Initiative was difficult for reviewers to evaluate due to the diversity of its tasks; however, a few tasks received positive remarks and it was observed that the effort provided a meaningful educational experience for many.

# Project # TV-001: Controlled Hydrogen Fleet and Infrastructure Analysis

Keith Wipke; National Renewable Energy Laboratory

## Brief Summary of Project:

The objectives of this project are to: (1) validate hydrogen ($H_2$) fuel cell vehicles and infrastructure in real-world settings and (2) identify the current status and evolution of the technology. The National Renewable Energy Laboratory (NREL) provides the facility and staff for securing and analyzing industry-sensitive data, performs analysis using detailed data in its Hydrogen Secure Data Center, and publishes and presents composite data products to the public and stakeholders.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- This project is a major technology validation activity that provides important metrics on programmatic progress. It is definitely relevant to the DOE Hydrogen and Fuel Cells Program (the Program).
- This project is fully relevant to DOE objectives because it collects and disseminates real-world operating data.
- This is probably the most important DOE project related to large-scale $H_2$ and fuel cell electric vehicle (FCEV) deployments that offer the best opportunity for widespread use of $H_2$ and fuel cells in the economy.
- The data collection over the years is great and the improvements demonstrated are commendable. It is unclear where this type of data collection goes from here—additional testing and data collection will be absolutely necessary in the next few years for acceptance of the technology.
- The Program has devoted substantial resources to fuel cell vehicle and $H_2$ infrastructure technology validation projects. NREL's collection, analysis, and reporting of performance data associated with those projects has made a vital contribution to understanding the status of technology development relative to DOE's goals. NREL's data products have been continually refined so as to increase their value for both government and industry decision makers. The detailed, objective results of NREL's work are easily understood by those responsible for decisions on public and private investment in technology research, development, and commercialization. Over the past few years, this work has likely been the most significant contributor to unbiased, supportable, statistically valid conclusions about progress toward programmatic targets—vehicle range, stack durability, mean time between failure, fueling rates, and many other metrics.

## Question 2: Approach to performing the work

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

- The approach is solid and clearly demonstrates that extensive data collection is and will continue to be necessary to move this technology into the mainstream.
- This project featured a very well organized and excellent data presentation, with results widely used by others in the hydrogen and fuel cell community.
- The project approach is very good; the project team collected and analyzed numerous data provided by operating partners for vehicles, filling stations, and performance. The project, correctly, does not make strong judgments on the performance of fuel cell vehicles, but rather objectively presents the results. Important components of this project are the communications to DOE, stakeholders, and the public.

- The approach has evolved over several years and is based on what works best. It is totally dependent of self-reporting by the program participants. Over time, participants have grown to trust the NREL team and to value the conclusions and findings of the project.
- Over time, Keith Wipke and his team have steadily focused and refined this project's approach to data collection, analysis, and reporting. The result is an outstanding and constantly expanding collection of Composite Data Products (CDPs) and Detailed Data Products (DDPs). With ongoing feedback from data providers and users, NREL developed a logical approach that has resulted in routine periodic production of high-quality, informative CDPs, DDPs, and progress reports. These reports provide outstanding documentation for industry, government, and the public. While the project being reported on is near completion, the approach employed by NREL's team for this work is serving as a model for fuel cell and $H_2$ projects other than those focused on on-road vehicles; these include projects for fuel-cell-powered material handling equipment and stationary fuel cells.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.8** for its accomplishments and progress.

- Progress continues to be steady and constant over the nine years that the project has continued. This year was no exception, with significant accomplishments.
- The hard data collected over 10 years was clearly needed and demonstrated value in continuously monitoring developments and improvements.
- A large and useful quantity of data has been obtained over the nearly 10-year life of this project. Many presentations and publications have been made. However, some of the slides presented in this DOE Hydrogen and Fuel Cells Program Annual Merit Review presentation may be a bit esoteric to the non-expert. It is generally, but not always, clear how close the real-world performance results are to the DOE performance targets. There is good leverage into forklifts and other emerging fuel cell markets.
- This project provides the best, most objective, and most independent indicators of progress toward DOE's targets and goals for the key metrics associated with fuel cell light-duty vehicles and related $H_2$ infrastructure. The development and publication of 99 CDPs is an exceptional accomplishment. The highlights of selected accomplishments during the past year (the final year of the project), included in the presentation, are quite impressive. Taken together, they provide an appreciation for the merits of both NREL's data project (reported on here) and the progress of fuel cell vehicle and $H_2$ infrastructure technologies. The final data analysis and report on the vehicle learning demonstration has been published. Publication of the final report has been complemented by papers, webinars, and other initiatives.

### Question 4: Collaboration and coordination with other institutions

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

- Needless to say, collaborations are outstanding and absolutely necessary because the value and worth of the project is totally dependent on accurate, current input from project partners.
- Collaborations seem quite fine with Daimler, General Motors, and Air Products. No indication was given why Ford Motor Company/BP and Chevron/Hyundai-Kia dropped out in 2009. The project continuation needs more partners.
- Collaborations are extensive—it is too bad that several partners dropped out two years ago. It was surprising that some of the high-throughput stations shut down "early." If the project was to be renewed, it is essential that such stations "stay the course."
- NREL's data collection and analysis team has earned the trust of all organizations that have participated in the vehicle learning demonstration program. Industry confidence in NREL has grown continuously since its data project commenced in 2003. Contributing factors include ongoing communications, opportunities for input and feedback to the process, and NREL's system for protection of sensitive and proprietary information (the Hydrogen Secure Data Center). NREL's team has also maintained excellent communications with many organizations sponsoring related activities and/or having a stake in the outcome of DOE's program, such as the California Fuel Cell Partnership and the Fuel Cell and Hydrogen Energy Association.

### Question 5: Proposed future work

This project was rated **3.6** for its proposed future work.

- There are good plans for the completion of the project, which is scheduled for the end of this fiscal year.
- Such a real-world project must continue as fuel cell vehicles continue their advancement toward commercial reality.
- It is very important to keep this team involved with the next technical validation projects to keep continuity in data evaluation.
- This question is not applicable because the project is ending.
- This NREL data project was undertaken in conjunction with, and in support of, the major DOE fuel cell vehicle learning demonstration activity. Activity associated with the learning demonstration program has been completed; NREL's work on this project is winding down and will be completed this year. DOE is currently planning new vehicle and $H_2$ infrastructure evaluation projects. NREL's team is supporting DOE in launching these initiatives. DOE is also utilizing NREL's data handling and analysis capability for the benefit of technology validation activities related to other fuel cell applications.

### Project strengths:

- This project features strong analysis connections to the developmental industry and demonstration efforts.
- This project has an excellent team of experts and an excellent track record of digesting and presenting results of FCEV testing.
- This project's strengths include its strong project team, proven analysis methodology, and extensive participation of project partners that provide basic data on vehicle and infrastructure performance.
- The experience and expertise of NREL's team, including Keith Wipke, NREL's team leader, is a strength. Other areas of strength include industry's confidence and trust in NREL's team and approach to the project, and the continuous improvement and enhancement of project products, particularly CDPs and DDPs. The project provides a significant contribution to the merits of the vehicle learning demonstration (technology validation) program for a relatively small expenditure of the total program resources.
- One strength is this project's many years of statistical data analysis and data collection—it is too bad this will be ending, because additional data is required to demonstrate improvements in technology.

### Project weaknesses:

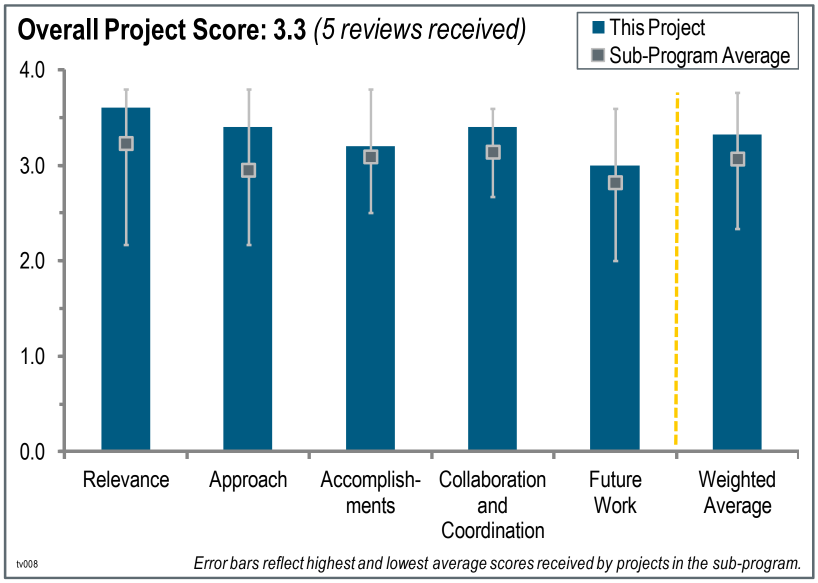
- It would be even more effective if more partners participated.
- This is not really a weakness, but more information on the specific benefits that the project partners have derived from the project results would be helpful. Perhaps case studies could be an option.
- One weakness is the "premature" shut down of refilling stations and other partners no longer involved—it raises the question of how to get commitments for the longer term.
- Two reviewers could not identify any weaknesses.

### Recommendations for additions/deletions to project scope:

- This project is too important and useful to end—it needs to be continued in some form.
- The project is near its contractual end. Continuation and expansion are recommended, if funding becomes available.
- The project being reported on is nearly complete; its final report has been published. DOE is urged to maintain and utilize the data analysis expertise existing at NREL, which has been built as a result of this project. It seems this is being done in the context of other validation initiatives and projected future activities.
- The database and technical analysis team should be maintained through the next generation of FCEV deployments. If possible, someone should keep track of the FCEVs with the longest-running fuel cell stacks.

## Project # TV-008: Technology Validation: Fuel Cell Bus Evaluations

Leslie Eudy; National Renewable Energy Laboratory




### Brief Summary of Project:

The overall objective of this project is to validate fuel cell technologies in transit applications. The specific objectives for 2012 are to: (1) document more than 10,000 fuel cell hours and double the fuel economy compared to baseline technology (diesel and natural gas buses); (2) continue data collection and analysis for second-generation fuel cell buses at Burbank, SunLine, and AC Transit; (3) collaborate with the U.S. Department of Transportation (DOT) and the Federal Transit Administration (FTA) to collect data on sites for the National Fuel Cell Bus Program (NFCBP); and (4) conduct crosscutting analysis and comparison of fuel cell electric bus (FCEB) status at all sites.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- This project featured excellent relevance to DOE needs regarding gathering bus operating data for analysis and public summary.
- Next to passenger vehicles and forklift trucks, FCEBs are the best option for widespread fuel cell utilization and public visibility.
- Transit operations are an integral part of the DOE Hydrogen and Fuel Cells Program (the Program); thus, the project that monitors FCEB performance and cost compared to conventional technologies is a relevant component of the Technology Validation sub-program.
- While this data is necessary to collect, and demonstrations are critical for future implementation of the technology, it is unfortunate that there is no post-mortem analysis to understand why some of the buses failed so early. It is highly commendable that a bus reached >12,000 hours, but it would benefit the community to couple this with an explanation of why this was possible.
- The Program has devoted substantial resources to fuel cell vehicle and hydrogen ($H_2$) infrastructure technology validation projects. Buses have been an important target of opportunity for fuel cell development and demonstration. Significant funding, from both DOE and DOT, has been provided for FCEBs. The National Renewable Energy Laboratory's (NREL's) collection, analysis, and reporting of performance data associated with vehicle demonstration projects, including those focused on buses, has made a vital contribution to understanding the status of technology development relative to DOE's goals. NREL's data products have been continually refined, increasing their value for both government and industry decision makers. The detailed, objective results of NREL's work are easily understood by those responsible for decisions on public and private investment in technology research, development, and commercialization. Over the past few years, this work has been an important contributor to achieving unbiased and supportable conclusions about progress toward DOE and DOT targets—bus fuel economy, fuel cell bus utilization, durability, miles between roadcall, fueling rates, and other metrics.

### Question 2: Approach to performing the work

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

- The approach is strongly based on the highly successful approach used in the Hydrogen Fleet and Infrastructure Analysis and is focused on self-reporting by participating transit agencies.
- This project features lots of data collection, but it lacks some understanding as to what factors affect lifetime, fuel economy, and performance. More information regarding “out-of-service” would have been appreciated.
- The approach involves looking at a broad spectrum of fuel cell, hybrid, and diesel buses for real operating environment comparisons. The data cover a variety of bus and fuel cell manufacturers, transit systems, and fuel sources. The data are analyzed on various bases relative to DOE targets. A key value of the project is the presentation and publication of the results.
- Since commencement of bus evaluations in 2003, the principal investigator (PI) and her team have steadily refined this project’s data collection, analysis, and reporting activities. With ongoing feedback from data providers and users, NREL has developed a logical approach that results in routine periodic production of high-quality, informative reports. These reports provide outstanding documentation for industry, government, and the public. Since 2010, data has been collected on an increasing number of buses. The process uses data already collected by transit agencies. Data is collected on conventional diesel buses, as well as natural gas and diesel hybrids, for comparison purposes. The current second-generation fuel cell bus evaluation process is building on the experience gained during operation of first-generation buses from 2005 to 2010. Several steps could be taken to improve project results and benefits. These include: (l) acquiring additional data on the performance of advanced technology hybrid electric buses; (2) improving access to warranty costs; (3) and increasing the ability to compare buses with similar age, size, and service conditions. These issues are understood by the PI. They were not included in the project presentation, but they are alluded to in the reviewer-only slides. With regard to information on slide 14, it is recommended that more intensive investigation be done into the reasons for the unavailability of fuel cell buses. Particular focus should be on “bus maintenance,” which accounts for more than 60% of bus unavailability for two transit agencies.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has produced good results so far.
- This project made good progress on reporting the performance of first-generation buses—the project is moving on to second-generation buses, with significantly more buses included in the study. The project features significant publications and presentations that are important to the dissemination of project findings and conclusions.
- Much good data has been obtained from the bus operators, and it has been thoroughly and understandably analyzed. The analyzed results show improvements in fuel cell bus technology over many performance parameters: fuel economy, availability, repair frequency, lifetime, cost, etc. The overall results are very positive for the future of fuel cell buses. The slides are simple and easily understandable. Some qualitative parameters could be added; for example, drivers’ personal comments. Many publications and presentations resulted from the analysis work.
- This project provides the best, most objective, and most independent indicators of progress toward DOE’s targets and goals for metrics associated with FCEBs and related $H_2$ infrastructure. Ms. Eudy’s presentation includes readily understandable graphs that enable reviewers to compare goals and bus performance for key metrics, including monthly service miles and fuel economy. Results are presented for each bus type and transit agency. Variability in factors influencing performance, such as duty cycles, is noted. For selected metrics, such as miles between roadcall, the improvement in second-generation fuel cell buses is cited. The presentation’s highlights of selected accomplishments during the past year are impressive. Taken together, they provide an appreciation for the merits of both NREL’s data project (reported on here) and the progress—for example, hours in service before repair or replacement—of fuel cells used in operational buses. An annual FCEB status report, with analysis comparing fuel cell and other bus results for all FCEB locations, is published. Publication of this report is complemented by papers on individual bus projects and presentations at selected conferences.

### Question 4: Collaboration and coordination with other institutions

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

- There are many very useful collaborations in this project.
- The project has good collaboration between partners, but communication could be improved a little.
- The project features good collaborations with project partners—the quality of project results is dependent on collaborations with transit operators.
- As indicated on slides 8 and 19, NREL's FCEB data project involves collaboration and routine communication with every transit agency in the United States that operates any type of fuel cell bus. NREL's evaluation team routinely coordinates with federal and other government organizations; transit agency management and operating personnel; bus manufacturers; fuel cell and related system providers; hybrid electric technology providers; and others that have a stake in FCEB research, development, and commercialization. NREL's bus data collection and analysis team has earned the trust of all organizations participating in the FCEB demonstration program. NREL's team also maintains excellent communications with many organizations sponsoring related activities, both in the United States and other countries.

### Question 5: Proposed future work

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

- There are good plans for expanding transit agency participation in the project—significantly more partners will be contributing to the program as more fuel cell buses are introduced into the national fleet.
- The proposed future work is good, and the project team should continue as planned. The team should expand into other (new) bus operations, if possible, as well as add other fleet vehicles—for example, forklifts, delivery vans, mine vehicles, and other types.
- This NREL data project was undertaken in conjunction with, and to support, FCEB demonstration projects supported by U.S. government agencies. New FCEBs are being funded with the support of NFCBP. Slide 20 of the presentation provides an excellent display of bus projects to be evaluated through 2013. Buses to be evaluated will be operated in all regions of the country. NREL's team intends to continue its dialogue with transit agencies and others regarding data collection at new sites.

### Project strengths:

- The project features a good team, database, and analysis.
- This project has a strong team and good methodology based on a successful automotive project.
- This project has good analyses of real operating data as well as widespread dissemination of results, which are convincing.
- The project features a large mass of data, but its goals are unclear and recommendations to improve performance in buses are not part of the project.
- A strength of this project is the experience and expertise of NREL's team, including the PI. Funding support from FTA is also a strength. This averages about $250,000 per year. (This information should be included in the presentation.) Another strength is the active collaboration and interaction with manufacturers and users of advanced technology buses. The project has made a significant contribution to the merits of the FCEB demonstration program for a relatively small expenditure of total program resources.

### Project weaknesses:

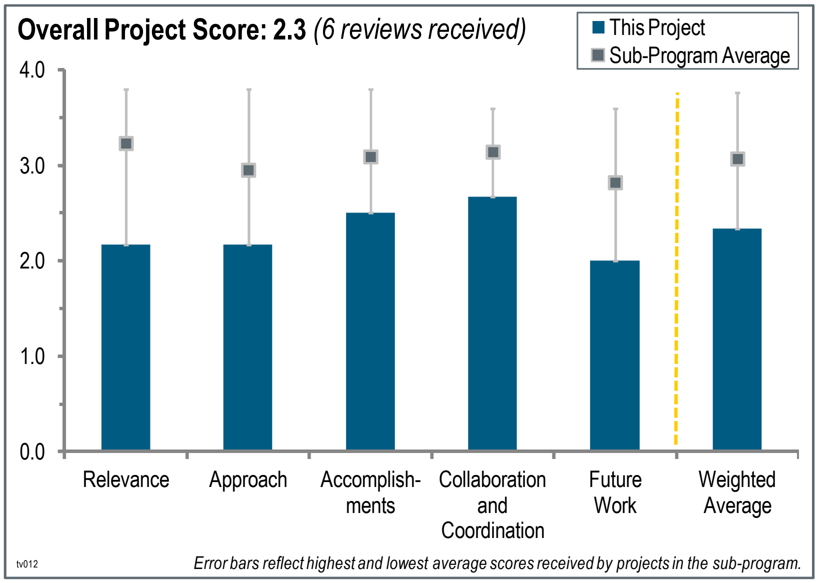
- The data collection is challenging and results are encouraging, but it is still unclear how the data is being used to lead to technological improvements.
- Achieving statistically valid performance comparisons among buses is inherently difficult, due to factors beyond NREL's control. Such factors include regional differences, transit agency procedures, the variety of bus types, multiple fuel cell bus design strategies, variability of duty cycles, and differing service profiles. Another weakness is the limitations on cost details provided by bus manufacturers and transit agencies.
- Two reviewers did not identify any weaknesses.

### Recommendations for additions/deletions to project scope:

- The project team should continue and expand as much as possible.
- The project team should add a component to the project to encourage interpretation and recommendations for further improvements.
- The project might add a new category such as “Powertrain Availability.” Ideally the “availability” (or lack thereof) should only reflect fuel cell system failures. Fuel cell technology should not be penalized due to other bus failures, accidents, etc. Of course, the diesel and compressed natural gas buses should be treated the same, with availability only dependent on powertrain failures.
- DOE is urged to maintain and utilize the bus data analysis expertise available at NREL, which has been built as a result of this project. Ms. Eudy mentioned sharing of information with organizations in other countries. Continuing the initiative is encouraged because it could lead to comparisons of performance results for FCEBs around the world.

## Project # TV-012: Florida Hydrogen Initiative (FHI)

David Block; University of Central Florida




### Brief Summary of Project:

The objectives of this project are to: (1) develop hydrogen ($H_2$) and fuel cell infrastructure, (2) create partnerships, (3) sponsor fuel cell and $H_2$ research and development (R&D), (4) facilitate technology transfer, (5) develop industry support, and (6) develop unique education programs. Twelve individual projects were competitively selected; six have been completed to date, and nine were presented at the 2012 DOE Hydrogen and Fuel Cells Program (the Program) Annual Merit Review and Peer Evaluation meeting.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.2** for its relevance to DOE objectives.

- Each of the individual projects in the overall umbrella project is relevant to a specific DOE objective.
- Technology validation is very important and this effort took a number of attempts across a number of areas. Unfortunately, this effort has not resulted in any significant technology validation. There are eight projects that make up this Florida Hydrogen Initiative (FHI), but few of them relate to the others, which diminishes the overall impact.
- Selection of projects outside of the normal Program Funding Opportunity Announcement (FOA)/Annual Operating Plan (AOP) process makes it difficult for DOE to ensure the strong relevance of the projects. In addition, the very long duration of time between project award and completion of individual projects makes this even harder.
- Some of the sub-projects underway as a part of this project appear to be relevant, while others do not bring any new, unique contributions to the Program. The sub-projects are not selected by the DOE programmatic procurement process; thus, they may or may not fit in the Program's portfolio.
- FHI manages individual, independent projects using DOE funds appropriated prior to fiscal year (FY) 2009. Some of the nine active projects contribute to achieving the Program's goals and objectives and include activities that can be linked to DOE's targets. The most relevant projects are "Development of a Low Cost, High Efficiency polymer electrolyte membrane (PEM) fuel cell System" – Florida State University (FSU)/Bing Energy, Inc.; "High Efficiency, Low Cost Electrocatalysts for Hydrogen Production and Fuel Cell Applications" – Florida Solar Energy Center (FSEC); "Chemochromatic Hydrogen Leak Detectors for Safety Monitoring" – FSEC; and "Advanced HiFoil Bipolar Plates" – EnerFuel, Inc. References to a DOE target were mentioned only twice in the presentation's 83 slides. During discussions at the poster session, leaders for two of the projects above demonstrated familiarity with DOE's targets applicable to their work.
- This is a project involving nine diverse and different tasks. With a couple of exceptions (e.g., "Portable Fuel Cell" and "Hydrogen Storage"), it is not very clear how they relate to DOE's quantitative targets and goals. With the exception of the "Hydrogen Technology Rest Area," this project does not generally fit in the category of technology validation. Eight projects seem like R&D, not technology validation, making it difficult to rate this project by the Technology Validation sub-program's standards and goals.

## Question 2: Approach to performing the work

This project was rated **2.2** for its approach.

- Basically, this is a collection of unrelated projects. The individual projects all have reasonable approaches to achieving their particular objectives.
- The approach to selecting new sub-projects for this project seems to be reasonable. However, it is noted that all of the sub-project participants seem to be from the state of Florida. It is questionable if the best possible, most qualified staff are secured for each of the research areas. The sub-projects appear to be a mixed bag of unrelated activities. There is no common theme, focus, or technical thrust to the selection process for sub-projects.
- Again, having multiple projects within a project makes the approach difficult to assess. The management of this effort has changed multiple times, leading to a scatter-shot technical approach that bears no resemblance to an integrated plan. Enerfuel, FSU, the University of Central Florida, the University of South Florida (USF), Florida Institute of Technology, and Florida Atlantic University are all capable performers and represent a diverse set of partners. The renewable $H_2$ from renewable methanol was poorly conceived; it lacked a domestic source of supply and had to be ultimately cancelled. The lack of a coordinated approach to FHI fails to move the state forward in any real way toward an $H_2$ infrastructure or any significant validation of promising technologies.
- Across the nine active projects within FHI, the purposes are varied and the range of activities is very broad. When the projects are considered together, it is not possible to describe an overall approach with clarity. No attempt was made to do so in the poster presentation. The overall initiative has little, if any, relationship with technology validation. Across the nine projects, there are elements of basic research, applied R&D, training and education, technology transfer, and partnership development. For two projects, a description of the approach was well stated. For others, the approach was either not included or so general that it was not useful.
- This project has a bewildering array of distinctly different activities, most subcontracted by FSEC to others. Some seem to be interesting and possibly unique. Others seem similar to other foreign and U.S. activities. The principal investigator (PI) has not made clear presentations as to which approaches are really new and how they relate to others (other than virtually all are being performed in Florida).

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Progress appears to be adequate, with sub-projects being completed and new ones initiated.
- The accomplishments of the individual projects have been of a varied nature. Most of the projects have shown reasonable results, although none of them has produced outstanding results. There was one project that was quite interesting, and one project that was disappointing.
- Overall, decent progress has been made on the individual projects. Some have nearly completed demonstrations, and some have proven new technologies. Given that most of the projects are two years old or less, overall the accomplishments are respectable and generally (though not in all cases) address DOE goals.
- Good work has been accomplished by some of the nine projects within the Florida Hydrogen Initiative. In particular, DOE should examine closely the results of four of the sub-projects. A short conversation with the team leader for task 11, "Development of a Low Cost and High Efficiency 500 W Portable PEM fuel cell System" (slide 58), made it clear that some of the R&D has led to patent applications and commercial interest. Technical accomplishments are also reported for task 7 (slides 22 through 27), task 8 (slides 33 and 34), and task 14 (slide 80). Note: "Projects" are referred to as "Tasks" in the poster presentation.
- There are some interesting results, and significant quantities of data have been obtained that should be of value to the Program. It is not always made clear how much advancement has been made beyond national and international states of the art. From the funding summaries, it seems that all tasks have not been funded in FY 2011 and FY 2012. Half of the tasks have already been completed, and the remaining tasks are to be completed by the end of calendar 2012.
- "EV Charging Station Powered by a Fuel Cell" achieved moderate success (Enerfuel and Florida Atlantic). The mechanical and chemical durability of membrane electrode assemblies (MEAs) does not appear to be a noteworthy project with significant validation. The production of $H_2$ from biowaste poster did not justify assertions, including the projected cost/kilogram of the resultant $H_2$ and the efficiency of the bromine-hydrogen process. "Design and Development of Advanced $H_2$ Storage System" using novel materials did not have reproducible results. "Advanced HiFoil Bipolar Plates" (Enerfuel) appeared to achieve good results. "Low Cost

High Efficiency 500W Portable PEM Fuel Cell" achieved good results (>90% platinum [Pt] utilization using "Bucky paper"), but FSU/Bing ended up transitioning the technology to a Chinese supplier.

### Question 4: Collaboration and coordination with other institutions

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

- Collaborations on the individual projects were found to be somewhat limited.
- There are significant collaborations, but virtually all are within Florida. If this is really a DOE-funded activity, there should be more national and international collaborations.
- There are collaborations within this project, but partners are paid participants and there appears to be little or no interaction between sub-projects. In fact, the sub-projects have little in common and thus little opportunity for collaboration.
- The PI engaged with a wide range of Florida-based universities and companies in the eight projects that were undertaken. There was very little coordination and collaboration among the institutions involved. They each had their own project, but they did not participate in review or coordinate with each other. Little technology transfer has occurred despite the large number of universities involved.
- FHI, managed by FSEC, funded proposals submitted in response to a request for proposal. To accomplish the work being done within the various projects, FSEC has partnered with multiple organizations in Florida, primarily universities. The manager of FHI maintains excellent knowledge regarding each of the nine active projects. Based on a discussion with him at the poster session, it is clear that he coordinates routinely with project leaders, and that he is quite familiar with project plans, content, progress, and results.

### Question 5: Proposed future work

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

- All projects will be ended by December 2012.
- This project is scheduled to end this year. The subjects should be moving to completion on that schedule.
- The future work is qualitatively reasonable, but it does not seem to be very well directed toward DOE targets and barriers. All tasks are shown as ending in the next seven months or are presently unfunded.
- Future work was not planned in a timely and logical manner. The poor performance of many of the projects and the need to ask for even another extension to December 31, 2012, do not show good schedule or risk management. There are no significant barriers that appear to have been eliminated as a result of these sub-projects. The Technology Validation sub-program has not been advanced as a result of this project.
- FHI is nearly complete. No DOE funding has been provided since FY 2008. Three of the nine active projects are reported as complete by FHI; the rest are scheduled to be finished by the end of calendar year 2012. The incremental additional work on the six projects still to be completed is not expected to contribute substantively to the achievement of DOE objectives. There are no plans to request more DOE funds or extend activities beyond this year.

### Project strengths:

- Each individual project has relevance to DOE programmatic goals.
- The project contains some interesting approaches.
- Strengths include the two projects conducted by Enerfuel: "EV Charging Station Powered by a Fuel Cell" and "Advanced HiFoil Bipolar Plates." "Low Cost High Efficiency of PEM Fuel Cell System" achieved some interesting results in terms of Pt utilization using Bucky paper.
- One area of strength is the research capabilities of some project teams. In recent years, the management attention provided by FSEC was a strength. As a result, some beneficial results, and relevant additions to knowledge, have been achieved by FHI.
- The diversity of activities is an area of strength. The projects seem to be fairly well focused, although objectives are not in all cases aligned with (current) DOE goals. The Florida A&M/Bing Energy MEA structure looks interesting—the work should be followed up.

## Project weaknesses:

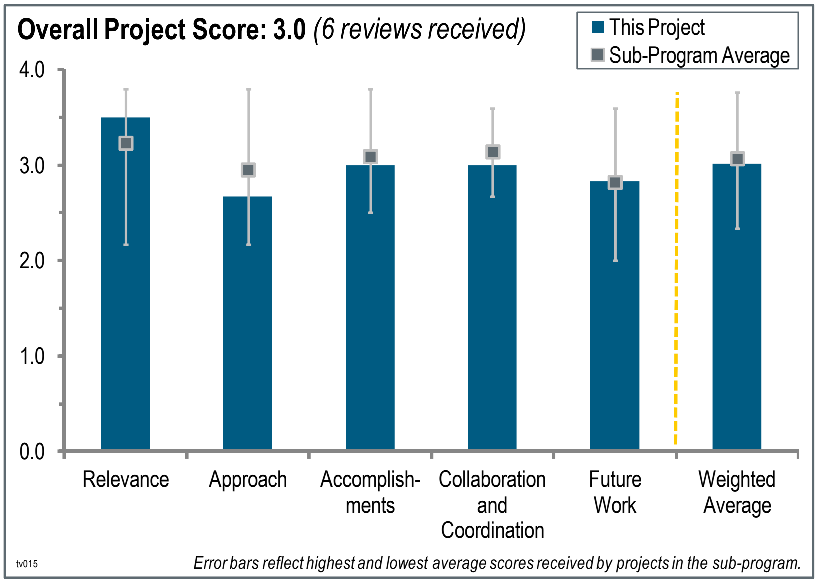
- The overall project is essentially an assembly of unrelated individual projects.
- There was no coordination of sub-projects and no selection of sub-projects on the basis of a single, common technical goal or objective. The project includes an inefficient process in terms of technical oversight and overhead costs (for FSEC), and it is of questionable benefit to the Program.
- The overall project represents a handful of discrete projects that have no relationship to one another. “Advanced Hydrogen Storage System” did not produce repeatable results. “Hydrogen from BioWaste” did not demonstrate results that were measurable. The project leadership team has undergone turnover, resulting in delays and starts and stops for many projects. There has been poor technology transfer despite the large number of universities involved. FSEC did not demonstrate enough initiative to quickly make adjustments and decisions related to the work plan when difficulties were encountered.
- FHI is a collection of individual, unconnected projects with widely varied activities and objectives. DOE resources have been divided among many relatively small projects, with no overarching focus. There is a lack of clear linkage among most project activities, DOE technology targets, and barriers to achieving the targets. There is minimal or no collaboration with industry for most projects.
- The project is not focused well toward DOE targets and objectives, and it is not very collaborative beyond the borders of Florida. All tasks can be led from Florida, but there should be some national and international collaboration to optimize the efficiency and value of the project.
- The overall approach of soliciting and funding projects apart from the normal DOE FOA/AOP process is poor. The $H_2$ leak detection tape sub-project seems not to hit the major objective of a sensor—an electronically monitorable response (voltage output). The requirement of visual inspection to see tape change color seems to greatly limit applications. The low-cost catalyst sub-project needs to show industry interest. It is not clear how the bipolar plate project is providing new materials, processes, or products.

## Recommendations for additions/deletions to project scope:

- For the “EnerFuel HyTech Rest Area,” DOE should ensure that operational data is collected from the demonstration. Regarding the academic program, someone should evaluate if this activity is replicable to other universities. The activity does not do DOE much good if it cannot be adopted other places. The $H_2$ storage project at USF did not appear to accomplish any goals—it should not be continued.
- Project activities should be completed this year as planned.
- Four reviewers had no recommendations, since the tasks are all ending in December 2012.

## Project # TV-015: Wind to Hydrogen

Kevin Harrison; National Renewable Energy Laboratory




### Brief Summary of Project:

The main objectives of this project are to: (1) perform characterization and performance testing on electrolysis systems developed from U.S. Department of Energy (DOE)-awarded projects; (2) test electrolyzer stack and system response with typical renewable power profiles; (3) develop and validate a system to characterize hydrogen ($H_2$) mass flow; (4) identify opportunities for system cost reduction and optimization as they pertain to electric utilities; (5) characterize, evaluate, and model integrated renewable energy systems (IRESs); (6) evaluate the reliability growth of IRESs; (7) characterize electrolyzer performance with variable stack power; and (8) design, build, and test shared power electronics and direct-coupled renewable-to-stack configurations.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This project is relevant to the goals and objectives of the DOE Hydrogen and Fuel Cells Program (the Program). $H_2$ production is an important component of the Program and electrolysis is being supported by the Hydrogen Production and Delivery sub-program.
- The project is quite relevant to DOE plans and objectives. It is especially useful to include testing of DOE-funded systems.
- $H_2$ storage of intermittent renewable energy could provide a much-needed boost to renewable energy deployments.
- Validation of electrolyzer systems is essential, but it is unclear how such manufacturers/systems are selected. Validations were only presented for Proton OnSite, and it was unclear if Proton OnSite is the only manufacturer. Responses to tests are only assessed by a single person, and it is unclear if there is a panel.
- Electrolyzers are "reverse fuel cells" and have a lot in common with regular fuel cells. They also can work in conjunction with fuel cells in a renewable microgrid. For that reason, it is critical to the fuel cell industry to prove itself in this new and potential huge market—Ancillary Grid Support Services. With many states adopting renewable portfolio standards, utilities are having to deal with renewables whether they initially wanted to or not. Electrolyzers have been mentioned in the context of renewable energy storage and grid support, but not to the same degree as batteries, nor has there been much, if any, technology validation from independent sources. This project provides evidence that electrolyzers can perform the grid frequency support mission. This effort ties in three parts of DOE: the Office of Electricity, the Fuel Cell Technologies Program (FCT Program), and the Wind Program. Integrated projects across multiple DOE programs can have a higher payoff.
- The relevance of this project can be considered from multiple perspectives: performance testing of electrolysis systems developed by other DOE projects, technology development and understanding, testing and characterization of electrolyzer system response to renewable power profiles, and evaluation and modeling of integrated renewable energy systems. Some technical targets for water electrolysis $H_2$ production are provided on slide 4 of the presentation. While statements on development needs are also included, the slides and oral presentation did not establish direct linkages between project activities and progress toward meeting the targets.

## Question 2: Approach to performing the work

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

- The independent component testing of electrolyzer and fuel cell systems is highly valued.
- The approach focuses on testing and evaluation of electrolysis equipment. The real value of this project should be the benefits it contributes to the commercialization and implementation of this $H_2$ production technology. It is not clear how the approach supports the dissemination of results of the study and its potential impact on the $H_2$ production marketplace to obtain renewable, cost-competitive $H_2$.
- The project addresses many barriers to the use of electrolyzers in a microgrid. A significant amount of testing is included in this project, so as to leave little doubt that the results are indicative of actual performance. The only negative is that so many areas were addressed in the project approach. It might have been too much, especially because the project is only given $450,000 or so per year in resources. For instance, the $H_2$ pressure vessel mass flow measurements were not critical to the outcome of the project. It was pointed out by one of the reviewers that off-the-shelf $H_2$ gas measurement systems are available for purchase. On the other hand, it is a credit to the initiative of the co-principal investigators (PIs), who were willing to take on so much. Good partners were chosen for the effort. Having a utility (Xcel Energy) involved was critical, as were all of the domestic electrolyzer manufacturers.
- To someone not familiar with this project, the activities seem to go in too many directions and try to serve too many purposes. The approach stated in slide 5 supports this conclusion, as does the overall presentation. There is good work being accomplished, but the clarity of the project's primary purposes is somewhat lost in the maze of its varied activities. Besides evaluating and validating technological progress, the project has elements of a test facility, a user test facility, technology research and development, a diagnostics laboratory, technology modeling, and test procedure development.
- The approach seems sound, but it is limited and somewhat arbitrary—it is unclear how systems for testing/validation are chosen. It seemed that only companies willing to donate a system could participate, and it would be interesting to know about the others. It is unclear if all of the manufacturers are approached to participate.
- This project is investigating a number of useful controls and system improvements to achieve optimum integration of electrolyzers with inherently variable alternative energy systems (especially photovoltaic and wind power). Correctly, cost reduction is the main barrier addressed. Although this is an excellent engineering simulation, it is not always clear why the work needs to be largely experimental. It would seem that more of the work could be modeling, based on the previously known performance of electrolyzers, control components, and wind power. There is question as to whether it is always necessary to validate with an actual integrated system experiment.

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Great progress has been made—the PI seems to really understand the systems and is completely capable of conducting these assessments. More coordination among the manufacturers for arranging/scheduling validations for meaningful comparisons would have been appreciated.
- Progress on this project seems to be fairly slow. A lot of time and effort was devoted to building and qualifying a mass control system when readily available mass flow and controllers could have been used. It was not clear why this approach was taken.
- Great progress was achieved on all fronts. Having the data on electrolyzer performance in a renewable (simulated) microgrid, especially more than 6,000 hours, was a significant accomplishment. Industry benefitted as well, as exemplified by the PI telling of a situation with Proton OnSite in which "tightening some nuts" on the electrolyzer was the solution to a set of field failures that had the company puzzled as to its cause. The IRES demonstration does indeed fill in gaps where industry does not have the data. Opportunities to reduce system costs—based on real data—will be identified and shared with industry as a result of this effort. Excess costs were mitigated by testing electrolyzers that were bought under other DOE-funded projects.
- Some activities reported on in the presentation are impressive. Examples include the characterization of stack performance under varying current conditions, development of the prototype diagnostics laboratory now available at the National Renewable Energy Laboratory (NREL), mass flow measurements, and construction of

an integrated "complete system" for testing and evaluation. As a project that emphasizes technology development and testing, contributing to technology progress toward targets—or at least documenting progress—is important. However, no specific information was provided about how the work on this project has overcome barriers to achieving DOE's $H_2$ production goals and targets. Slide 2 states that the project started in 2003. In the oral presentation, however, the PI said it is a "new project" this year. In either case, quantitative information is needed on how the project is overcoming barriers.

- A reasonable number of accomplishments have been achieved, and real-world data have been generated. It was not always clear exactly what was done in the last fiscal year and what may have been done earlier. The measurement of $H_2$ production via a pressure technique is good and can be directly used in the final commercial products. There were many public presentations and publications. One or two examples of problem solving for vendors were cited. This is excellent, but it begs the question of whether commercial suppliers of fuel cells and other components should be expected to contribute financially to the laboratory effort. After all, the end commercial market will be theirs.

### Question 4: Collaboration and coordination with other institutions

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

- This project featured excellent collaborations and communications.
- There are collaborations with various governmental agencies, but for the most part collaborations seem to be superficial. More collaboration with actual hardware developers, working together in the hardware testing and evaluation activities, would strengthen the project.
- Validation projects should be "objective and independent" rather than focusing on single manufacturers, and they should not be derailed by additional responsibilities such as development. The validation aspects were diluted in some way because of other distractions.
- Each institution that is a part of this team (California Fuel Cell Partnership, California Department of Food and Agriculture, electrolyzer original equipment manufacturers [OEMs], and Xcel Energy) has benefitted from the knowledge gained from this effort. NREL in general does a superb job in collaborating, and the co-PIs in this project were no exception. More could have been done in terms of collaborating with the U.S. Department of Defense, which has funded a large amount of federal microgrid work to date. DOE will certainly benefit from the collaborative nature of this project, involving objectives from the DOE Wind Program, FCT Program, and Office of Electricity. Perhaps it will generate ideas for further projects in this area.
- Slide 19 indicates that the project staff has collaborations with a variety of organizations on a variety of topics related to electrolysis and $H_2$ production. Work with Xcel Energy and the California Department of Food and Agriculture was cited in the presentation. Slide 10 mentions a contract between two California government agencies, but it does not identify NREL's role. Information on funding for this project from sources other than DOE would be helpful.

### Question 5: Proposed future work

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

- The future path is unclear—it looks like the project is coming to a close.
- Future plans seem to be more of the same. Work in the future should focus on integration with private-sector hardware manufacturers bringing competitive hardware and electrolysis processes to the commercial marketplace through incremental product improvement.
- It appears that this project—or set of projects—has built upon previous milestones in a systematic and logical manner. It was difficult to make an assessment of the past 12 months of work because the key decision points had passed. The PI laid out a logical path forward for after this project is completed in October 2012.
- Future work plans, as presented on slide 20, include validation of electrolysis $H_2$ production systems, continuation of stack testing, continuation of work on mass flow, and development of a renewable electrolysis system integration simulation tool. No information is provided on milestones, work priorities, or costs projected for various elements of the project.
- In general, the proposed future work seems reasonable. The effort should be continued. Given the obvious commercial potential for electrolyzer-alternate energy integration, it is reasonable for the electrolyzer and utility industries to take over more of this work.

### Project strengths:

- The project features very good systems analysis, reliability of data analysis, and testing protocol.
- Practical $H_2$ production (electrolysis) and alternative energy integration work are strengths of this project.
- Good research facilities have been accumulated at NREL. The project has good testing capabilities to provide third-party validation of technologies.
- Strengths include the experience and capabilities of NREL's team; NREL's credibility and collaboration with U.S. electrolyzer manufacturers; and the importance of improved knowledge and understanding about integration of renewable energy, $H_2$ production, and $H_2$ storage.
- The project has many strengths:
  - Incorporation of real-world renewable microgrid scenarios into a broad demonstration.
  - Significant amounts of data generated.
  - Strong team assembled for the effort across all key disciplines.
  - Leveraged other programs' previously purchased assets to keep costs down, making the project achieve even a greater degree of accomplishments compared to the cost.
  - Utilized actual wind profiles in the demonstration.
  - Included all domestic electrolyzer OEMs in the project.

### Project weaknesses:

- The project team is trying to do too much, resulting in project dilution. The project should focus on testing and validation only.
- The project probably needs more financial support from industry.
- A minor weakness might be the lack of full system analysis and integration.
- This project seems to be doing the job that should be done by private-sector hardware developers—testing and evaluating commercially available equipment. The project should be refocused on precompetitive technology issues that would benefit the electrolysis community as a whole.
- The project may have taken on too many tasks—specifically the design, build, and test of an $H_2$ volumetric mass flow system. There are already commercial off-the-shelf products on the market for that.
- The broad scope of activities included within the project is a weakness. Resources are spread across too many types of work. Other weaknesses include the lack of a clear linkage among project activities, DOE technology targets, and barriers to achieving the targets.

### Recommendations for additions/deletions to project scope:

- The project is almost over. Sharing the results with the Technical Director of the Smart Power Infrastructure Demonstration for Energy Reliability and Security (SPIDERS) Joint Concept Technology Demonstration is highly recommended. That effort is in the midst of performing a Military Utility Assessment on microgrids (to include renewables), with United States Pacific Command overseeing the effort. In response to a question, the PI stated that the primary project goals are testing of electrolysis $H_2$ production technology developed by industry, and improved understanding and insights about renewable energy to $H_2$ systems and related technologies. DOE should consider narrowing the scope and types of activities included within this project. A suggestion is to focus on creating a robust, integrated user facility capable of evaluating technologies and components developed by industry, universities, and other national laboratories. Other work, such as research on mass flow, technology development, and control strategies, could be performed by other contractors.
- The team should use an integrated wind/electrolyzer/storage/fuel cell system to verify models of variable wind energy storage. This may not fall under the scope of this project, but someone at NREL should conduct a dynamic analysis of a wind/electrolyzer/storage/fuel cell system, matching the dynamic supply of wind electricity in the Boulder region with the dynamic electricity load; thereby establishing the economic value of $H_2$ storage with different storage times (assessing the value of days, weeks, months, and hopefully seasonal storage). This project could then conduct the integrated experiment suggested above to validate the dynamic model.
- This reviewer had no recommendations.

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## 2012 — Safety, Codes and Standards
## Summary of Annual Merit Review of the Safety, Codes and Standards Sub-Program

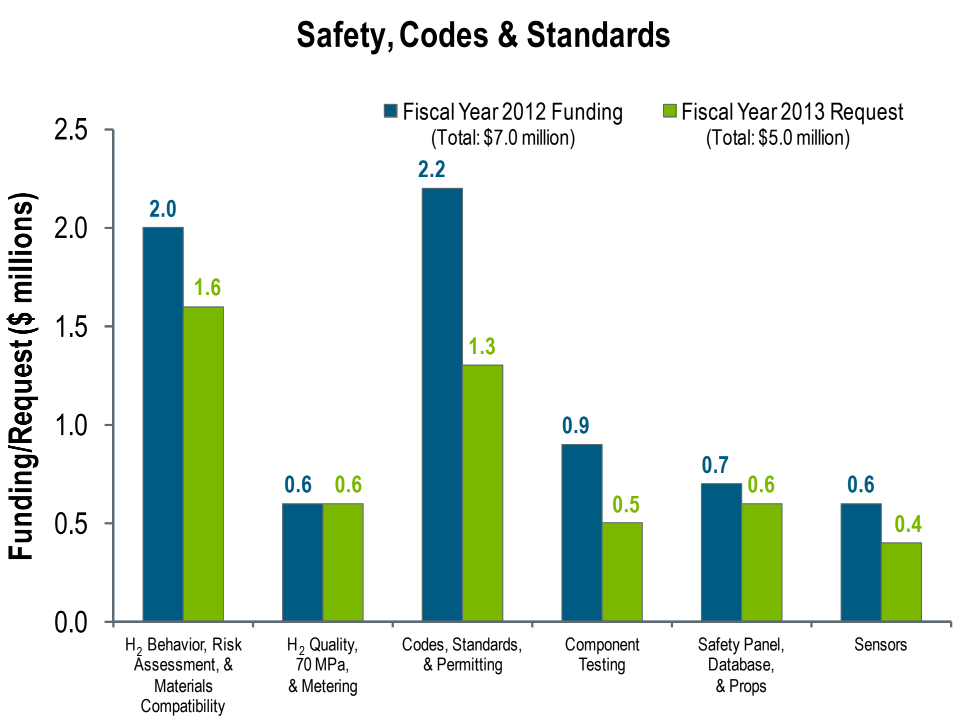
### Summary of Reviewer Comments on the Safety, Codes and Standards Sub-Program:

The Safety, Codes and Standards sub-program supports research and development (R&D) that provides the critical information needed to define requirements and close gaps in safety, codes and standards to enable the safe use and handling of hydrogen and fuel cell technologies. The sub-program also conducts safety activities focused on promoting safety practices among the U.S. Department of Energy (DOE) projects and developing information resources and best practices. Reviewers observed that the sub-program continues to provide strong support in the following areas: hydrogen and fuel cell codes and standards, permitting, and education; hydrogen sensor technology; hydrogen components and material compatibility work; safety training for first responders and researchers; and development of an international hydrogen fuel specification standard. Reviewers repeated similar observations from prior years that projects in this sub-program have effectively leveraged the resources and intellectual capital of academic institutions, standards development organizations (SDOs), national laboratories, government agencies, industry, and other offices in DOE.

In addition, this year reviewers commended the sub-program for a strong international participation with a focus on international harmonization for the safe deployment and early market commercialization of fuel cells and hydrogen. Reviewers felt that the sub-program was well-focused, but better alignment between the R&D and safety implementation aspects of the sub-program would allow for better cohesion.

### Summary of Safety, Codes and Standards Funding:

The fiscal year (FY) 2012 appropriation was $7 million for the sub-program. FY 2012 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 2013 request of $5 million will continue these efforts.



## Majority of Reviewer Comments and Recommendations:

In FY 2012, 10 projects were reviewed in the Safety, Codes and Standards sub-program, with a majority of projects receiving positive feedback and strong scores. Reviewers' overall scores ranged from 3.0 to 3.8, with an average score of 3.3.

**Codes and Standards and Permitting:** One codes and standards and permitting project was reviewed and received a score of 3.4. The reviewers commended this project for its strong core team and coordination with critical codes and standards development organizations. However, the reviewers suggested that the project should focus more in the State of California, where deployment will happen in the near future.

**Component Testing:** One component testing project was reviewed and received a score of 3.0. The reviewers commended the good progress and strong coordination with sensor manufacturers. Reviewers suggested that testing in higher concentrations of hydrogen would be beneficial.

**Hydrogen Behavior, Risk Assessment, and Materials Compatibility:** Three hydrogen behavior, risk assessment, and materials compatibility projects were reviewed, with an average score of 3.3. Reviewers commended the technical expertise found within these projects and the impact on standards development. The reviewers suggested continued collaboration with appropriate SDOs, incorporation of sub-zero temperatures for materials compatibility, and publication of a web-based qualitative risk assessment tool for indoor releases.

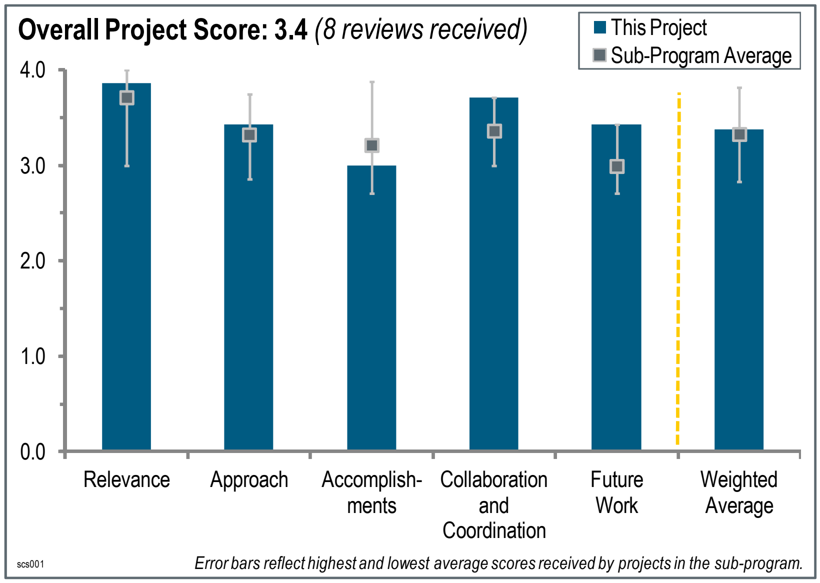
**Hydrogen Quality, 70 MPa, and Metering:** One hydrogen quality, 70 MPa, and metering project was reviewed, which received a score of 3.8. This project was awarded with the highest score within the sub-program. Reviewers commended this project for making steady progress with the advancement of the technology. Reviewers suggested the incorporation of short stack testing as a next step.

**Safety Panel, Database, and Props:** Three projects in these areas were reviewed with an average score of 3.4. Reviewers stressed the importance of these projects to the deployment and commercialization of hydrogen and fuel cell technologies, especially in the case of the Safety Panel. Reviewers highlighted the importance of the information dissemination for the databases. They suggested further alignment with key agencies such as the national fire academy, various regional/state organizations, and emergency medical services.

**Sensors:** One sensor project was reviewed, which received a score of 3.0. Reviewers saw good progress toward developing a reliable, cost-effective hydrogen safety sensor, which will be used for hydrogen infrastructure and stationary fuel cell applications. The reviewers commented that a more active role from the industry partner and a cost analysis of manufacturing would be beneficial.

## Project # SCS-001: National Codes and Standards Coordination

Carl Rivkin; National Renewable Energy Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) conduct research and development (R&D) needed to establish sound technical requirements for renewable energy codes and standards with a major emphasis on hydrogen ($H_2$) and fuel cell technologies; (2) support code development for the safe use of renewable energy in commercial, residential, and transportation applications with a major emphasis on $H_2$ fuel cell electric vehicle technologies; (3) advance renewable energy safety and code development by collaboration with stakeholders; and (4) facilitate the safe deployment of renewable energy technologies by working directly on key codes and standards projects and $H_2$ technology deployment projects.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.9** for its relevance to DOE objectives.

- This work is critical in enabling the transition from demonstration to true commercial deployment.The project is an essential component of the overall SCS sub-program, and its effectiveness is critical for the success of the sub-program.
- Continued funding of this project is absolutely critical in order to continue the progress of creating and shepherding the necessary codes and standards (C&S) to develop a hydrogen and fuel cell economy.
- The project is inputting data to the C&S development process, and is currently working on many codes. The National Renewable Energy Laboratory (NREL) should coordinate the different projects to establish sound technical requirements for R&D and support code development.
- This project is critical and well aligned with the needs of the Safety, Codes and Standards (SCS) sub-element of DOE’s Fuel Cell Technologies Program. As the roll-out for 2015 moves forward, the SCS activity, including this project, is on a critical path.
- Coordinated C&S development, gap analysis, research support, and national and international outreach are crucial to facilitating deployment of vehicles and refueling infrastructure that meet consumer expectations of safety, reliability, and convenience (i.e., on par with conventional personal transportation). Every year this effort builds on previous successes and targets new areas in need of support to ensure international harmonization of C&S.

### Question 2: Approach to performing the work

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

- The project approach appears solid, has sufficient collaboration, and is addressing a very comprehensive list of standards organizations.
- The project employs a good, comprehensive approach to addressing C&S internationally, achieved through various coordinating committees, targeted workshops, and a web-based information compendium.

- The project should address conflicts for C&S. Coordination is a key element; gap analyses provide a framework for which projects to address first; California is a focus; hosting workshops is critical.
- The project appears to be very connected with the relevant C&S organizations. The principal investigator (PI) clearly brings a great deal of expertise in this community. The PI clearly understands the process and what is rate limiting, which is critical to best focus resources and efforts on those rate limiting, critical road blocks that might get in the way of the code development process.
- This project has shown good work done to date on national coordination and gap analysis. A good next step would be to coordinate with key international fuel cell market regions on key topic areas. Also, the project needs to engage the International Organization for Standardization (ISO) at some point and work toward better coordination.
- The overall approach is based on coordination, but more specific information on the project's role in this coordination is needed. Much of the presentation is at a programmatic level, not at the individual contributor's (project) level. There is too much cataloguing of organizations associated with the project's effort and not enough on what is unique or important about the project's approach in overcoming barriers by increasing synchronization of national C&S and access to safety data and information. The "coordinating tool" (slide 5) may be a useful tool for coordination, but it is not obvious from the slide itself how effective the tool has been and what impact it has had on increasing coordination among standards development organizations (SDOs) for the benefit of the Safety, Codes and Standards sub-program.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project has done excellent work tracking the efforts of C&S communities.
- This project has made good progress on its goals and has a well-identified plan of future work with Federal Motor Vehicle Safety Standards (FMVSS).
- It is important to continue this work through 2020. The capabilities and connections that have been established will be a key resource for parties involved in commercial deployment of $H_2$ stations and fuel cell vehicles.
- This project's accomplishments include work on the National Fire Protection Association (NFPA) Hydrogen Technologies Code (NFPA 2), coordination and direct support of the C&S development process, the 2020 vehicle deployment plan, and its management of several contracts.
- In general, the project showed programmatic accomplishments and progress, but not enough of its specific contributions and how essential they were. For example, slide 7 shows the "hierarchy" of regulations, codes, and standards (RCS), but not what the project did to build or solidify the pyramid, only that "NREL did extensive support" to build it. Slide 8 shows highlights of the 2020 Vehicle Deployment Plan, while slide 9 claims that NREL has "defined a key path to deployment of HFCVs [hydrogen fuel cell electric vehicles] in the United States." This reviewer questioned whether the Vehicle Deployment Plan is this path. If so, much more detail about the plan is needed. Slide 8 shows only "key findings"—not a path to deployment. More detail is also needed on slide 9: for example, this reviewer questioned what "guidance documents" the project produced to help California deploy vehicles and what were its "major contributions" to the Society of Automotive Engineers (SAE) J2601 and the CSA H series. In summary, the project failed to show important details of its work, how its work was integrated into the overall DOE program, and how its work contributed to meeting the program's overall goals.
- The project's accomplishments and progress continue. This work has supported various C&S development over the years for components, vehicles, buildings, and refueling. The vehicle deployment plan for 2020 identifies key issues/barriers in RCS, and NREL is working with California closely as the lead market for rolling out vehicles and infrastructure. The work's focus and facilitating RCS in California is a good testbed for expanding lessons learned there to a wider national and international deployment. If similar markets exist, for example, in Japan, NREL should work with those nearer term market areas as well. A lot of contracting support for C&S for NFPA, SAE, ISO, CSA, and supporting projects for Global Technical Regulation (GTR) and NFPA 2.
- Developing the 2020 plan helps focus the efforts on what is necessary to accomplish the goal. This project does a nice job in keeping abreast of a diverse selection of code development organizations (CDOs) and SDOs and brings the needs forward to DOE to address gaps as they emerge. Indeed, there are examples of NREL sub-contracting to individuals to accelerate the harmonization of codes between NFPA and the International Code Council (ICC). This activity was put into play to accelerate the efforts of the Hydrogen Industry Panel on Codes

(HIPOC) in harmonization of domestic codes. This project provides a valuable activity to keep tabs on the C&S organization, providing an integrated point of contact for activities and identifying gaps, problems, and opportunities.

### Question 4: Collaboration and coordination with other institutions

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

- The project's component testing must be performed in coordination with the R&D activities.
- This work has lots of partnerships, collaborators, and teamwork involved.
- It will be important to strengthen international collaborations going forward.
- This project is very well connected in the CDO and SDO community. This activity is vital to the acceleration of the deployment of $H_2$ technology. Indeed, this project is well connected to those who are trying to roll out the infrastructure, such as California through the California Fuel Cell Partnership.
- The project works with all key domestic and international SDOs and has played an important role in coordinating C&S development. Participation by and involvement of SDOs on the $H_2$ fuel cell C&S Coordination Committee conference calls are excellent.
- This project has displayed a lot of collaborations with national laboratories; CDOs and SDOs such as SAE, CSA, ISO, NFPA, the California Fuel Cell Partnership, and international organizations such as the International Partnership for the Hydrogen and Fuel Cell Economy (IPHE), and the World Forum for Harmonization of Vehicle Standards (WP29) GTR; and information shared through meetings of the U.S. DRIVE Partnership's Codes and Standards Technical Team.

### Question 5: Proposed future work

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

- This project is well planned and executed; the future planned work is right on.
- This project will work at the leading edge of vehicle deployment; component testing must be coordinated.
- This project identifies future work with FMVSS and slides 8–9 help demonstrate the planned activities and partnerships needed to continue progress.
- Future work includes continuing past work with a focus toward developing GTR/FMVSS, assisting code officials, focusing on key deployment areas (California), and working at the leading edge of deployment to reduce barriers to refueling station deployment. This is a needed natural progression from SDOs for safe vehicles to C&S for infrastructure to support the vehicles. Future work is needed to assist in refueling infrastructure as the next step in deployment.
- Proposed future work is again very general, with a lack of detail: stating that the project will "work at the leading edge of vehicle deployment to reduce barriers to fueling station deployment" is almost nonsensical. The development and promulgation of FMVSS were deemed not critical for the targeted 2015 deployment of fuel cell electric vehicles by a key auto original equipment manufacturer during review of the 2020 plan, but this is one of the "key project areas" that the project will support (slide 15). Project plans (brought out during the questions and answers session) to hold a workshop on component testing and certification and to develop a fueling station permitting template for California are good and should have been highlighted in the presentation. Note that the guidance to AMR presenters should place more emphasis on providing details of proposed future work.
- Recommendations for future work include: (1) investigate the needs of maintenance bays at dealerships (i.e., sensors, permitting, etc); (2) provide support for developing a new flow meter that can operate within the fueling protocol limits with 1.5% accuracy or better; (3) support the American Society for Testing and Materials (ASTM) Inter Laboratory Study to validate ASTM standards (the current process is lengthy and will not establish precision and bias statements within the five years required by ASTM); and (4) support the development of an $H_2$ cleanliness standard. As stations are being built, there needs to be a specification to clean equipment to.

### Project strengths:

- This project displays a good framework for domestic building codes.
- This project is comprehensive and, in general, is moving forward/closing gaps in vehicle C&S development to infrastructure.
- The PI and team are well connected and very knowledgeable. One could not have asked for a better team to engage in this particular critically needed activity. The team is very good, and the execution is very good.
- The project has played a key role in the national coordination and international harmonization of $H_2$ and fuel cell RCS and continues to work along a path that is well established. The project interacts with all of the essential domestic and international SDOs and remains a key player in the RCS community.
- The project organizes and coordinates across a broad array of organizations and topic areas. It provides a roadmap for a C&S world that can be overwhelming to new players and even to established industry players undertaking new projects, such as the retailing of hydrogen fuel.

### Project weaknesses:

- One reviewer felt there were no weaknesses.
- The project's weakness is the length of time being taken to develop the codes, standards, and regulations. This looks to be a 15- to 20-year program.
- Many codes exist and it may be time to focus on the implementation of these codes, especially in California. As the community gets real experience deploying real systems to real customers, the program should focus on learning and improvement.
- The project needs to better define its performance indicators, how to measure its progress against these indicators, and how to determine its contributions toward DOE goals. The project's recent presentations at the AMR have been overly general, reflecting DOE programmatic activities, but not providing sufficient detail on the project's role, contribution, and effectiveness in meeting DOE goals and program objectives.
- The project needs further work to simplify the key messages on what's needed, what's in place, what the gaps are, and where the risks are. All the information seems to be there, but still seems to be missing an effective executive summary format.

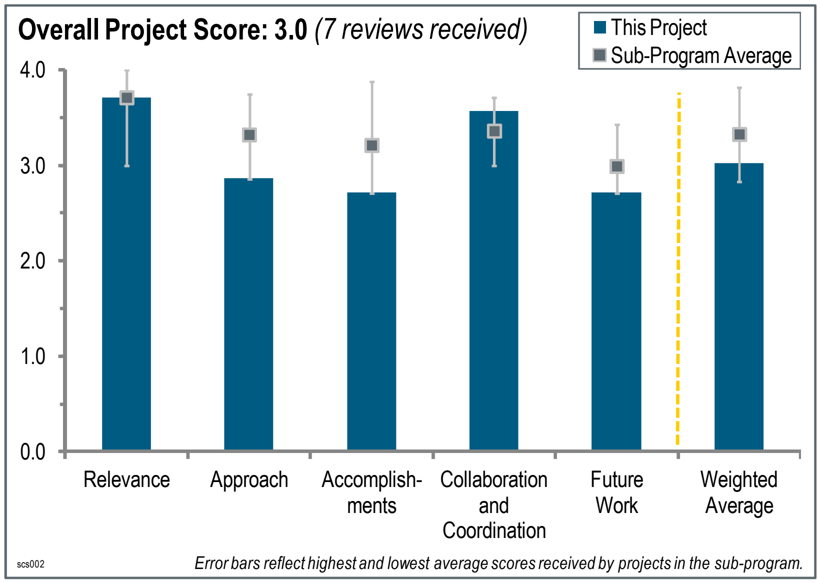
### Recommendations for additions/deletions to project scope:

- The team should plan on working directly with the authorities having jurisdiction in California.
- It is difficult to address this as the project scope is large and general—a more targeted project scope should be defined by NREL, one that can be assessed against specific performance indicators.

## Project # SCS-002: Component Standard Research & Development

Robert Burgess; National Renewable Energy Laboratory

**Brief Summary of Project:**
The objective of this project is to help ensure safe deployment of hydrogen ($H_2$) fuel cell technologies by conducting hydrogen component research and development (R&D), both through the National Renewable Energy Laboratory's (NREL's) internal testing efforts and through subcontract programs, thus determining which components are proven to meet new safety and performance standards. Additionally, NREL's component R&D accomplishments have provided a sound technical basis for new $H_2$ codes and standards requirements and have supported industry by providing independent third-party assessment of performance against those requirements.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.7** for its relevance to DOE objectives.

- Development of sensor and component standards is absolutely critical to future program success.
- Sensor testing is very relevant to the eventual commercialization of $H_2$ technologies.
- The project did at one time have relevance to DOE's objectives, but the industry already has $H_2$ sensors on the market that meet the objectives for safety, etc.
- Component and sensor testing is a critical path issue for $H_2$ technology roll out. Timing is critical, as the industry and governments prepare for the 2015 vehicle deployment around the world and domestically in New York and California.
- Components of $H_2$ and fuel cell technologies must be safe and reliable. There is a need to acquire technical data, especially on the most recently developed technologies to support/revise standards. Therefore, component standard research and development is critical to the Hydrogen and Fuel Cells Program and fully supports the DOE objectives.
- The general focus of getting the science correct before writing $H_2$ component specifications is good and is required for new technologies. This presentation leans heavily on the discussion of point sensor developments and the facility NREL has constructed in Colorado. The second section of the presentation concerned NREL support for standard development organization (SDO) efforts at standardization. Testing is critical to getting adequate specifications. Both of these areas (sensors and component testing) are important to the $H_2$ economy.

### Question 2: Approach to performing the work

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

- The testing methodology is adequate.
- Working with sensor developers and manufacturers is effective.
- The testing being performed in this project is essential to future sensor development efforts. However it is difficult to pull out of the presentation how this information is being used to affect codes and standards.

- There are limited facilities that perform the types of testing required for component development. NREL is to be commended for creating a facility that can perform some of this testing. However, there should be more participation from industry members.
- The approach is good and contributes to overcoming some barriers. The participation in different technical committees and the work performed during international cooperation with different laboratories allow researchers to identify knowledge gaps and could offer immediate benefits to the industry. The project could be improved by obtaining stronger cooperation from the original equipment manufacturers (OEMs) for the identification of real standard conditions and environments faced by the fuel cell components in different applications. Nevertheless it is recognized that such cooperation is difficult to obtain.
- This is a nice piece of work and much needed to understand the performance characteristics of point sensors. Providing capabilities to quantify how to use, response functions of, and domain over which the sensors behave (response function) are all critically important to ensure the sensor application is correct and the output is interpreted correctly. However, a critical element that is missing is the investigation (characterization) of large area sensor technology. NREL would be well advised to develop the laboratory investigation capability necessary to investigate the class of sensors of wide area detection.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.7** for its accomplishments and progress.

- Sensor testing is going well, but the sensor workshop results were not published in a timely manner.
- Sensor progress has been good. Testing in support of J2579 and the Global Technical Regulation (GTR), as well as the CSA standard on pressure relief devices for compressed $H_2$ vehicle fuel containers (HPRD1) has been useful.
- The project showed a sensor technology that has already been in fuel cell electric vehicles (FCEV) since 2004 (helium sensors).
- This program has made good progress. It is moving a little slower than expected, but that might be driven more by available funding than any fault of the principal investigator (PI) or NREL.
- This presentation highlights predominantly test activities and results, showing a small amount of progress on standards development. Good progress has been made supporting sensor developers and participating on standards groups for technical guidance. The presentation, however, does not specifically highlight codes and standards progress.
- Providing an independent, third-party assessment of the performance of fuel cell components is an accomplishment of high value and corresponds with significant progress toward the objectives. The new $H_2$ codes and standards may acquire a sound technical basis through the work performed in NREL. Performing "post mortem" analysis of deficient components could also help the industry to improve its product. This could be taken into consideration for the future.

### Question 4: Collaboration and coordination with other institutions

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

- This project has good collaborations with international testing laboratories.
- There are good collaborations with institutes and international efforts.
- The project team is very much engaged with the sensor community and the relevant standards organizations.
- Increased participation by NREL personnel within the code development community has been useful in presenting technical data and interpreting that data for code writers.
- Collaboration with codes and standards committees, national partners, industries, and international institutions is close and appropriate, and coordination is granted.
- The international collaborations developed by this team are very good: in particular, the collaboration with the Joint Research Center (JRC) Institute for Energy and Transport (IET) is very constructive. The two facilities are similar (but complementary), providing a natural cross check on results and a leveraging opportunity, which has been/is being exploited. In times of shrinking financial resources, this type of collaboration is critically needed. Nicely done!

### Question 5: Proposed future work

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

- This project will continue to work with technical committees.
- This project does not align with industry needs anymore, so proposed future work is not needed. Standards (ISO TC 197, WG 13 and UL) already have baseline information.
- The future plan for creating a prioritized list of needs for component standards validation testing indicates that the project has a goal but doesn't indicate how any potential barriers will be overcome.
- Some of the sensor work should be shifted to wide-area sensors, as this is the mostly likely area where there will be a need. The infrastructure is attempting to comply with building and fire codes with inadequate wide-area sensors.
- The effect of $H_2$ on valve and pressure gauges, which are in contact with $H_2$, should be added. To determine long-term sensor stability, real-world deployment scenarios should be mimicked. Forty weeks is not enough time.
- The proposed future work is planned in a logical manner and considers possible barriers to the goals. Stronger cooperation with OEMs could be considered, even if it is difficult. Sharing results with other research centers instead of trying to perform all tests is an excellent initiative and will help to disseminate these results internationally.
- A critical element that is missing is moving this program to investigate wide-area sensors. We are on the threshold of aggressive fueling station deployment (globally) where wide-area sensing is needed to ensure the detection of an unintended failure. This reviewer was disappointed that investigation of wide area sensors was not on the future plans for this program.

### Project strengths:

- The analytical methodologies for testing $H_2$ sensors are a strength of this project.
- Excellent relationships with the SDOs and the sensor manufacturers are clear strengths of the project.
- The technical capabilities of the national labs are excellent. If the laboratories continue to perform testing and supply the industry with the data, everyone benefits.
- $H_2$ detection and sensor development is a critical part of the safety and performance of any $H_2$ system. As was stated during the presentation, there is way more work than any one laboratory can perform. This project has great collaboration and interaction with other test laboratories as well as SDOs.
- The program has developed a nice facility (not fantastic but adequate) to measure the operating characteristics of a variety of point wise sensors. The program works well with industry (particularly the sensor manufacturers), but also with the code development/regulatory (U.S. Department of Transportation) stakeholders. The international collaborations, in particular the JRC, are a very good and strong point. Largely, the work done on the pointwise sensors and working with the development and end use industry is critically important and clearly a strength.

### Project weaknesses:

- This project is no longer connected to industry needs. Some testing is questionable, such as crash testing in a conventional vehicle.
- This project does sensor testing only, and diagnosis of failure is not part of this work. This testing-only effort may not be appropriate for a national laboratory.
- The presentation does not do this project justice, as it does not highlight the good work being done on standards development.
- The definition of real operating conditions and environment for different applications could improve the obtained results and their use in defining new codes and standards.
- As pointed out above, wide-area sensors must be embraced as part of this activity. It is disappointing to see that this was not even in the future planning for this work. As pointed out earlier, wide-area sensors are critically needed to ensure safe operation of $H_2$ infrastructure implementation, like fueling stations. This is a critical shortcoming of this work.

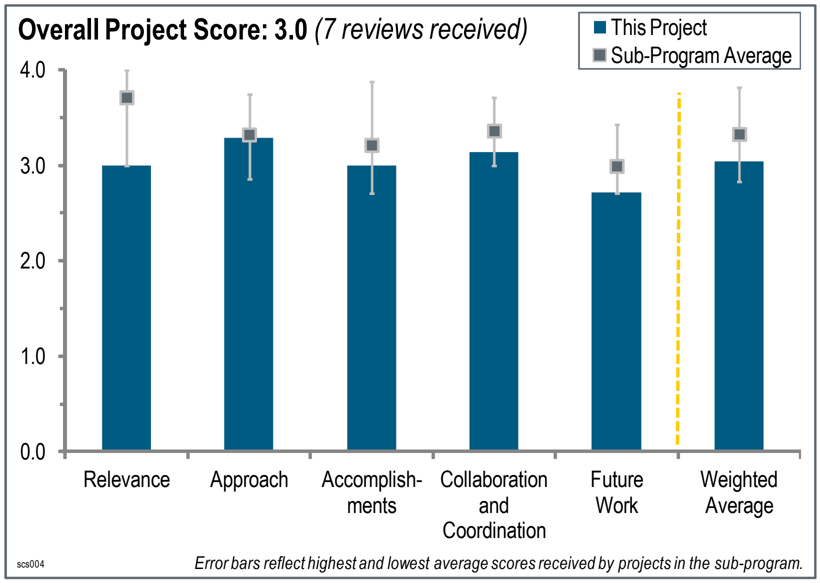
- This work takes an inordinately long time for data to get publicly released. As an example, as referenced in the reviewer comments from last year, the brinelling issue, which was brought to the Society of Automotive Engineers (SAE) in 2003 with an update from Japanese OEMs in 2008, has been pending for years. The priority of the technical investigation into this problem has slipped.

### Recommendations for additions/deletions to project scope:

- Include wide-area sensor technology as part of the overall sensor portfolio.
- This project should include work on wide area sensors. It is also recommend that plastic material be investigated for use in the low-pressure side of fuel cell systems. This would be a relatively short test program, with a good payoff to the industry.
- One reviewer suggested that this project be cancelled.
- Work already appears to be in progress on this, but testing is at higher $H_2$ concentrations. This next recommendation may best be addressed by creating another project, but what is needed for large-storage $H_2$ systems is development of wide-area sensors, not just point sensors.

## Project # SCS-004: Hydrogen Safety, Codes and Standards: Sensors

Eric Brosha; Los Alamos National Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) develop a low-cost, durable, and reliable hydrogen ($H_2$) safety sensor for vehicle, stationary, and infrastructure applications, through material selection, sensor design, and electrochemical research and development (R&D) investigation; (2) demonstrate working technology through rigorous life testing and application of commercial (reproducible) manufacturing techniques; (3) disseminate packaged prototypes to the National Renewable Energy Laboratory (NREL) and work toward commercialization by engaging appropriate industry partners; and (4) pursue transfer of the new sensor technology and commercialization through industry partnerships. NREL will evaluate sensor performance and ensure adherence to codes and standards, field evaluation, and performance requirements.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The development of low-cost, durable, and reliable $H_2$ safety sensors for vehicle, stationary, and infrastructure applications is an important need for the DOE Hydrogen and Fuel Cells Program.
- Given that point sensors will be used for their $H_2$ detection ability, this is a very relevant project.
- It is unclear if this project has support from automotive and station original equipment manufacturers. This reviewer wonders if the industry need has been demonstrated.
- The principal investigator (PI) has successfully developed an $H_2$ sensor that meets the detection criteria for an $H_2$ safety sensor: detection at 25% of the $H_2$ lower flammability limit (LFL) with tolerance of up to 10% water.
- These relevance objectives are similar to objectives used by the power electronics researchers at Oak Ridge National Laboratory. The objectives involve working with industry to find a viable approach to an issue, in this case sensor drift, and working to mitigate that problem within an industry, not an academic, framework.
- The project supports the safe use of $H_2$—specifically by developing $H_2$ leak detection technologies such as sensors. End users have indicated that they require robust, reliable, high-performance, and cost-effective $H_2$ sensors; as such, the market for these sensors is confirmed. Research to develop such sensors is essential to supporting their use and commercialization, thereby facilitating the safety of $H_2$ applications.
- The project aligns with DOE objectives to develop reliable, cost-effective $H_2$ safety sensors. The focus of this development should be primarily for $H_2$ infrastructure applications and secondarily for stationary fuel cell applications. An objective of the project is to develop a safety sensor for both applications and also for vehicular applications (slide 3). The sensor technology can address all three applications, but if NREL is to "evaluate sensor performance and ensure adherence to codes and standards," a more focused project objective would be better and more likely to meet DOE objectives. Furthermore, it will be difficult for NREL to conduct "field evaluation and performance requirements" (slide 3) for all three application areas within the project time and scope. The pursuit of industry partners for technology transfer and commercialization is in good alignment with DOE objectives.

### Question 2: Approach to performing the work

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

- It is excellent to see the focus on using commercial manufacturing processes.
- The work has been very logical in developing the sensor, performing testing, and addressing issues.
- The work does focus on one sensor technology only. The stability issue seems to be on the road to a solution.
- The overall approach is focused on the critical barriers for $H_2$ sensor technology.
- The approach to transfer a demonstrated technology from one application (oxygen [$O_2$] detection) to another ($H_2$ detection) is commended. The choice of developing this particular technology, which is claimed to be conducive to miniaturization, suggests amenability to cost-reduction, which is a critical factor for successful commercialization.
- Most of the work was to fix a signal interface problem with diagnostic equipment. The researchers solved the problem and appropriately isolated the sensor from the diagnostics―this is necessary for a real-world application. Even though it may have seemed to be specific to the NREL measurements, it is applicable to a broader application space, which is good.
- Basing the approach on the Lambda $O_2$ sensor and working with industry partners is good. The industry partners are helping to address the mass fabrication of the sensor element and interface electronics (slide 7). Los Alamos National Laboratory (LANL) and Lawrence Livermore National Laboratory (LLNL) have addressed key sensor parameters of aging, reproducibility, and selectivity in laboratory devices, but it is not clear how these parameters and others identified (long-term stability, drift, exposure to the environment, etc.) have been addressed in the context of technology transfer and commercialization (a key objective—slide 4). With the project 80% completed, it would seem that the commercial industry partner (ElectroScience Laboratories, [ESL]) would be much more involved in addressing the challenge of meeting key performance parameters at a given price point.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This is cutting-edge technology—the project is a good demonstration of robustness and response time.
- The progress is good, but not outstanding.
- The project only focuses on one sensor technology. Control electronics are certainly critical, and the work accomplished to date is encouraging.
- The PI made adequate progress since the last DOE Hydrogen and Fuel Cells Program Annual Merit Review. The team demonstrated 6,000 hours of sensor life, performed further testing at NREL, and resolved electrical issues with measurements performed at NREL.
- The pre-commercial prototype performed well in a static volume testing experiment, but this hardly seems to qualify as a "more real-world" scenario (slide 8). The aging and tolerance to humidity and temperature response testing at NREL are good technical accomplishments for the laboratory development of an advanced sensor; however, there is little indication of accomplishment and progress toward technology transfer and commercialization with one year remaining until project completion.
- Based on the literature listed in the references to the presentation, the sensor is expected to show good performance. Nevertheless, the measurements reported in the paper show a number of deficiencies, the cause of which is neither clear nor explained. Other comments from this reviewer include:
  - Regarding the very long t90 response time in the graph on slide 8, it is unclear if this is caused by a leaking chamber, and whether the last part in the graph corresponds to chamber evacuation. It is unclear why the sensor response in the graph on slide 8 does not match that in the top graph on slide 12 (claimed to be initial, i.e., pre-NREL tested response).
  - There is contradictory information from both graphs in slide 9. The right-side graph indicates an increasing output signal with increasing H2 content (as expected, and also complying with the logarithmic concentration dependence). However, the left-side graph shows a decreasing signal magnitude with increasing concentration (likely due to an incorrect legend in the figure).
  - Contrary to the claim made in the first bullet on slide 9, the sensor is not reliable during the first 1,000 hours (as shown in the left graph).

- Results in the right-side graph suggest the presence of non-negligible hysteresis, which is not further elaborated upon.
- Data shown on slide 11 not only show “strange baseline behavior,” but also a substantially decreased sensitivity. The label on the left vertical axis in both graphs is wrong.
- No explanation is provided for the quicker response and for saturation of the sensor response, as apparent from the bottom graph on slide 12.
- Regarding packaging, in addition to the evidence provided in the graph on slide 14, the stability of the sensor under environmental conditions (i.e., varying temperatures and pressures) should be demonstrated.
- In terms of the second bullet on slide 15, the high impedance buffer (HIB) does not protect the response of the sensor; it may protect the sensor during handling, although there is no direct evidence of that.
- Regarding the top graph on slide 16, the magnitude of the sensor response at saturation (75 mV) for 2% H2 is quite different from what is shown in all of the other graphs, but there is the comment appended to the graph about the “correct” sensor output obtained without use of gain. This reviewer wants to know how this discrepancy can be reconciled.
- The bottom graph of slide 16 indeed shows high signal-to-noise ratio when HIB is enabled. However, the logarithmic sensitivity dependence seems to have vanished.
- No indication is given about the threshold level of the sensor or how this is affected when using HIB. In summary, some progress seems to have been made to bring this sensor to the pre-competitive stage, including incorporation of the resistive temperature detector (RTD) and control electronics. However, the results presented do not allow this reviewer to confirm these claims. Because this reviewer is of the opinion that this is more due to the low quality of the presentation and the lack of information provided in the slides and during the actual oral presentation, a score of “fair” is maintained. However, lessons should be learned for the future. Also, issues regarding cross-sensitivity to other species, identified in previous years, do not seem to have been addressed. Results from a larger number of sensors are definitely required to demonstrate performance reproducibility.

### Question 4: Collaboration and coordination with other institutions

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

- The level of collaboration with other national laboratories and industry partners appears to be very good.
- The manufacturers/developers are fully involved. That is an asset.
- One reviewer would like to see industry support beyond sensor manufacturers.
- Collaboration with industry indicates the potential for this technology. The roles of the collaborators have been clearly defined and there seems to be appropriate coordination of project activities.
- The team coordinated with Sandia National Laboratories and NREL to perform tests and address issues related to sensor electrical circuit and noise. The team is starting to coordinate with small companies to address commercialization.
- This project is a development activity and does not lend itself to extensive outside collaboration. Within the constraints of intellectual property and partners, another reviewer thinks the calibration is about as extensive as possible at its current stage of development. The researchers need to embrace a manufacturer as part of the team sooner rather than later. This also leads this reviewer to wonder if these are the correct people to move this to the manufacturing stage as opposed to a manufacturer.
- Collaboration among national laboratories (LANL, LLNL, and NREL) is excellent, and the industrial partners are playing essential roles in the project. It is not clear to what extent, if any, ESL (the “commercial industry partner”—slide 2) is involved or concerned with the commercialization of the technology.

### Question 5: Proposed future work

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

- The plans for future work are a logical continuation of previous efforts. Other technologies would be good.
- The focus on a low-cost commercial product is essential. The team did good work in identifying the issue at NREL.

- The project is on track, but progress is a bit slower than one would have expected. It took a year to understand and isolate the signal processing issue.
- The future plans were focused on commercialization, but that plan seemed somewhat vague. This reviewer is not sure that a national laboratory is the right institution to be focusing on commercialization of the sensor system.
- A commercial development partner (or partners) should have been involved by now in the project. The future work should focus on issues that most affect commercialization of the technology, because the project is 80% completed.
- The proposals for future work clearly build on the results achieved to date. Seeking commercial development partners seems premature for the stage the sensor is currently in—more evaluation and R&D is required, particularly with regard to long-term stability under real-world conditions, cross-sensitivity, and reproducibility. Detection threshold and response time characteristics should also be addressed.
- The project should have a future task to complete a comprehensive cost analysis based on a complete commercial product assessment, including electronics. Slide 7 indicated that the focus was on technology commercialization and listed key commercial-related tests and criteria. The future work appeared generic rather than focusing on these key commercial requirements.

## Project strengths:

- The project features a strong partnership with industry.
- The project seems to be making excellent progress with a potential low-cost sensor technology.
- The PI and the team have a good understanding of technical aspects of the sensor system and how to tune the functionality of the mixed-potential-type sensors.
- This technology shows sufficient promise to merit further development. Miniaturization of technology offers potential cost and performance benefits.
- The technical competence of the national laboratory partners is strong, and the involvement of industry partners in technology development and scale-up is good. The use of NREL's sensor testing facilities and expertise is also a strength.

## Project weaknesses:

- Because of funding restrictions, only this type of sensor will be investigated to this degree.
- There is no manufacturer on the team.
- The team needs to perform a higher-fidelity analysis of the manufacturing costs before pursuing commercialization.
- The lack of commercial development partners at this late stage in the project is an area of weakness. The project's success will depend on the transfer of the sensor technology to industry and the commercialization of the technology so that the sensor can improve the safety of $H_2$ facilities and applications. To date, there is little evidence that the technology will be fully transferred to industry and result in a cost-effective safety product.
- The team needs a better approach to align the test set-up at NREL with LANL and LLNL to avoid the issues with anomalous behaviors that are seen at one laboratory and not at another. This issue of inconsistent results appears to have increased the development time.
- Cross-sensitivity to other species has been consistently identified by sensor end users as an important performance criterion. Cross-sensitivity is an issue with this technology. Response stability and reproducibility may also be of concern. The project has not identified for which application(s) this type of packaged sensor is most suitable. The impact of packaging on the additional features (RTD, HIB) and on overall power consumption has not been addressed. The quality of the presentation of the results constitutes a weakness in the execution (probably not in the actual performance of the work and in the scope) of the project.

## Recommendations for additions/deletions to project scope:

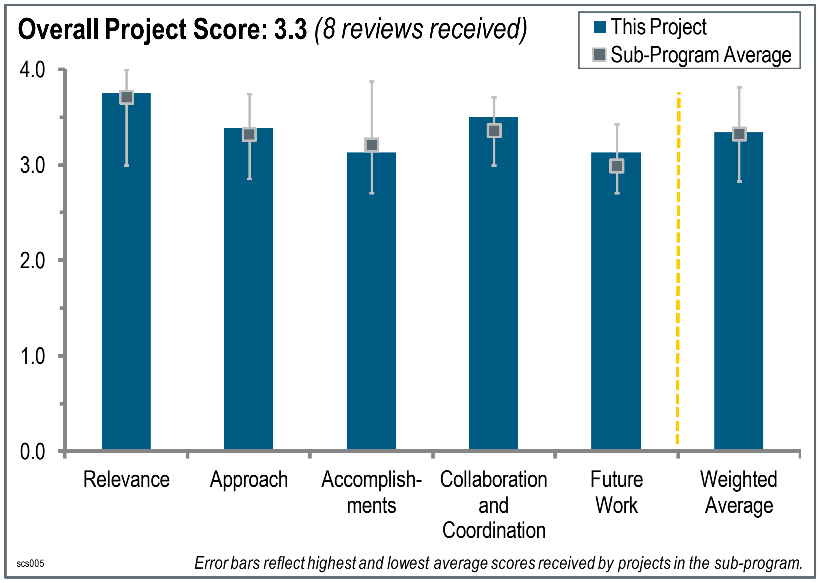
- The team should find a manufacturer to partner with or turn over the lead to.
- The team should pursue directed testing for infrastructure applications (i.e., refueling installations).
- High-fidelity cost analysis should be conducted by an independent organization that has experience with design for manufacturing and assembly (DFMA).

- The team should focus on what is critical for technology transfer (for a commercial development partner to cost-share in the remaining portion of the project).
- Potential markets for this sensing technology should be investigated, as should manufacturability and the potential for cost reduction. Additional (exhaustive) tests should clarify the performance of the sensor in terms of threshold level, accuracy, hysteresis, and sensitivity.
- This reviewer recommends adding to the project an effort to work with a codes and standards organization to develop an industry-accepted guideline to qualifying a sensor for aging, selectivity, etc. The project needs to include a complete cost analysis with an industry partner.

## Project # SCS-005: R&D for Safety, Codes and Standards: Materials and Components Compatibility

Daniel Dedrick; Sandia National Laboratories

**Brief Summary of Project:**
The two objectives of this project are to: (1) enable technology deployment by providing science-based resources for standards and hydrogen ($H_2$) component development and (2) participate directly in formulating standards, including design and safety qualification standards for components and materials testing standards. A materials reference guide will be updated, reflecting the latest understanding of material property data gaps. Materials testing will be executed to address targeted data gaps in standards and critical technology development. More efficient and reliable materials test methods will be developed.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The work is focused on specific data needs to allow the development of science-based standards.
- This project has been one of the major references in the $H_2$ industry for $H_2$ compatibility worldwide.
- Clearly an important activity, this work goes to the very core of the Safety, Codes and Standards sub-program. This work is generating critically important information.
- Sandia National Laboratories (SNL) has extensive materials compatibility testing and expertise pertinent to influencing standards. Utilization of their expertise and continued support on this project will help to enable further standards development.
- The project's goals clearly relate directly to DOE program objectives. For the safe deployment of $H_2$ technologies, knowledge of the effect of $H_2$ on materials' mechanical properties (under static as well as cyclic loading conditions, and covering base materials and welds) is an absolute prerequisite.
- This is important work to confirm the safety of type 1 storage vessels already being deployed in material handling. Also, it is important to confirm light duty vehicle materials currently in use and open the pathway for approval of new materials in the future.
- As noted in the questions, the research and development effort needs to be expanded to reflect field applications and the temperatures and pressures anticipated by industry. Current vehicle applications will see temperatures in the -40°C to -70°C range, and the data should support the standards development efforts in that area. In addition, it is industry's desire to be able to specify a starting point for materials. If the research can be expanded to incorporate this, it would be helpful for industry in moving forward.

### Question 2: Approach to performing the work

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

- This project is using very good materials science techniques applied to highly relevant structures and samples.
- SNL is providing standards development organizations (SDOs) with empirical data as evidenced in slides 6 and 7.
- This team is actively participating in relevant committees, including serving in leadership roles.

- The methods are correct for $H_2$ embrittlement studies; however, pre-charged probes tested in air are not the extreme case encountered with probes tested in pure $H_2$. This should be recognized and communicated. Other work in Germany and Japan has shown these differences.
- This work has been effective in characterizing existing materials and establishing test protocols. Its ultimate goal should be a deep understanding of $H_2$ effects and predictive capabilities or guidance on measures to retard or prevent material degradation in a $H_2$ environment.
- Even though this reviewer is identifying an area that needs to be addressed to significantly overcome barriers, the project/effort and research has contributed significantly to the advancement of the safety codes and standards activities. As noted in the questions, the research and development effort needs to be expanded to reflect field applications and the temperatures and pressures anticipated by industry. Current vehicle applications will see temperatures in the -40°C to -70 °C range and the data should support the standards development efforts in that area.
- The approach that consists of identifying gaps in knowledge, establishing and validating representative test methods and methodologies, performing a limited amount of targeted testing, and ensuring adequate knowledge transfer to SDOs and code development organizations (CDOs) is fully correct. However, the exchange of views and cooperation with non-U.S. advanced materials testing experts (in addition to the Japanese institutions mentioned), should be further explored to increase efficiency of the work and disseminate its outcome.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Although this kind of activity is inherently expensive and time-consuming, steady progress is being made.
- This project has filled in key gaps in materials knowledge.
- This work measured the properties of $H_2$-exposed welds and aluminum alloy and optimized fatigue crack growth testing in ASME Article KD-10 tank standard.
- This project has provided a good evaluation of the current protocol. It is developing a procedure to accelerate the tests, with some success, and is working to incorporate scientific results in existing standards.
- The project has met all objectives and should be used as a baseline for the important understanding in the United States.
- This project has done a nice job developing a faster method to perform testing and gathering data and to get buy-in from the SDOs from the new methods. Also, the work has done a great job identifying progress on existing milestones/accomplishments.
- This research is providing the needed data to support the development of the standard to address material compatibility and is contributing significantly to fill a gap in information needed by industry. As recommended previously, the research work should be expanded to include the low temperatures that will be seen in the application.
- It is difficult to rate the degree of progress because two out of three "metrics for success" listed on the third slide are not directly applicable to the activities. Although during the oral presentation it was mentioned that the range of experimental conditions used in the fatigue testing is defined on the basis of industrial input, this does not seem to be sufficient from a materials science point of view. Indeed, important aspects that govern the component behavior (as opposed to pure material behavior), such as the presence of residual manufacturing stresses and of natural defects, do not seem to have been explicitly taken into account for their consequence on the parameter range that should be covered in the tests (e.g., other stress ratios, both positive and negative). The non-availability of experimental data under these additional conditions reduces the ability to assess and predict component behavior based on analysis of results obtained on test specimens. For some material/load combinations, this may result in non-conservative safety assessments.

### Question 4: Collaboration and coordination with other institutions

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

- This project is collaborating with relevant experts around the world.
- This project is truly international (including standards organizations, original equipment manufacturers, and research institutes) and should be used as a benchmark for future collaborations.

- This project has displayed excellent involvement, participation, and demonstrated leadership. Representatives meet commitments in a timely manner.
- This project should continue outreach to industry to understand the requirements of materials in service and to prioritize research.
- This project has a good list of collaborators, but it is unclear how much true “collaboration” has been going on. There should be more cross-referencing with testing performed at other sites.
- The presentation highlighted multiple collaborators. The level of collaboration on this effort with industry partners is unclear, that is, whether they are customers or test collaborators. Similarly, the participation from international collaborators is not explicitly identified in relation to what way they are contributing.
- Collaborations with relevant institutions and organizations within the United States seem purpose-oriented and sufficient. Reaching out to non-U.S. materials testing houses is recommended for two reasons: (1) to increase density of experimental data sets, and (2) to include other materials (in particular nickel-steels) that are used for $H_2$ applications elsewhere in the world.

### Question 5: Proposed future work

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

- Plans are well conceived.
- The work proposed seems sufficient, but some changes could be made to further assist the industry.
- This work should continue and accelerate; plans to do this seem to be in place.
- A large amount of work (important tasks) is identified as still ongoing. The work identified is well within the capabilities of SNL; few barriers are apparent.
- The mechanism of the effect of vol% on fracture toughness should be studied.
- This research is providing needed data to support development of the standard to address material compatibility and it is contributing significantly to fill a gap in information needed by industry. As recommended previously, this research work should be expanded to include the low temperatures that will be seen in applications.
- The topics identified for further work (fatigue crack initiation, welds) are correct and need consideration. However, no attention seems to be paid to other, non-metal material classes. This should be seriously considered.
- Safety in early applications, such as material handling, is important, but future work should focus on on-road transportation (buses and light duty vehicles) and fast-fill fueling infrastructure. Also, it is important to support $H_2$ delivery technical needs.

### Project strengths:

- This project has an excellent team with the required expertise.
- This project has a solid methodology, exceptional empirical results, and communication directly to those in the “need to know.”
- This project uses a good experimental technique and has a good choice of materials/structures to study.
- This program is filling a significant need in industry; this is the basis for helping industry determine what materials are used in applications in industry.
- Strengths of this project are the soundness of the approach, the diligence of execution of experimental work, and the direct link to SDOs and CDOs.
- This project has a great organization of the project’s objectives. It has clearly stated approaches, accomplishments, and future work (and discussion of work-arounds when problems were encountered). There are extensive $H_2$ material compatibility experts involved in this project.
- This project has unique capabilities and research results that are directly applicable to the design and safety assurance of commercial products. Direct participation in standards development work brings technical expertise to the table and has helped to move key documents forward.

### Project weaknesses:

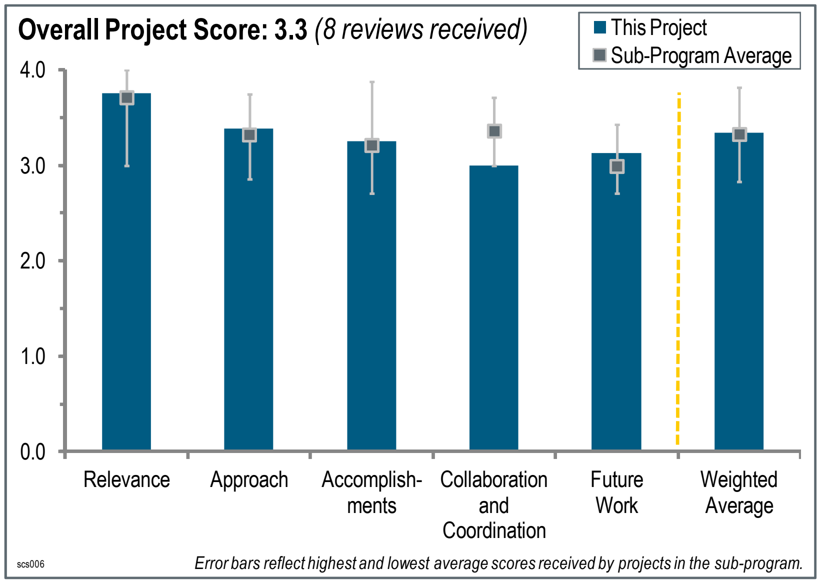
- Currently, the project's only weakness is that the program needs to address the temperatures that will be seen in applications and consistent with temperatures specified in current component and system locations.
- The project's weakness is using pre-charged $H_2$ probes versus. work done in pressurized gas.
- The project needs to continue to work toward international agreement on and acceptance of test methods.
- Weaknesses are the "U.S.-internal" orientation, the limitations of the experimentally covered ranges that do not really allow assessment of component behavior from the test specimen results, and the non-inclusion of other material classes.

### Recommendations for additions/deletions to project scope:

- As recommended previously, the research work should be expanded to include the low temperatures that will be seen in applications.
- Now that the team has well-established procedures and practices, and significant experience, the work should be ready to speed up.
- One reviewer recommends future testing at -40°C, where most $H_2$ embrittlement occurs, adding more materials to the test matrix, and testing materials in commonly used components such as manual valves (not just tank materials).
- Another reviewer recommends testing an industry-recommended (SAE J2579, etc.) list of stainless steel materials in environments up to "end of life" (that is, equivalent to 20,000 hours of use, etc.) and compare present results of pre-charged $H_2$ samples verus pressurized, cold (-50°C) testing.
- A third reviewer didn't have any deletions, only additions to address some weaknesses.

## Project # SCS-006: Hydrogen Safety Knowledge Tools

Linda Fassbender; Pacific Northwest National Laboratory




**Brief Summary of Project:**
The objective of this project is to establish and maintain two websites: www.H2incidents.org, for incident reporting and lessons learned, and www.H2bestpractices.org, for safety best practices. The objectives specific to H2incidents.org are to: (1) collect and share lessons learned from hydrogen ($H_2$) incidents and near-misses, with a goal of preventing similar safety events from occurring in the future; (2) increase the number of records in the database by encouraging "incident owners" to share lessons learned with the $H_2$ community; and (3) analyze and summarize lessons learned from incidents and near-misses. The objectives specific to www.H2bestpractices.org are to: (1) capture the vast and growing knowledge base of $H_2$ experience and make it publicly available and (2) update existing content and add relevant new content based on Hydrogen Safety Panel guidance and other means.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- This project is a key component of DOE objectives for $H_2$ implementation.
- This is a nice adjunct to the main thrust of the Safety, Codes and Standards (SCS) sub-program. This reviewer personally finds the websites interesting, and sometimes very useful.
- Communication of incidents and lessons learned is critical to developing a safe $H_2$ economy. This project has high relevance to the goals and objectives of the DOE Hydrogen and Fuel Cells Program (the Program).
- The project involves the creation of a safety repository for $H_2$ incidents and near-misses. The team will manage the repository based on input from stakeholders in the $H_2$ industry.
- This project corresponds to a cross-cutting activity that is critical to all projects supported by DOE. There is a clear need to centralize, in a database, the knowledge generated by all DOE projects with regard to safety; to make this database anonymous and open to anybody interested in the field; and to link this database with the other databases existing in the world.
- This project directly aligns with Barrier A from the Fuel Cell Technologies Program Multi-Year Research, Development, and Demonstration Plan (MYRDDP) with regard to promoting and promulgating safety data and information. Maintaining this resource is critical to breaking the "tribal knowledge" tradition of safety as this market emerges. It will be difficult to quantify how many lives and injuries will not occur due to this resource, but it is clear that it has a significant impact.
- The project has developed useful tools for disseminating experience and lessons learned from a number of persons and organizations working with or exposed to $H_2$ technology applications. As such, it is a valuable and necessary contribution that is assisting the safe deployment of $H_2$ technologies. However, the project does not address any research and development issues, and as such it has a "special" position in the Program.

## Question 2: Approach to performing the work

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

- The success of this project is evident in that there is good participation from industry to add to the knowledge base.
- The approach—establishing and publicizing easily used websites—is a good one. Migrating to "social media" should be in the mix.
- The ranking scale does not allow for an "A-" evaluation. This reviewer thinks that the approach of the project is outstanding but lacks a few things; this reviewer would give it a 3.7–3.8 on this scale. The presenter articulated efforts to promote or market the Internet tools, but the formal approach does not highlight this as a core part of the process. If there are barriers to the growth of the project or to evaluation of its effectiveness, those were not clearly identified by the presenter.
- There is a lot of data available from other organizations outside of DOE-funded projects. For the incidents that occur on DOE-funded activities, this project does a great job of harvesting, verifying, and posting the critical information. This reviewer recognizes that many difficulties arise when trying to gather incident data that occurs in the private sector or with the U.S. Department of Defense.
- The approach of establishing two separate websites is expected to be complemented by efforts to increasingly link them. It is unclear why the number of visits to both websites is so different. This may require some further evaluation. The population of the incidents database could definitely benefit from a DOE requirement that all DOE-funded projects should feed their findings and experiences into it.
- The approach to update the database is good. A clear and independent analysis of the reported incidents is needed before publication. This analysis allows for anonymity, which needs to be maintained. The problem is that the project is strongly dependent on the willingness of the organizations to participate and to report the incidents. Even if it is strongly suggested to the beneficiaries of DOE funding to participate in the reporting, should any problem occur, there is no such action for all the other projects that are privately supported. Following Devlin's presentation, when DOE is investing in one forklift project, there are five similar projects privately supported. Because security concerns everybody, a more pro-active approach of all concerned actors including insurance companies, authorization delivering officials, and firefighters' organizations could be recommended.

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project's accomplishments are consistent with the goals.
- Steadily increasing Internet traffic (except for the anomalous jump in 2008) indicates that the approach is working.
- The content of the website is improved, and the content of the database increased even with the reduced budget. The project features a great website and management of the information. Another accomplishment is providing the recommendations for best practices. The funding needs to be continued.
- The lessons learned corner is a nice addition. The team should consider adding the website (Safety Snapshot, at minimum) to a listserv for California emergency responders and permitting officials.
- The number of consultations is continuously increasing, which is a clear signal of the interest generated by the project. Many companies outside of the fuel cell and $H_2$ community are using (compressed) $H_2$-rich gases (petrochemical, chemical, and steel companies). Consultations of these companies on incidents do not seem to have been performed, but they could improve the project.
- Although there were 11 incident additions since the last DOE Hydrogen and Fuel Cells Program Annual Merit Review, the most significant accomplishment is the number of site visits. This indicates that the site is useful to groups in this industry. It would be nice to see a breakout of which country is accessing the site the most. This is a DOE-funded activity, and it would be nice to see U.S. industry utilizing this tool. It is still really good information for everybody globally, but perhaps more targeting could occur if it were determined that U.S. companies are not utilizing this information. Unfortunately the "Best Practices" section has not been updated due to funding issues.

- As mentioned in the oral presentation, reduced funding has resulted in a backlog of inputs to both websites. This trend should be reversed. Information should be provided on the time and effort required for "vetting" an entry into the incidents database and identifying which specific problems arise, if any, once an incident has been provided for input. Also, information should be provided on the number of and justification for submitted entries that eventually are not included. The addition of the "Safety Snapshot" feature to one of the websites is positive.

### Question 4: Collaboration and coordination with other institutions

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

- The project features good collaboration with the Hydrogen Incident and Accident Database (HIAD) and other laboratories.
- There is a very good connection into other organizations to gather incident data. There appears to be international collaboration as well.
- The team should increase efforts to investigate how collaboration and cross-referencing with the IA-HySafe HIAD could be enhanced.
- The collaboration with other peers is good. It still seems like the project needs to be more widely disseminated. It is a great resource that is unknown. Perhaps the use of social media or linking to other websites to get the word out about the best practices section, for example, would make a useful addition. However, there needs to be consideration as to what the general public readily sees (to avoid over-dramatization).
- Collaboration with the HIAD database is mentioned, but during the presentation it was not clearly explained how this collaboration took place and if there is now a uniform procedure for recording and introducing new events. Moreover, there are at least three projects led by the Pacific Northwest National Laboratory (PNNL) (projects SCS006, SCS008, and SCS015) that deal with safety at different moments of a project's life. There is a clear need to identify precisely the goals of each project, to show that there is no overlap between the different tasks performed in these projects, and to show how each project may benefit from the experience gained in the other two projects.
- The relationship with HySafe is certainly a key contact, but it is unclear what new collaborations were established. It was also unclear how this project reaches beyond the $H_2$ safety and national laboratory community to achieve the goals specific to the barriers. There is no dispute that this database is a marquee for pro-active, behavior-based safety; this reviewer wants to know why the U.S. Chemical Safety and Hazard Investigation Board (CSB) compliment has not been further leveraged and made visible to the broader safety community. This reviewer wonders if there is any other industry that has such an extensive database. If not, this reviewer wants to know why this is the case and what insights other industries/communities could gain from this resource. This reviewer wonders how much effort or activity has been put into investigating this interest and the potential to leverage collaboration or other safety incident databases.

### Question 5: Proposed future work

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

- The team should further expand on plans to offer value as a communication document.
- The suggestions from last year's review are good ones—a brainstorming session could yield more good ideas for promoting the site.
- The team has identified ways to overcome a number of comments that have been made in previous reviews. These should be implemented, provided that sufficient funding is made available.
- An insurance company may have accident data. This reviewer suggests the principal investigator contact such a company.
- The plans are built on excellent past progress, but a "routine" seems to be put in place without a clear strategy on how to encourage the private-sector to submit records of incidents. This strategy may not be in the hands of the persons responsible for the project, but perhaps DOE could "strongly encourage" such a report.
- The future work plans are good, but they do not "excite" this reviewer with how this project would grow or needs to grow. It was unclear if the project administration should concentrate on making the process more efficient because some people do not want to grow the project. Four items in the list of proposed future work

suggest continuing the activities of the project, and three items suggest solicitation for improvement (gap analysis, survey, and brainstorm). This reviewer wants to know what the project leader wants to do. This reviewer also wonders, with all of this great work, what the next stage is in the strategy of the project. A few suggestions are provided in this review, but the project leader should articulate where this project is going and what the project's long-term aspirations are (e.g., increase efficiency by lowering costs to posting incidents, website overhaul, collaborations with various organizations, cross-reference with various safety databases).

### Project strengths:

- This project should be continued—it is valuable in many ways, from the public relations benefits to the substantive support of the project's stakeholders.
- This is great information that is used by many. The site is getting lots of visitors, which indicates that people are seeking this type of information.
- It is important work to provide a resource for authorities having jurisdiction (AHJs) and the like as commercialization progresses. The international exposure is excellent.
- Safety concerns everybody and is absolutely necessary for public acceptance. It must be obvious to the public sector that reporting incidents in a standard, normalized, and anonymous format is for the good of the society as a whole if one wants to change the initial opinion of the population when $H_2$ is mentioned (e.g., a reference to the Hindenburg accident or the nuclear bomb). PNNL has developed a strong and consistent approach with the three other projects to introduce safety concerns at each step of a project dealing with $H_2$ and fuel cells. This must be maintained and reinforced.
- The ongoing work to keep the database relevant and trustworthy is the key benefit of this project, and the growth of recognition and use is a testament to that. The recognition by the CSB is a tremendous compliment and shows the impact of this database on the broader safety professional community.

### Project weaknesses:

- The value that this provides is communication.
- The team needs to reach out beyond the audience of the "usual suspects."
- The team could consider the approach of other industries that have been dealing with $H_2$ for many years: this could bring added value. New approaches to obtain more data must be encouraged.
- The project staff seems to be "resting on its laurels" a bit. The websites are good, but improvements in visibility and content could be made.
- This reviewer recommends identifying which countries are visiting the site and developing a strategy to increase the number of visits from countries with developing $H_2$ economies. Another weakness, sadly, is the lack of funding given to this project.
- The website tools provided by the project do not seem to be exploited to their full potential. To achieve this, continued and possibly increased financial support is needed, and the awareness of the availability of the tools and of their potential should be increased. For the latter, an increased number of hyperlinks from other public websites relevant for $H_2$ technologies should be investigated, as well as possible usage of social media.
- The project seems resistant to implementing new ideas and concepts based on responses to previous reviewers. The project seems to lack a direction to grow; the proposed future work includes a gap analysis by an existing and closely linked collaborator (rather than a broader request for analysis, perhaps from a professional safety organization). The proposed future work also includes "brainstorm." The compliment by the CSB is undercut by the fact that the database's existence is not widely known, and the project seems to lack a cohesive strategy to address this. Based on barriers identified in the MYRDDP, it is not clear how the project addresses treating safety as a continuous process. This is left to the reviewer to infer. Based on the barriers identified in the MYRDDP and the comments above, this reviewer wants to know how the project plans to use this fantastic database to address the lack of $H_2$ knowledge by AHJs.

### Recommendations for additions/deletions to project scope:

- A stakeholder survey to obtain feedback on the utility of the websites is clearly a very valuable initiative that could bring a lot of side effects, especially if this survey contacts persons who do not report incidents.

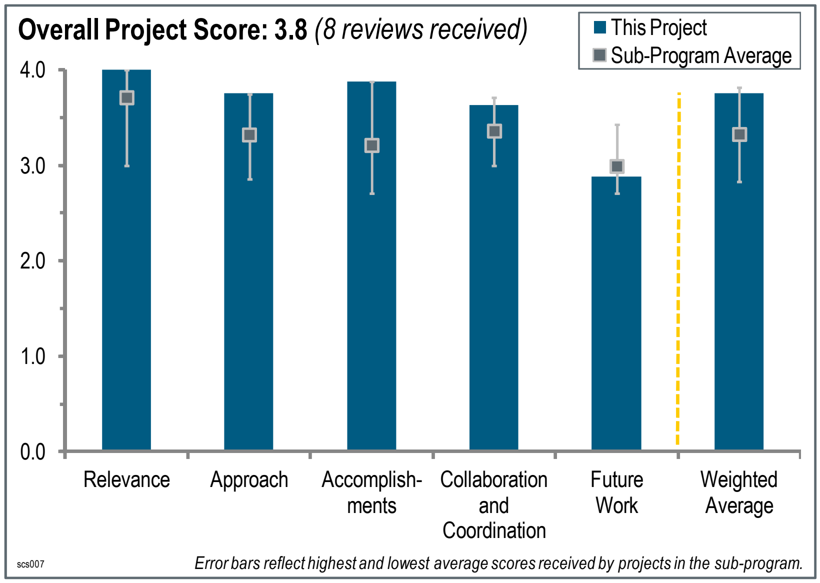
- The team needs to consider this as a reference site for AHJs to reference. This should be referenced in any and all templates and tools used in the commissioning of the stations, as well as with the operation of vehicles and fleet managers.
- Funding should be maintained to continue, build, and improve upon the project. This resource is valuable to point AHJs to—even if they do not use it, they know it is there and it provides some level of assurance and comfort that someone is tracking and watching the industry. It adds a level of transparency that is critical to infrastructure deployment.
- The team should expand collaborations beyond national laboratories and HySafe. It could develop a marketing strategy to promote the site through other Safety, Codes and Standards sub-program elements and contacts. The team could also reach out to the professional safety community or submit articles to the journal *Professional Safety*. One reviewer wants to know how the insurance community values this resource.
- Whatever was done in 2008 to increase website visits should be tried again. Adding more sophisticated "tracking tools" would help the team understand the customer database and its interests, and it also might provide DOE with insight into newly developing safety trends or concerns of the $H_2$ community.
- Another reviewer suggested the addition of compressed natural gas vehicle incidents due to their similarities with $H_2$ issues. This reviewer recommends identifying which countries are visiting the site and developing a strategy to increase the number of visits from countries with developing $H_2$ economies.

## Project # SCS-007: Hydrogen Fuel Quality

Tommy Rockward; Los Alamos National Laboratory


### Brief Summary of Project:

The objectives of this project are to: (1) determine the allowable levels of hydrogen ($H_2$) fuel contaminants in support of the development of science-based international standards for $H_2$ fuel quality (International Organization for Standardization [ISO] TC197 WG-12) and (2) validate the American Society for Testing and Materials (ASTM) test method for determining low levels of non-$H_2$ constituents. Los Alamos National Laboratory (LANL) will apply expertise in ultra-low impurity measurement and analysis capabilities for single-cell testing to the development of a science-based international standard for $H_2$ fuel quality.


### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **4.0** for its relevance to DOE objectives.

- Fuel contaminant testing is necessary for commercialization.
- This project is essential work that serves as a basis for establishing commercial fuel-quality requirements.
- This data was used directly for standardization (Society of Automotive Engineers [SAE] J2719), and is a basis for validation that the levels in the standard are proper to avoid significant degradation of fuel cell performance.
- Establishing universally accepted, science-based fuel quality standards is absolutely essential to the future acceptance and widespread use of $H_2$ as a fuel.
- Developing and publishing fuel quality standards are essential to the successful commercial roll out of fuel cell vehicles. Understanding the effects of constituents in $H_2$ fuel and validating sampling and analytical methodologies is likewise critical.
- This project developed a tolerance in the fuel quality testing to determine the maximum contamination allowed before mitigation strategies need to be included. This will directly be able to establish fuel quality specifications and standards and supports the SAE 2719 specification levels.
- This is very nice work and critically important to understanding the effects of fuel impurities on fuel cell performance. This type of work might be argued as being a bit premature only in that the fuel cell technology is still under development. Presumably, the tolerance of the stack to impurities will change with development improvements of the stack. With that being said, this work is relevant to the understanding of impurities as the stack develops. The researchers have done nice work.
- The project has played a critical role by conducting single-cell tests to help determine the effects of low levels of contaminants, especially CO, $H_2S$, $NH_3$, and their mixture, on polymer electrolyte membrane (PEM) fuel cell performance in road vehicle applications. The project has also provided significant data and understanding of how such contaminants affect performance. The validation of ASTM standards via inter-laboratory studies is critical for the application and verification of the ISO and SAE fuel quality specifications.

### Question 2: Approach to performing the work

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

- This project seems to have a very good approach, given the limitations of available test materials at this time.
- This project has a well-equipped laboratory, highly experienced personnel, well-planned experiments, and a winning approach.
- The analytical approach with round robin testing, etc. is very clear and has been accepted by the entire industry.
- Conducting measurements at the levels necessary to understand tolerance levels is a very challenging task. The researchers have done some nice work.
- LANL continues to have knowledge and capabilities far beyond most laboratories. These projects are well designed and the results are extremely valuable.
- While single cell testing is important, greater relevance can be had by expanding to stack testing. This issue will get more relevant as commercialization gets closer to reality.
- This project is focused on testing three contaminant families (CO, $H_2S$, and $NH_3$), and on testing at varied temperatures, relative humidity levels, and the concentration of contaminant. This project developed a validation of the FTIR contamination measurement for $H_2O$ and $NH_3$.
- This project conducted state-of-the-art testing and diagnostics on fuel quality to support the development of ISO and SAE standards deemed essential for the commercial deployment of fuel cell electric vehicles (FCEVs). The approach to testing and focusing testing at catalyst loading levels that meet DOE targets help the project to address critical barriers. The project is well integrated with both fuel cell research and development, and codes and standards development.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.9** for its accomplishments and progress.

- ASTM FTIR testing on ammonia and water was completed.
- This work has been very carefully executed.
- $H_2$ quality standards are in place for first generation(s) of commercial products.
- Studies have focused on the industry-identified primary contaminants, which is good. Participation in the ASTM FTIR validation effort is also a positive accomplishment.
- The accomplishments work towards furthering the durability targets (5,000 hours) and enables understanding of other degradation factors, such as carbon corrosion and catalyst degradation.
- Steady progress is being made in this project. The $H_2S$ test results are particularly interesting and using FTIR to verify concentrations is a good idea. The mass flow controller-driven mixing can sometimes provide misleading results.
- Understanding of the CO effect on the membrane electrode assembly (MEA) is critical to DOE's efforts, including the canary species for steam methane reforming (SMR). $H_2S$ and ammonia work is similarly critical as its effect on the fuel cell is critical. This project has done excellent work supporting the FTIR method and produced valuable data, which will be integrated into the ASTM standard.
- This project made excellent progress toward a systematic understanding of the effects of CO, $H_2S$, and $NH_3$ on single-cell PEM fuel cell performance and durability. The accomplishments of the project were a significant factor in establishing technical consensus on the levels of these contaminants that are tolerable and that can be defended in a standard. The project also made an important contribution toward the validation of an ASTM standard needed to help verify compliance with the standard.

### Question 4: Collaboration and coordination with other institutions

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

- LANL has support from industry and government, and has kept on target with timelines and deliverables.
- Industry, academia, and government have been working well together on $H_2$ quality for several years. That cooperation continues.

- This project has been collaborating with the Fuel Cell and Hydrogen Energy Association (FCHEA), numerous universities, three DOE technology teams, and directly with the standardization efforts.
- This project is well linked with a number of relevant organizations. The round-robin testing as implemented in this project is absolutely essential to wide acceptance.
- It is unclear if the $H_2$ suppliers have been fully engaged and if they provided input in the costs of purifying hydrogen. This reviewer wondered about system studies that would inform the tradeoff between projected fuel cell stack degradation and fuel cost.
- This is an area that could be strengthened. While working with WG12 is important, the principal investigator (PI) could benefit from others in the field by engaging in a "technical" collaboration. This work should seek out technical collaborations to accelerate, leverage, and share results. The PI should stay connected with the activities of the International Partnership for the Hydrogen and Fuel Cell Economy’s Regulations, Codes and Standards Working Group (IPHE/RCSWG) as they organize a round robin in this very area.
- The PI is internationally recognized as a leading contributor to the development of both the ISO and SAE standards. The project has been an integral part of the DOE effort and the PI is a critical member of the DOE team of experts. LANL and the PI have worked very effectively with other DOE team members and with the international experts who worked on the standard.

### Question 5: Proposed future work

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

- The proposed program is good, but stack-impact studies on these same contaminants should be added.
- This project should do additional testing of other species critical to the fuel cell performance.
- The tie into system degradation mechanisms and cathode side degradation has not been made clear.
- Continuing on this very productive path is a good idea. This reviewer wondered if the team will turn to studying other impurities.
- LANL's work to support future ASTM Interlaboratory Study Program (ILS) efforts is critical for validation of ASTM test methods. It would be beneficial to the industry for LANL to have a more active role in ASTM.
- More detail is needed on how testing conditions will be varied to provide more data (slide 20). Test data that can illuminate recovery mechanisms under PEM fuel cell systems and FCEV operating conditions would be especially valuable. The close collaboration with the Fuel Cell Technologies Team to address the effects of shutdown/start-up procedures and operating strategies would be valuable as well.
- Looking at combinations of impurities seems like the best next step. It may also be possible now to test newer materials or to test at the stack level, since fuel cells are moving into commercial applications. It might also be useful to understand what contaminants are in dispensed $H_2$ and test their combined effect. There should be some field work that could guide the direction of future lab work.
- The PI recognized the importance of combining contaminants to understand the interplay. This is a very appropriate and timely direction for this work. The PI should embrace the notion of contaminants coming from a “system” installation (i.e., particulates, contaminants from improperly cleaned delivery systems, etc.). The researchers should also pay attention to the response of the fuel cell and the influence of impurities under operating conditions typical of what might be seen for steady state operation of stationary applications, such as CHP, in addition to the periodic operation of the transportation applications. The stationary application is out of the scope for WG12, but it is very relevant to the fuel cell development community and fuel quality specifications for the stationary applications.

### Project strengths:

- The expertise at LANL on impurities remains superior.
- This project has knowledgeable researchers and clear directions from industry.
- This project uses very good science, both in planning and execution. This project is highly relevant to DOE’s $H_2$ mission.
- This project has a clear methodology, an industry condensed plan, clear results, and communication with the industry.
- This project followed the disciplines of the ASTM testing regimes: good correlation with results and theory.

- This project has a world-class team of scientists and engineers that can be called upon as needed, state-of-the-art laboratories, and an established history of collaboration with industry.
- This project has a strong analytical approach and good tools have been developed. This project is directly relevant to the commercial use of $H_2$ as a fuel for fuel cells.
- This project is well thought-out with carefully executed experiments. It is yielding very valuable data on the performance of the fuel cell in response to various containments in the fuel stream. This work is well positioned to stay up-to-date with the changes in fuel cell technologies to make sure the fuel quality standards do not adversely hinder deployment, which is critically important.

### Project weaknesses:

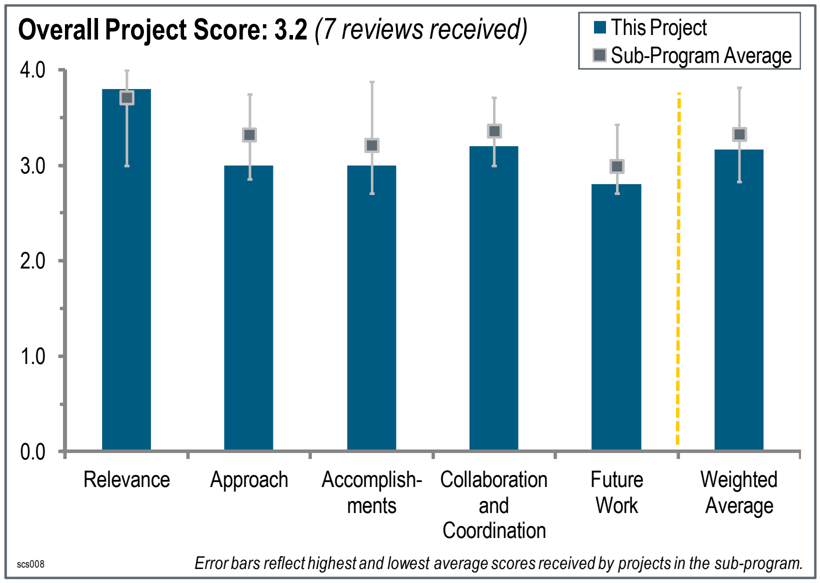
- Budget constraints limit the evolution of stack testing.
- The connection between system degradation mechanisms and cathode side degradation is not clear.
- The lack of access to commercial dispensed $H_2$ and commercial fuel cell stacks is a problem that should be resolved over the next 3–5 years.
- This project should have focused more on the bottom line. It was unclear what the current standards for fuel quality are and how this work supports, extends, or overrides those standards.
- This project needs a stronger technical collaboration element. While working with WG12 helps to keep this work relevant to the standards development community, a stronger technical interaction will help to ensure the data is of high quality (cross checking), and that the program is leveraging the efforts of other domestic and international facilities.

### Recommendations for additions/deletions to project scope:

- This project should continue with system testing (short stack system) efforts up to 5,000 hours.
- These researchers and facilities should continue investigating other impurities.
- This project needs more effort and allocation of resources to lead the preparation for testing the review paper (slides 19 and 20).
- There should be further identification of the variables to be addressed as research is moving from single-cell to multi-cell fuel cells, as well as changes in the platinum loading to meet.
- A slide listing all of the current best guesses at maximum allowable impurity levels and the assumptions that led to them would be very useful.
- The work needs to include competing effects with other contaminants (this is being planned, which is good). The work also needs to include the class of contaminants that one might find in a system environment (the balance of the fueling station from storage to delivery), and the work should increase its technical collaboration with other capabilities globally. The IPHE/RCSWG will be a possible vehicle to enable this collaboration.

## Project # SCS-008: Hydrogen Safety Panel

Steven Weiner; Pacific Northwest National Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) provide expertise and recommendations to the U.S. Department of Energy (DOE) and assist with identifying safety-related technical data gaps, best practices, and lessons learned; and (2) help DOE integrate safety planning into funded projects to ensure that all projects address and incorporate hydrogen ($H_2$) and related safety practices. Pacific Northwest National Laboratory (PNNL) conducts safety planning activities and safety evaluation site visits to accomplish these objectives.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.8** for its relevance to DOE objectives.

- As a decision on commercialization approaches, this work is relevant if their recommendations are heeded.
- This project is critical for the objectives of safety data and information, but it is insufficient for technical data.
- External peer review of safety plans for DOE-funded projects is very relevant to the deployment of $H_2$ technologies, as safety is first.
- This work is directly relevant to the DOE Hydrogen and Fuel Cells Program (the Program) goals; this activity is critical to the mission of the Safety, Codes and Standards sub-program.
- In order to increase public acceptance of $H_2$ and fuel cell technologies, it is very important to be sure that all new deployment, demonstration, and research projects using these technologies are safe or at least have a safety plan that will minimize the consequences of any encountered problem. This “ex ante” analysis performed by the Hydrogen Safety Panel (the Panel) plays a critical role for the success of the Program.
- The work of the Panel is critically important to the success not only of the Safety, Codes and Standards sub-program but of the Program overall and is a major component of the Fuel Cell Technologies Program Multi-Year Program Plan. The Panel embodies the need to make safety management a priority in every key aspect of the Program.
- The objectives of this project are to provide expertise and recommendations to DOE and assist with identifying safety-related technical data gaps, best practices, and lessons learned, and help DOE integrate safety planning into funded projects to ensure that all projects address and incorporate $H_2$ and related safety practices. This work is valuable in reviewing existing safety plans and providing feedback; however, this reviewer doesn't see any direct correlation between the activities in the project and the advice that DOE provided. There was obviously work performed and reviews conducted; however, there is no reflection of whether the recommendations were implemented or how the feedback was used by DOE.

### Question 2: Approach to performing the work

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

- The approach is straightforward: provide safety plan guidance, review, site visit or phone interview, and follow-up.
- The approach of site visits and plan reviews has been effective where applied.

- This is a well-focused project: taking a "big tent" approach (getting members from many segments of the industry involved) from the start was a good idea. A possible improvement might have been to put in place formal "bylaws" that describe governance, membership requirements, and maybe even "terms of office."
- The composition of the Panel and the advisory role is critical to the elevation of critical issues. Integration of safety planning into projects is valuable, along with "continuous and priority attention" and the composition of the team.
- The project approach, as noted in slide 7, seems thorough and comprehensive. The application or end result is unclear and this information would be beneficial to understand the effectiveness of the program. Slide 10 does indicate that 90% of the recommendations are implemented voluntarily, which is great. However, this reviewer wants to know about the other 10%. This reviewer questions what the implications are of the recommendations that are not made and how these will be followed up. This reviewer also questions what the impact is on the overall Program.
- The approach, based on historical experience with continuous increases of knowledge and insights gained, is good and will clearly help to overcome barriers. A more systematic use of the Panel is recommended not only for DOE-funded projects but for all projects implementing $H_2$ and fuel cell technologies. Contacts with the insurance companies that will insure new installations (who could possibly offer a financial incentive like a reduction of the insurance costs if the safety plan is analyzed by the Panel and recommendations implemented) could benefit the project via an increased number of safety plans to analyze. The project could also benefit from a more regular follow-up of the analyzed safety plans, especially in the case of incidents.
- The project's approach improves each year and is becoming more comprehensive and strategic. The project still does not have an operating plan that integrates responding to current needs and identifying and incorporating potential emerging safety issues. For example, the "lessons learned" from site visits and safety evaluations provide valuable information about deployment (slide 9), and the Panel could apply its own "lessons learned" as a panel of experts and how its expertise and experience have been applied and could be applied more effectively. How an "integrated approach to project safety planning" (slide 9) can be achieved could have been explored in the presentation, perhaps under future work (slide 17) as could scenario and safety vulnerability analyses for key anticipated deployment pathways.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project has increased general awareness of safety issues with $H_2$, which is good for the industry.
- This work is fairly straightforward. Its accomplishments are listed as activity counts, and activities implemented or in progress. The scale appears to be proportionate to the funding level.
- The Panel clearly has been accepted by the community, and provides a valuable service.
- The program is good and definitely is an area needed by industry. It is time to determine if this is an area that will be required for industry on a continuing basis. If the answer is "yes," then it may be time to begin to transition at least portions of this work to the private sector.
- This project is an active and appropriate response to the need for more activity through an integrated approach to safety planning of early market fuel cell deployment. The Panel has been evaluating the deployments with a risk perspective, meaning that scenarios are considered. Regarding outcomes, 90% of recommendations are in progress or completed as well as five safety evaluations.
- The thorough and integrated approach is a significant progress area. The scorecard shows clearly that most of the recommendations proposed by the Panel are implemented. This highlights the value of the work performed by the Panel. There is nevertheless a too large difference between the number of safety reviews conducted and the number of follow-up interviews. Publication of standard good examples of safety plans for different applications and types of projects (i.e., laboratory experiments and demonstration or deployment projects) would benefit the project.
- There has been improvement on defining performance indicators and measuring progress against them, but they are still not completely convincing—there are overlapping categories, and it is difficult to evaluate the effect or impact of work on safety of individual projects or $H_2$ deployment in general. The strategic examination of its work (slide 13) is a good step, but the "new initiative ideas" should have been evaluated and expanded upon; for example, it was unclear if there were any consequences or changes emerging from the strategic examination.

Collaborative work with the University of California Los Angeles (UCLA) is a good example of the value of the Panel.

### Question 4: Collaboration and coordination with other institutions

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

- This project has very good collaboration from industry and international associations.
- This project has excellent collaboration. It seems the interest in the programs is growing and it seems logical this will increase as we approach the roll out of more vehicles and increase in infrastructure.
- The collaboration of this project is good. Collaboration is on a national and international level through appropriate conferences, working groups, national laboratories, and industry.
- This project has good interfaces with “customers” and maintains some international presence. It could be better hooked into the codes/standards community.
- This project displays good coordination with stakeholders through engaging industry in the Panel’s activities. It has a good mix of laboratories, fire officials, and industry. The UCLA workshop on safety planning demonstrates the value of the Program.
- The Panel has worked extremely well with industry, universities, and DOE national laboratories. The Panel could explore potential collaboration (and learning new ideas and activities) from other federal safety panels and perhaps also with safety agencies or boards in states such as California and New York, which are leading the deployment of $H_2$ fuel cell technologies.
- There are at least three projects led by PNNL (SCS006, SCS008, and SCS015) that are dealing with safety at different moments of a project life. There is a clear need to identify precisely the goals and targets of each project, to show that there is no overlap between the different tasks performed in these projects and to show how each project may benefit from the experience gained in the other two projects. Cross-fertilization is important, but duplication of tasks must be avoided (for example, the incident investigations appear to be more the responsibility of project SCS006 than of this project, even if it is reported here). The collaboration with other institutions could be better explained and presented notably at the international level.

### Question 5: Proposed future work

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

- The future work follows from past accomplishments.
- The future work described for fiscal year 2013 is very general and could be more specific and informative (see comments under “Approach”).
- Proposed future work was described well, a few specifics were identified, and the remainder is to continue conducting site visits and making recommendations, which sounds good. This program wants to get involved sooner in projects to improve safety. Future work includes more comprehensive data integration from safety learning and experience, which is critical to the risk activities. Future work includes a safety checklist.
- The project will continue reviewing safety plans and conducting site visits. It looks to be in a "routine" mode without any clear plan on how to enlarge the scope of activities and how to face the increasing number of projects (funded or not by the DOE) dealing with $H_2$ and fuel cell technologies.
- The ongoing services of the Panel, particularly site visits and Safety Plan evaluations, should be continued. This reviewer agrees that the function is too important to be left to commercial stakeholders. DOE support for the Panel gives it the independence it needs in order to do an unbiased job. It is a good idea to link more closely with the standards-generating organizations, particularly as the data collected by the Panel continues to grow.

### Project strengths:

- The project’s strength is the composition of the Panel.
- This project has an excellent panel of experts and support management.
- This project has an excellent working relationship between both team members and industry.
- This project provides the education, training, and safety review needed by industry. This also promotes awareness, understanding, and acceptance by the public.

- This project displays really excellent interface with customers. It is definitely providing a “go-to” resource to the community.
- The methodology and the experience gained are clear project strengths. The expertise acquired by the Panel is extremely valuable and the high rate of success for the implementation of its recommendations illustrates the good contacts with the different stakeholders.
- The Panel essentially provides a peer review of safety plans and practices. Utilizing a broad-based panel representing expertise from industry, national laboratories, vehicle original equipment manufacturer, and fuel suppliers to review safety plans, make recommendations, and conduct site visits is essential to ensure that the demonstration projects all benefit from the peer review of safety plans and site visits inspections.

### Project weaknesses:

- Committee membership seems a bit static—only one position turned over between 2011 and 2012.
- There is not enough detailed documentation of the results of Panel actions and recommendations.
- The project’s weakness includes the challenge of getting stakeholder buy-in. It is critical that safety planning and the Panel be integral to the future DOE-supported deployments.
- As the number of $H_2$ and fuel cell technologies implementations is constantly increasing, there is a clear need to increase the visibility of the Panel and, if possible, to make “mandatory” a contact with the Panel. The project needs to develop a strategy to be able to face an increasing number of demands.
- While this is not a project weakness, one reviewer pointed out that only stats (activity counts) are presented for the entire project and for the current year, and this reviewer would like to see a slide discussing the safety reviews and white papers that will be generated for the current year, like the list on slide 21.
- It is not mandatory that sites follow recommendations. There are many reasons for this, most of them economic. Until full commercialization happens, the safety team will only see about 90% incorporation. Another reviewer questions if there are statistics that show how many installations/facilities declined a visit from the Panel.

### Recommendations for additions/deletions to project scope:

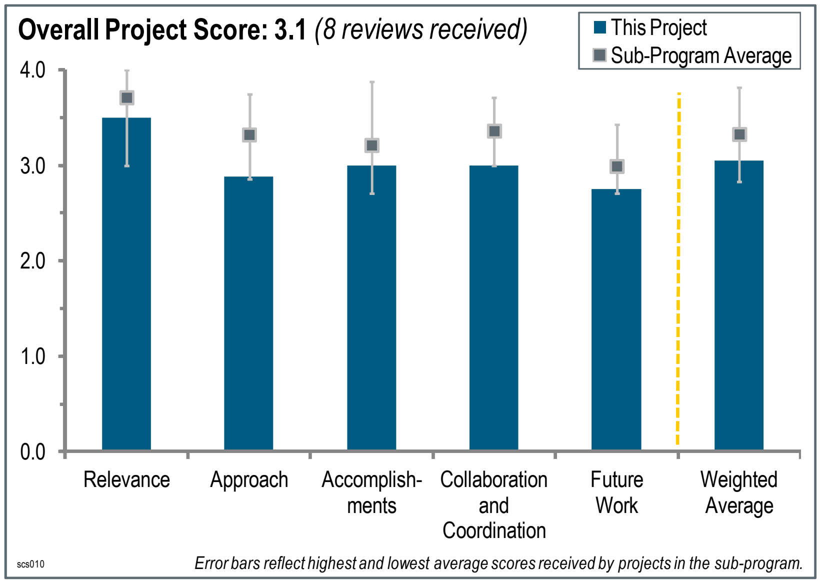
- Follow-up visits are good. Bringing these safety findings to the first responders table would be very good and could be added to a training program.
- There may be overlap with National Renewable Energy Laboratory (NREL) data collection. It is agreed that better data collection is needed, but it is best to coordinate directly with NREL.
- To improve the project, one reviewer recommends reinforcing the feedback loop after analysis of a safety plan and preparing “model” safety plans that are published on the web.
- Another reviewer questions if the project could tie in some information on how often sites are contacted, how many there are, what is the safety review cycle for each participant, and if new projects are coming on board each year. This reviewer also questions if some projects have expired and what the areas of focus are (such as how many are vehicle related, research related, authorities having jurisdiction [AHJs], demonstrations, etc.).
- A third reviewer recommends adding some “new blood” to the committee. Experience is golden, and certainly turnover should be relatively slow, but membership shouldn't be a sinecure.
- An annual report on “the state of hydrogen safety in the [United States],” and (in a more general sense) internationally, would be informative and would help cement the value and role of the Panel. The Panel could explore with NREL how business-sensitive safety data (slide 11) can be archived and shared in an aggregated form as was done in the Technology Validation and Learning Demonstration project.

## Project # SCS-010: R&D for Safety, Codes and Standards: Hydrogen Behavior

Daniel Dedrick; Sandia National Laboratories

### Brief Summary of Project:

The objectives for this project are to: (1) develop a science basis for hydrogen ($H_2$) safety, codes and standards (SCS) and (2) harmonize $H_2$ SCS development. Activities in the past year included experimental examination of fast-fill $H_2$ fueling of Type IV tanks in support of Society of Automotive Engineers (SAE) J2601, the Global Technical Regulation, and other regulations, codes, and standards; examination of the characteristics of predictive choked flow dispersion models; qualitative high-speed ignition imaging; and measurement of radiative heat fluxes from large-scale $H_2$ flames.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This work is critical to the success of $H_2$ commercialization efforts.
- Most of the work seems relevant to DOE's $H_2$ Safety, Codes and Standards sub-program goals.
- The project addresses issues that are critical to the safe deployment of $H_2$ technologies.
- The data being generated as a result of all the testing and modeling has direct applicability to DOE's objectives.
- The work being done on risk evaluation, $H_2$ release behavior, flame radiation, and the collaboration on $H_2$ safety are very relevant. However, the fast fueling work is so irrelevant that the results (average) can only be fair.
- The goal is to develop a science basis for $H_2$ SCS. Therefore, a long term program to support data-driven SCS though experiments, modeling, and validation is necessary ($H_2$ behavior during release, ignition, and radiation). Additionally, research support for the fast-fill protocol is a specific requirement for vehicle deployment.
- The project is critical to achieving program objectives, along with other Sandia National Laboratories (SNL) projects, and is a major component of the Fuel Cell Technologies Program Multi-Year Research, Development, and Demonstration Plan (MYRDDP). For example, fast-fill modeling and validation are essential to establish performance-based requirements for SAE J2601 that, in turn, is critical for the widespread deployment of $H_2$ fueling stations. The same case for relevance to DOE's objectives can be made for SNL's other research and development (R&D) activities covered in the presentation.

### Question 2: Approach to performing the work

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

- What was presented was a collection of individual science experiments, not a coherent research project.
- The approach is the right one. However, a concern is that the researchers are creating science projects with no end.
- The project addresses relevant issues in terms of pre-normative research and certainly contributes to closing some of the crucial knowledge/understanding/modeling gaps.
- The approach on slide 9, discussing risk reduction, is useful in understanding the gaps that are being addressed in the research. It shows how it may be integrated with other aspects of $H_2$ behavior, though it does not show for the other topics what has or has not been addressed so far under the program.

- The approach is good, but the presentation needed better correlation between project objectives and data being generated. For example, slide 5 outlines the project milestones and objectives, but the subsequent slides present topics that are not found on slide 5.
- R&D activities included in the project are well-designed and provide important data and information in support of the codes and standards development process. The presentation should show more clearly how the R&D activities are integrated. They are described as discrete activities, and one has to infer the context in which these activities are related and contribute to a better understanding of $H_2$ behavior. The entire SCS session (as well as the DOE Hydrogen and Fuel Cells Program Annual Merit Review [AMR] plenary and sub-program overviews) could be better integrated as well.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Several results were presented and many of them may have been good, but it was hard to tell.
- A lot has been accomplished in studies to assess $H_2$ leak and ignition behavior over the course of the project.
- The accomplishments of the risk evaluation and flame radiation work are outstanding. The $H_2$ release behavior and collaboration on $H_2$ safety is good. However, the work being done on fast fueling is so poor the progress (average) can only be fair.
- There has obviously been lots of progress on various tests and model validations. The relevancy of this work to affecting codes and standards is a bit difficult to discern from the presentation. The reader has to make certain assumptions and it is left to the reader to determine how the data fits the Safety, Codes and Standards sub-program.
- The risk-informed approach developed through this program is indeed a useful tool for both national and international code sets. The fast-fill modeling will be useful in evaluating the performance of refueling processes for verification of J2601. The other $H_2$ behavior work is interesting.
- The individual R&D activities presented have provided good results. However, a more integrated presentation would better show progress toward DOE goals. Success metrics are identified in slide three, but there is no measurement against these metrics for the individual activities described. The project milestones (slide 5) mention SAE J2601 under $H_2$ behavior/fast-fill experiments, but there are no relationships to codes and standards development in the other two activities under $H_2$ behavior. For fast-fill modeling validation, the maximum pressure is only 2,000 psi, far short of the 5,000/10,000 psi typical/planned for commercial stations. “Standards advocacy” in slide five should be replaced by “participate in codes and standards development process to ensure….”
- Good accomplishments were shown in the presentation. However, it is not easy to clearly identify from the presentation the level of progress related to last year and to what extent 2012 progress contributes to the achievement of the final target of the research in each field of investigation. It is not clear from the slides which topics have been already closed or are near to being closed. The percent complete (80%) in a very long timeline (2003–2015) is too generic for the AMR. Values for the metrics for success identified in the third slide are not given. In terms of fast filling, the experimental facility has the interesting capability of measuring the temperature inside the tank material. However, data is limited to 2,000 psi maximum; going up to 10,000 psi and finalizing the task within 2012 is definitively challenging. The very interesting work on turbulent jets should be supported by experiments at higher pressure than 1,000 psi. It is not clear from the presentation for which specific investigations there was an input/contribution to regulations, codes, and standards (RCS).

### Question 4: Collaboration and coordination with other institutions

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

- This project has a good number of good-quality international collaborations and a good connection with industry.
- Because of the cost and nature of this type of work, most of the work is performed among the national laboratories with some industry input. Those industry members who can cost-share this testing have an advantage.

- There is collaboration with other expert institutions on experiments and modeling and the results are made available to outside institutions. Additional collaboration with others is mentioned in the future work.
- This project seems to have performed joint research projects with five other groups. It is not clear what SNL's participation was, nor how it fostered further joint work.
- Slide 23 indicates that collaborations between various international partners had occurred, but does not indicate in what way the collaboration was beneficial to either party. The particular slide format appears more as a biography or reference list rather than collaboration.
- The project is very well integrated with both the domestic and international codes and standards development process. Project experts understand the codes and standards development process, and work well with and often serve as technical committee members who prepare codes and standards. The technical exchange of a researcher from China on the fast-fill model is an excellent example of collaboration, as is the involvement of a tank manufacturer (slide 6).
- The accomplishments of the risk evaluation and flame radiation work are outstanding. The $H_2$ release behavior and collaboration on $H_2$ safety is good. However, the work being done on fast fueling is so poor the progress (average) can only be fair. There were statements made that the work would be presented in the J2601 team. The work should have been first coordinated with U.S. industry before collaborating with the Chinese effort. It is unclear what the purpose of showing external temperatures and testing only to 2,000 psi was. The project is at best pretty slides, and has no merit whatsoever.

### Question 5: Proposed future work

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

- These are very ambitious plans, but it looks like another year of "odd jobs," rather than a coherently planned research project.
- The maximum final pressure of $H_2$ fast filling should be raised from 2,000 psi at least to 5,000 psi. The higher the final pressure, the greater the temperature increase.
- The National Fire Protection Association (NFPA) indoor refueling work, burst pressure ratio performance testing, and model validation activities are all important activities for the near term. They will also feed into other standards development organization (SDO) work.
- The future work identifies additional complementary research on $H_2$ behavior, and other work that feeds directly in to codes, standards, and protocols for fast fill, burst testing, and indoor refueling requirements.
- It appears that there is a lot of work remaining in fiscal year (FY) 2012 and no indication that these projects would be completed. The future work presented is a good mix of $H_2$ test activities, but does little to highlight how the work affects SCS. The only standards organizations that are mentioned are NFPA and the International Organization for Standardization (ISO), but the references are subtle and vague in terms of what specific tasks need to occur to "finalize indoor fueling requirements" and "incorporate mitigation table into NFPA and ISO codes."
- The proposed future work could be more informative if described in a table that shows the R&D category (e.g., $H_2$ behavior), the specific activity (e.g., validate burst ratio performance test), the codes and standards that will benefit from this activity, and the relationships to and/or extensions of current work. More details on future work would also allow better evaluation of project progress to be reviewed in the following year. This may require a change in the format of the AMR presentations.
- Some of the points identified are obvious continuations of the ongoing efforts. It seems that the effort or at least the number of topics is decreasing in 2013. For the fast filling, the plan to go up to 10,000 psi and finalize the task within 2012 is a challenging task. There was no mention of fast filling in the 2013 plan. It is not clear in FY 2012 who the collaborators are. They are not mentioned explicitly in the collaboration slides, at least for that topic.

### Project strengths:

- This project has excellent technical capabilities.
- There seems to be some good science in this project; it shines through the obfuscation.
- Producing data for science-based codes and standards, and developing protocols to remove specific deployment barriers are strengths of this project.

- This project has a significant amount of good $H_2$ behavior testing. The data generated is surely useful to codes and standards groups.
- This project has outstanding and focused R&D that demonstrates excellent experimental design and state-of-the-art experimental facilities and modeling capabilities.
- In terms of $H_2$ release behavior, this project has done excellent work on $H_2$ forklift and tunnel releases indoors. This is very valuable for the industry.
- In terms of flame radiation, the project has access to well equipped test facilities and diagnostic equipment, which are critical for validating models on consequence estimations of $H_2$ releases and flames.

### Project weaknesses:

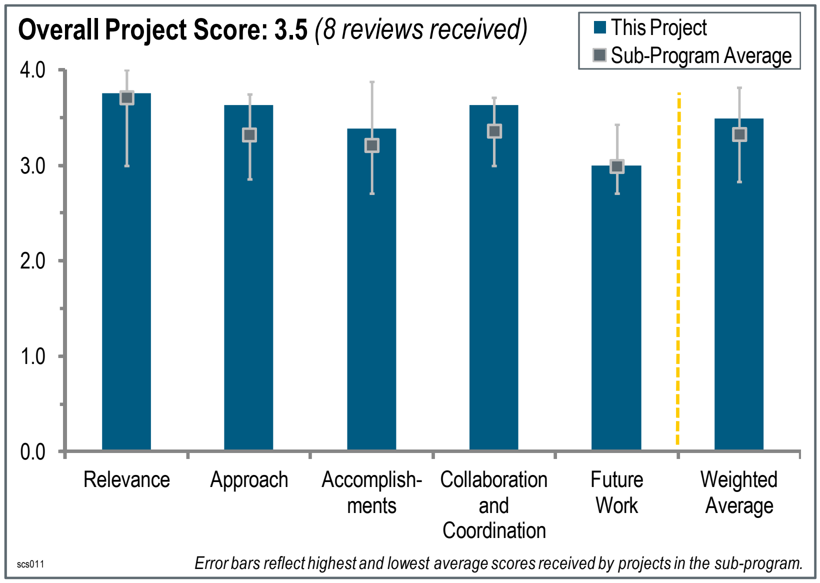
- All the work being done on fast fueling and simulation is poor and a waste of effort.
- This work is stretched out and the researchers need to define shorter term goals. This type of research takes time, but getting data out of the national laboratories and into the hands of the industry and SDOs is an issue.
- The range of conditions that can be investigated in some of the experimental facilities is not large enough to cover the range of experimental conditions that can be experienced in real-scale systems (e.g., pressures up to 10,000 psi). The identification and use of performance indicators will be beneficial to monitor the status of the projects and better demonstrate actual progress to reviewers.
- The presentation was very poor. It was disorganized, far too data-jammed, and not really responsive to the evaluation criteria. It was also notable that the responses to reviewer comments were virtually identical to those given at the 2011 AMR, with one minor addition. The project clearly needs to be less scattershot and to focus more on communicating results in a clear and usable way.
- The presentation did not adequately address how this project is helping certain SDOs with updating or creating standards. There is good work being performed, but the presentation style did not close the loop showing how this all fit together (or the relevance to the Probabilistic Risk Assessment [PRA] approach). Unfortunately for this project, its counterpart presentation was placed afterward, so reviewers were left very confused with what relevance this information had with codes and standards.
- This project includes a number of complex experiments related directly to a number of different code and standards development activities, and it is difficult to understand the breadth of the totality of these experiments and their implications for other codes and standards issues. Perhaps with an R&D program as complex as that of SNL, an overview presentation (not subject to review) should be given so that the presenters of the R&D work can go into depth with the scope and overall purpose, relevance, and outputs of the R&D understood by reviewers and the audience. This comment reflects more on the structure of the AMR and less on the project's weaknesses.

### Recommendations for additions/deletions to project scope:

- This project should include relevant additional accident scenarios identified in recent gap analyses.
- Continue working with SDOs to accomplish any validation testing required to get a refueling standard published.
- Next time, have senior management thoroughly review and revise presentations by this principal investigator (PI) before they are given in public.
- This reviewer would suggest that the PI improve his charts to highlight the correlation between SCS activities and the work being performed by this project. The work being performed is critical and substantive in nature and is beneficial to SCS, but the presentation had too much information crammed into such a short time and out of context because the presentation that should have preceded it was moved to a later time slot.
- Efforts should continued as planned; however, all efforts in the national laboratories should be canceled, as this has provided no benefit for the industry and has been a waste of time, effort, and funding because no coordination was asked for by industry or standardization efforts (like 2601). This project should help establish a U.S. 70 MPa (up to 87.5) fast-fueling facility at an institution comparable to the current monopoly in Canada, such as the Gas Technology Institute. A third party is needed in the United States, but not a national laboratory.

## Project # SCS-011: R&D for Safety, Codes and Standards: Risk Assessments

Daniel Dedrick; Sandia National Laboratories




### Brief Summary of Project:

Sandia National Laboratories (SNL) is using validated simulations, field data, and expert input to determine risk through quantitative risk assessment (QRA). The objectives of this project for fiscal year (FY) 2012 are to: (1) understand confined releases of hydrogen ($H_2$) through experimentally validated simulations, (2) update the risk model based on the consequences of confined spaces, (3) inform the National Fire Protection Association (NFPA) 2 Code Development Committee of updates; and (4) harmonize other codes and standards.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- This project is most relevant when it informs standards on mitigation strategies or setback distances.
- The work and leadership from the SNL team has been critical to support the standards/code development.
- The incorporation of risk assessment into codes and standards is important to the development of realistic and effective codes and standards.
- This project is essential for the DOE Hydrogen and Fuel Cells Program's (the Program's) success as it provides an analytical foundation for the research and development (R&D) conducted to identify, evaluate, and specify requirements in codes and standards.
- R&D activities, as performed in the project, coupled with input of the findings in codes and standards activities, are absolutely critical in order to enable a deployment of $H_2$ technologies which are accepted by the public.
- The development of a QRA methodology supports the DOE's research, development and deployment (RD&D) objectives. The main focus is to obtain quantitative information and to be able to incorporate these data into a robust model that could impact safety, codes, and standards (SCS).
- This project is relevant to the goals of the Program. The models it generates can be useful tools for evaluating potential safety hazards. Quantitative risk assessment is hard to get right and it is good to see an attempt to get $H_2$ on a firm footing.

### Question 2: Approach to performing the work

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

- This seems like a sound and thorough approach to risk analysis.
- The risk-informed approach is exceptional and by far the best that could be done in such a project.
- SNL is providing important risk-informed input to code developers. Using a strong team with good communication skills, they are making progress in incorporating risk into codes to better serve the safety community and the public.
- The risk-informed approach is sound, but the difficulty lies in the quantification of the different probabilities, as the number of experiences is limited with fuel cell and $H_2$ technologies. The project must base at least some of the calculations on data coming from other industries and applications.

- The project was methodically planned and the plan is being carried out. A bit more aggressive scheduling might have resulted in more rapid progress. Clear experimental validation under well controlled conditions is the key to establishing and tuning any risk-assessment model. Ultimate validation, though, will rest on the success or failure of the model in accurately predicting the probability of major safety events when the model is applied to a real-world situation.
- A risk-informed approach provides a critical link among $H_2$ behavior, system and facility design, hazards and harm, and a structured context for code development. The project has systematically established the capabilities, tools, data, recognition, and understanding by code developers to implement a risk informed approach. This presentation should have preceded the other SNL presentations to provide context for them.
- Science-based, risk-informed assessment with its consecutive phases as documented in the presentation is a correct approach. The term “informed” should be stressed more in the presentation and in the slides. What has not been stressed sufficiently in the presentation is the absolute need for coupling back to ongoing $H_2$ technology validation exercises in order to obtain $H_2$-specific metrics of frequencies and postulated initiating events to better document and quantify steps in the risk-informed analysis. This is needed in addition to the input that can be retrieved from “$H_2$-like” industrial and technological applications (which was mentioned in the presentation, but needs additional expertise to “translate” into $H_2$-relevant metrics). The comment in red font in the lower left hand corner of slide 10 is perfectly true and should be taken up at the level of the overall Program.

## Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Steady progress is being made toward the goal.
- Progress has been very good at incorporating risk into the code development process.
- The work and leadership from the SNL team has been critical to support the standards/code development.
- Ignition delay/location is a key factor, but does not seem to have been adequately studied yet. The application of this work could hinge on key questions as to whether ignition can be prevented or whether ignition should be induced sooner in some cases to mitigate the consequences of delayed ignition. The work on tunnel ventilation raises this question.
- The harmonization of NFPA 2 ($H_2$) and NFPA 502 (Tunnels) is perhaps the best possible outcome to the risk analysis and release simulations. The commercialization of fuel cell vehicles also needs to have the codes aligned regarding $H_2$ safety in enclosures and tunnels. Without this work, the timeline would be considerably threatened for the near-term fleets.
- The project has made outstanding contributions to the Safety, Codes and Standards sub-program. The project previously enabled incorporation of a risk-informed approach in preparation for NFPA 2, and direct participation by project experts in key NFPA 2 task groups is essential for the modification and improvement of NFPA 2. More discussion is needed on how the project met its metrics for success (slide three), although this is implicitly addressed by its impacts on regulations, codes, and standards (RCS) development.
- Significant progress towards the objectives has been made, but in view of the limited funding available, the choice of a limited number of problems to be treated is necessary. Instead of using data from related industries, like nuclear power plants and offshore oil and gas, the project could benefit from inputs coming from industries constantly using $H_2$-rich gases, such as producers of $H_2$ gas, the steel industry (coking gas contains at least 60% $H_2$), the chemical industry (chlorine and chlorate production sites emit large quantities of $H_2$), or the petrochemical industry ($H_2$ plants). Nevertheless, it is recognized that it could be difficult to obtain the expected data.
- It is unclear how the last step in the QRA sequence graphically displayed on slide 5 (nice picture) is addressed in this work, namely moving from hazard probability to harm probability. Members of the general public more easily understand “harm” than “hazard.” This translation from hazard probability to harm probability needs an additional factor, namely “likeliness and frequency of exposure” (people, as well as grey and green infrastructure) to the considered hazard. Good accomplishments were shown in the presentation. However, it is not easy to clearly identify from the presentation the level of progress related to last year and to what extent 2012 progress contributes to the achievement of the final target of the research in each field of investigation. It is not clear from the slides which topics have been already closed or are near to be closed. The percent complete (80%) in a very long timeline (2003–2015) is too generic for an annual merit review. The actual values for the metrics for success defined in the third slide are not given. Anticipated work on telecommunication towers

(mentioned in the 2011 DOE Hydrogen and Fuel Cells Program Annual Merit Review [AMR] as future work) seems not to have taken place.

### Question 4: Collaboration and coordination with other institutions

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

- This project has the appropriate set of collaborators with the right skills and needs.
- The work and leadership from the SNL team has been critical to support the standards/code development.
- Close and appropriate collaboration with other institutions was presented and resulted in the harmonization of standards.
- Coordination with NFPA has been very good, however work with the International Code Council (ICC) and other collaborations internationally, such as with Germany and Japan, would be beneficial.
- This project has a good number of good-quality international collaborations and also a good connection with industry as shown by the list of industrial partners.
- It sounds like there needs to be a process put in place to receive private sector inputs with a “clean room” type of approach that ensures confidentiality.
- This project has a good relationship with all the right organizations. This project is beginning to take an active role in standards organizations and applying early QRA results to help generate rational codes and standards.
- Collaboration and coordination are outstanding with the project playing essential roles in important codes and standards development under NFPA, Society of Automotive Engineers (SAE), Canadian Standards Association (CSA), and International Organization for Standardization (ISO), among others. The project has also made notable contributions to the International Energy Agency’s (IEA) tasks 19 and 31. Its collaboration with Canada is also valuable for international cooperation and sharing of expertise and data.

### Question 5: Proposed future work

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

- The proposed future work is presented in a logical manner and is built on past progress and experience. In view of the limited funding available, a clear definition of the problems to be addressed is needed.
- In addition to the analysis approach and tools, it would be good to see some papers written that demonstrate the application of learnings to real-world designs or that provide insights relevant to real world applications.
- The present list is valuable; however, it does not address a few near term needs. $H_2$ fueling standardization and codification needs to have immediate priority to help the build-up of infrastructure.
- An increased focus on “industry outreach” is planned now that the team is comfortable with the laboratory-validated model, and this is good. A plan to move more quickly into applying QRA to “real world” standard generation is needed.
- There will be a continuing need for this effort, partly because there is so much that needs to change in existing codes to incorporate the concept of risk in decision making. This is an unending “calling” that will need to be carefully applied to the most critical situations.
- Some of the points identified are the obvious continuation of the ongoing efforts. It seems that the effort, or at least the number of topics, is decreasing in 2013. Decision points and possible alternate pathways are not explicitly identified in the slides.
- More detail on the planned future work is needed. Such detail would show how current work is being extended and improved. This comment may require a change in the format for AMR presentations. Future work is treated almost perfunctorily: one summary slide with little detail. More emphasis on continuity, consequences, and building upon current work is needed.

### Project strengths:

- This is an excellent team at SNL with a deep understanding of the application of risk to decision making.
- The work and leadership from the SNL team have been critical to supporting the standards/code development.
- This project has a structured approach to risk analysis that can be adapted to multiple equipment or facility executions.

- This project has a very strong, methodical, approach to building and validating a QRA model for $H_2$ release events.
- A robust methodology based on a scientific approach is a clear strength of the project together with the good contacts with the codes and standards task groups.
- The researchers have excellent technical expertise in QRA and extensive experience in its application. The risk-informed approach that the project has built, and the incorporation of this approach in the codes and standards development process are major accomplishments for the Safety, Codes and Standards sub-program.
- The main strength lies in the method applied; namely, risk-informed analysis based on relevant scientific input obtained from targeted numerical and experimental efforts coupled with appropriate consultation and input from stakeholders. The project tackles crucial issues in risk assessment, generating progress in the field in a context of close collaboration with the relevant stakeholders (e.g., industry, codes, and standards groups/committees, other research institutes).

### Project weaknesses:

- This project has difficulty with quantifying risk.
- The efforts must be devoted to obtain reliable data from the industries already dealing with $H_2$ production and use.
- The presentation has absolutely no discussion of relative risk. The project doesn't mention QRA models developed for related industries, such as natural gas. It should be relatively easy, at least in theoretical model systems.
- The communication of results, such as pressure relief panels, should be brought to the forefront. This could be some good additional knowledge that industry could benefit from, such as with small enclosures for test vehicles.
- It seems that the issue of the level of uncertainties involved in risk assessment is not addressed (it is at least not mentioned). A methodology that provides the level of uncertainties of the risk assessment should be developed or included. The identification and use of performance indicators will be beneficial to monitor the status of the projects and demonstrate the progress achieved.

### Recommendations for additions/deletions to project scope:

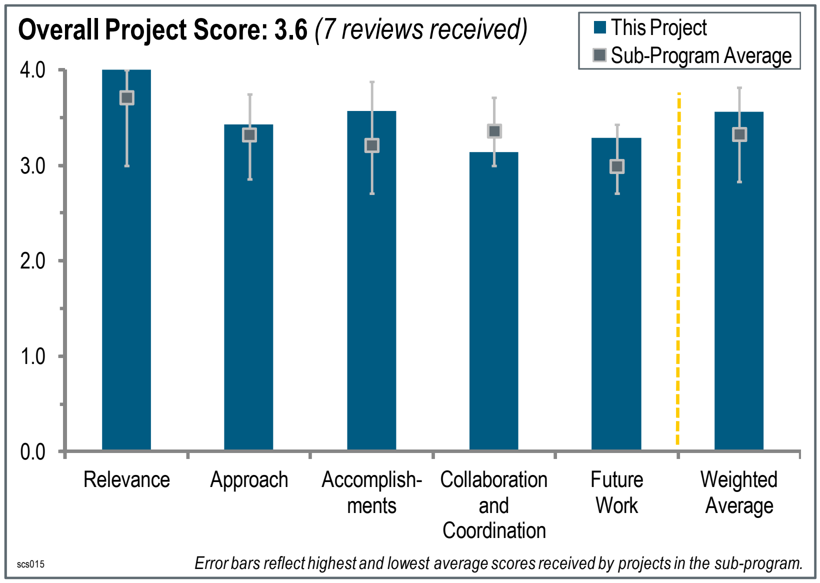
- This project should address uncertainties in the different stages and their propagation to the final outcome of the risk-informed assessment. There needs to be a dedicated effort to translate from “hazard” to “harm.”
- This project should help the standards for fueling including SAE J2601 putting their standards into the $H_2$ codes and NFPA 2 ICC to enable a safe infrastructure. This project should restart the Hypoc in the near term to accommodate changes in the codes to accomplish DOE 2015 goals in the code cycle.
- This project should attempt to provide statistical verification of the model by applying it to an ensemble of real-world installations and comparing the risk prediction with actual occurrences (or non-occurrences). It should be relatively easy, at least in terms of using the now-validated model, to compare relative risks incurred by $H_2$ storage with those associated with other energy storage methods (from gasoline to dammed water). Surely QRA models exist for those methods as well. This could be a very illuminating comparison.
- Development of a web-based QRA tool has been discussed for several years, but it has not been seriously considered or undertaken. Perhaps the “comprehensive reference for $H_2$ system QRA” (slide 14) can lead to the development of such a tool that will facilitate widespread adoption of a risk-based approach to designing, siting, operating, and approving $H_2$ systems. The evaluation of the effects of obstacles after ignition in indoor releases (slide 8) would be a valuable addition to project scope.

## Project # SCS-015: Hydrogen Emergency Response Training for First Responders

Monte Elmore; Pacific Northwest National Laboratory

**Brief Summary of Project:**

The long-term objective of this project is to support the successful implementation of hydrogen ($H_2$) and fuel cell technologies by providing technically accurate $H_2$ safety and emergency response information to first responders. The specific objectives for fiscal year (FY) 2012 are to: (1) offer the one-day operations-level course utilizing the U.S. Department of Energy's (DOE's) fuel cell electric vehicle (FCEV) prop at appropriate fire training centers; (2) continue to support the web-based awareness-level course (launched in FY 2007); and (3) continue outreach activities by disseminating first responder $H_2$ safety educational materials at fire training conferences to raise awareness.




### Question 1: Relevance to overall DOE objectives

This project was rated **4.0** for its relevance to DOE objectives.

- This project is critically relevant to serve the first responder community with training specific to alternative fuel technologies.
- This course directly addresses the education needs of the first responder community around a new/unknown vehicle and fuel technology, which is critical to the implementation and commercialization of $H_2$ and fuel cell vehicles.
- This project is extremely relevant to the DOE support of the commercialization of FCEVs. Without this program, there would be an uncomfortable void in the first responder community when discussing FCEVs.
- This project is clearly relevant to the DOE Hydrogen and Fuel Cells Program—both for its great contribution to public safety and its educational value with a key constituency. Fire departments are the "boots on the ground," and they will most likely be the ultimate enforcers of codes and standards, at least at the local level.
- The training of first responders is of critical importance for the public acceptance of $H_2$-linked technologies. The project is mainly focused on transport applications, but extension to stationary applications and early markets such as forklift or backup power installations is required. This project will need increased funding in order to acquire or build new modules and props. The project is definitely in line with DOE objectives. It could be extended, provided that extra funding is available, to emergency medical personnel and volunteer firefighters.
- Training for first responders is critical to the deployment of any alternative fuel. The public has an expectation that first responders will know how to proceed when they arrive on the scene of an accident or incident. The first responders must also have the training and confidence to approach such accidents, understand and mitigate the risks to passengers and to themselves, and bring the situation under control.

### Question 2: Approach to performing the work

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

- The project is focused on the barrier of understanding what to do when encountering an FCEV that is in dire straits.
- The project's training prop, one-day course, web-based training, outreach, and near-term focus on California deployment are all very positive. The project is able to reach a large audience with a low budget.
- The approach was tightly focused on the mission: to provide relevant, timely training to first responders in the most effective way. It is hard to see how the team could have done a better job on such a limited budget.
- The flame prop appears to be an extremely useful tool. More consistency among state and college programs would be beneficial.
- The hands-on approach is probably the best way to instruct firefighters. This is why the building of props covering other $H_2$ applications than cars is suggested. Problems of bursts and high-pressure leaks in flexible materials could be better observed and treated. In Europe, firefighters need to give their approval for the building and operation of equipment that uses explosive compounds. Information on how the $H_2$ is diffusing in close environments and how to ventilate correctly in these environments could be added to the training. The decreased number of consultations of the website is raising some questions regarding its content and maintenance. An effort to make it more attractive and highlight the recently added elements could be realized.
- This course could be adjusted so that it is easily scaled to meet the needs of the particular department being trained in order to accommodate time frames, for example (i.e. truncate the in-class time) or to allow for the experience of the students (e.g., modify the prop portion)—all of which would maintain the quality of the course and the information delivered.
- The approach is consistent with the expectations of the project sponsors and the barriers. This reviewer asked whether the approach for the target audience (first responders) has changed with regard to more comprehensive training in all types of alternative fuel vehicle response techniques. If so, this reviewer would like to know whether this project has revised its approach to address this change. In addition, this reviewer asked whether activities in other industries could benefit from pooling resources for the community, and whether there is a private company that is interested in assuming responsibility for this material as an end result of this project.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Progress has been excellent. This training has been well received by the first responder community.
- This is an excellent and very well received course; however, DOE has funded other emergency response programs on alternative fuel and/or $H_2$ vehicles that may not have the same quality of vetted information or messaging.
- This project has trained more than 21,000 web users and 710 on-site training attendees, more than 600 of whom work in jurisdictions along the California Hydrogen Highway. The project has made impressive progress.
- The number of trained officials is constantly increasing, and the feedback received from trainees is very positive. Last year, it was recommended to enlarge the geographical distribution of the training sites and to go outside of California. Unfortunately, it seems that in 2011, training only took place in California. This starts to be more and more critical in view of the deployment of an increasing number of $H_2$-powered forklifts outside of California.
- Outreach activities were limited by the budget, particularly the live training with the FCEV prop. This reviewer would have liked to see some work on leveraging DOE resources. With a bit more funding, many more fire departments could have been visited. The course material may be getting a bit stale; however, this looks like it will be addressed in the coming year.
- Training accomplishments are excellent, particularly the number of participants, given the limited resources. The impact of training was unclear. For example, this reviewer wondered whether there is a means for evaluating incident responses where training has occurred versus incident responses where training has not yet occurred. There are several recent incidents (in the $H_2$ incident database) where the responders were not provided this training. It was also unclear whether the trained first responders have had any incidents, even false alarms; and whether there are any conclusions that can be drawn by comparing the responses. This comparison

might help provide a critical data point (i.e., the value of this training), which seems to be missing from the accomplishments and progress measurement.

### Question 4: Collaboration and coordination with other institutions

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

- This project has achieved excellent collaboration with the first responder community.
- This project has demonstrated very good collaboration thus far, both locally in California (California Fuel Cell Partnership) and through the International Association of Fire Chiefs (IAFC) and the U.S. Department of Transportation (DOT).
- There appear to be redundant efforts in developing first responder training funded by DOE. Optimizing training material and reducing duplicative efforts would increase the value of this program.
- Collaboration exists, but it appears to be limited to California. Even if it is recognized that California is probably the most advanced state for FCEV deployment, efforts to cooperate with other institutions for other types of applications could improve the project.
- Collaborations and coordination suggest that this project has created a premier educational tool that has been very beneficial in targeted communities. This reviewer wondered why state fire academies are not included in the list of collaborators. Even negative responses would be beneficial and would credit this project with investigating options for growing the project. It seems that the project funding has significantly declined, which raises a question about how strategic collaborations can play a role to leverage the existing funding to ensure that project goals are met. This reviewer also asked what training would interest the fire service, if they are not as interested in this training (e.g., would they be interested in a combination of all alternative fuel vehicles in a single class). If such a concept was appealing and supported by the fire service or the Federal Emergency Management Agency (FEMA), this reviewer asked whether this project has identified the potential collaborators. In addition, this reviewer asked whether the project has investigated the potential for federal facilities to act as 'early adopters' of $H_2$ technology and the opportunities to train U.S. Department of Defense (DOD) first responders. The Defense Logistics Agency is highlighted, and it is unclear whether there are plans to expand this interaction to other fuel cell and $H_2$ technology deployments.

### Question 5: Proposed future work

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

- This reviewer recommends focusing on early market areas for fuel cell vehicles.
- $H_2$ applications are developing progressively. The proposed future work, built on past progress, focuses on new barriers for new types of applications, but it could be more ambitious in terms of its geographical distribution and addressed topics.
- With the commercialization of $H_2$ fuel cell vehicles and the implementation of fueling infrastructure in California and on the East Coast, there needs to be continued and increased funding for the delivery of this course.
- This project should focus on on-site training for up to 500 additional responders in the coming year, mostly in California, and possibly on the East Coast. It would be beneficial to expand the training eastward.
- Expanding the training to include material handling equipment is an excellent idea. It's a bit surprising that responding to events at refueling/storage sites isn't already in the course, but perhaps firefighters consider themselves to already be trained to respond to similar incidents.
- It is unclear where this project is going, and how much funding is required to support it. It would appear that there has been a precipitous decline in funding that is inconsistent with the project's future plan and possibilities. The same future work was proposed in 2011, and funding was not received, so this reviewer asked what other avenues are possible for this project if funding remains so low, what level of funding is too low to support the project at all, and whether there is a way to leverage some funds for a targeted effort in a different direction (e.g., the state fire academy assumes responsibility, the national fire academy provides sponsorship, FEMA provides funding, a private company shows interest, and so on).
- The purpose of this training is to familiarize the first responders about what to do with the hardware they encounter. However, in large metropolitan areas, 80% or more of the first responders are emergency medical

services (EMS) personnel. This reviewer asked whether there can be some focus on the physiological aspect of exposure to $H_2$ fuel in an accident scenario. This reviewer also asked whether students are given a material safety data sheet for $H_2$, or whether that is left to individual organizations to see to their own EMS training. Putting some emphasis on the physiological effects of $H_2$ exposure may work.

## Project strengths:

- This project is a good value for the money.
- The training is credible, with hands-on applications.
- The quality of the training and the hands-on approach are clear project strengths.
- These are excellent instructors with a wealth of knowledge. The course is comprehensive and features excellent hands-on time with the prop.
- This is very necessary training. It is beneficial to provide on-site and online training first in critical deployment areas, and then follow up with on-site training for the rest of the country prior to deployment in those areas.
- The project has created a comprehensive education program consistent with its project goals. The impact of the project to those who have used it appears to stretch beyond the metrics presented (i.e., there is more value than the metrics presented).

## Project weaknesses:

- Budget constraints limit the scope of this program. This reviewer expressed the hope that funding will accelerate as a commercialization decision nears.
- There are duplicated efforts on this same topic(s) that have been funded through other DOE programs, and there is no obvious tracking/monitoring of the material, content, and messaging throughout these efforts.
- Should increased funding become available, the project could be extended in scope and focus. The training of other categories of first responders (firefighter volunteers, medical emergency personnel, and private company personnel) could be considered.
- There are no project weaknesses, except that funding appears to be very low. It would be beneficial to expand on-site training or at least have some way to ensure that online training is well advertised. The project should collect stats on regional participation in online training, and it should provide some outreach to authorities having jurisdiction throughout the country and (perhaps) to schools that have fire technologies in their curricula.
- There is a need to reach more firefighters with the hands-on course and to increase website traffic. It is disconcerting that more users are not signing up—$H_2$ usage is growing, and it is important to train as many first responders as possible, in case major incidents occur.
- The project has created an educational program that exceeds the needs of the target community. While this was perhaps the intention of the project's charter, given the decelerated pace of light-duty fuel cell vehicles, it is unclear what the project has done to respond to the accelerated use of fuel cells in 'early' or 'niche' markets, such as backup power and forklifts. It seems that this project is in danger of losing funding before vehicles are on the road and before the training becomes very relevant. It is unclear how the project is prepared to bridge this gap and whether the project is leveraging all of the resources in the DOE Safety, Codes and Standards sub-program and the Fuel Cell Technologies Program to identify sources of collaboration to address the funding and strategic goals. This reviewer also asked whether all of the Safety, Codes and Standards sub-program leads are aware of this project, and whether they promote its use among industry relationships; whether the $H_2$ and fuel cell industry is a user and promoter of this program for its installations and interactions with the fire service; how this program could reach those "friendly" marketing networks to better reach targeted audiences (e.g., local fire departments and state officials involved in fuel cell and $H_2$ deployments); and whether the industry views this project as a helpful tool for its deployments, and if so, whether there is value in their support.

## Recommendations for additions/deletions to project scope:

- This reviewer recommends widening the scope of the training.
- Cooperation with other projects as regards the exploitation of real incidents and how to act in front of these incidents could improve the project.
- The project should consider leveraging fire academies, DOD, DOE program advocates, and industry advocates. It should also identify alternative support mechanisms beyond the Safety, Codes and Standards sub-program. In

addition, it should explore the insurance industry's view of the value of this training (e.g., would they consider training as a value to their risk evaluation).

- The project should try to arrange an "East Coast road trip" paid for by industry/insurance companies/DOE. Also, they really need to freshen up the web course. It is surprising that the number of users is shrinking, rather than growing, considering the growth in the number of deployed $H_2$ fuel cells and refueling/storage sites. This reviewer strongly recommends increased funding for next fiscal year.
- The material in this course has been vetted by industry and is comprehensive in its information and accuracy (although updates are needed). Some funding should be made available to allow for a comprehensive review of all DOE-funded ER programs (e.g., the National Alternative Fuels Training Consortium and National Fire Protection Association programs) and collaborative efforts (e.g., the DOT/IAFC and DOD-Tank Automotive Research, Development and Engineering Center collaborations) that include (or intend to include) $H_2$ in order to identify gaps in messaging, content, accuracy, and so on. If multiple programs are being funded by DOE, they need to be consistent in these areas. Additionally, this course should be the primary "go to" course (along with the online portion), meaning that more advertisement is needed. This reviewer recommends continued and increased funding for deployment, at least through 2015. The project could potentially decrease spending by printing fewer hard copy materials (every attendee does not need/want a book). The project may also consider providing online access to materials (via password after the course has been taken).

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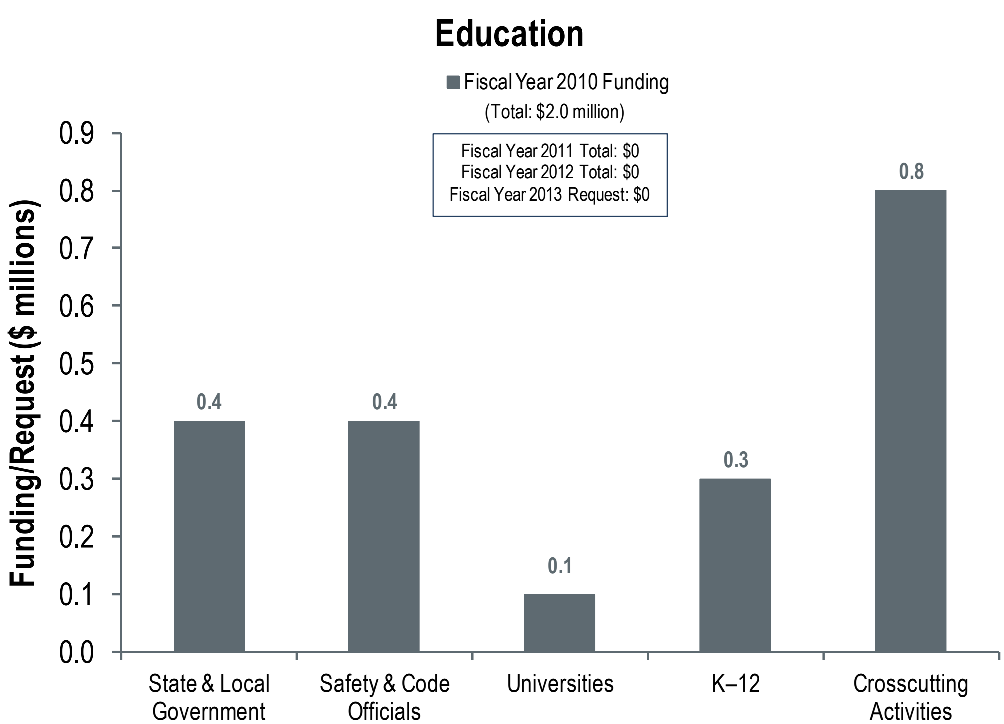
# 2012 — Education
# Summary of Annual Merit Review of the Education Sub-Program

## Summary of Reviewer Comments on the Education Sub-Program:

Reviewers considered the Education sub-program to be focused, well-managed, and effective. They highlighted that the sub-program has made important impacts on the knowledge and comfort level regarding hydrogen among the general public, education system, decision makers (both state and regional), and first responders. Reviewers emphasized that the sub-program activities are critically important to the successful adoption of fuel cell technologies, particularly in the areas of state and regional education, partnership building, policy formation, and information management. Concerns about the lack of funding for the Education sub-program were expressed repeatedly, and reviewers consistently encouraged continuation of education efforts to support hydrogen and fuel cell deployments.

## Education Funding by Technology:

The Education sub-program efforts are prioritized to focus on the target audiences involved in facilitating the use of hydrogen and fuel cell technologies for near-term and longer term applications. No funds were appropriated for the Education sub-program in fiscal year (FY) 2011 or FY 2012; projects reviewed were funded with prior year appropriations.



## Majority of Reviewer Comments and Recommendations:

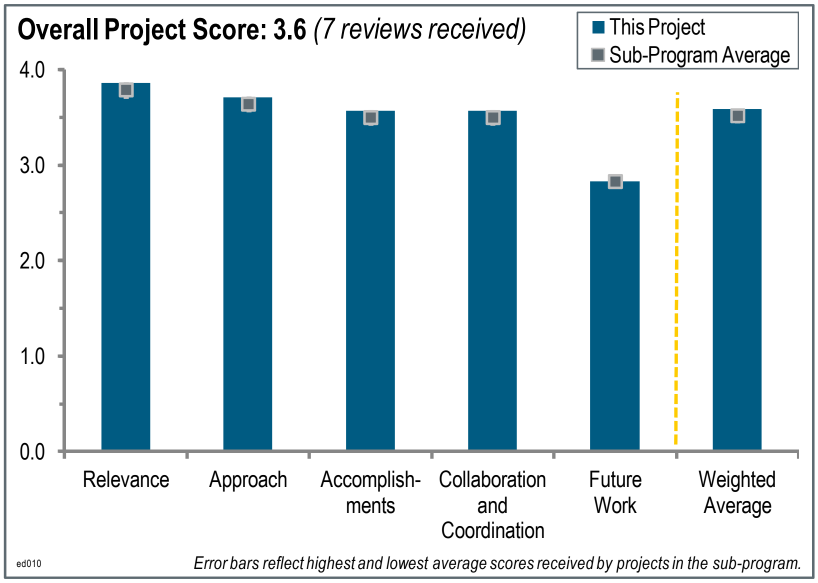
Two Education sub-program projects were reviewed, and they were rated very highly, receiving scores of 3.6 and 3.4. Scores reflect the progress made over the last year and the plans for future activities.

**State and Local Government Officials:** Both projects reviewed were for educating state and local government officials. Reviewers observed that because state and local leaders are potential technology deployment facilitators, their education is essential to the future success of hydrogen and fuel cells. Reviewers also commented that the locales of the projects were well-chosen, in states with an existing hydrogen and fuel cell presence. Key recommendations were that additional collaboration should be pursued to create programs that can be replicated across multiple states and regions, that the metrics used to track progress should be improved, and that integration with the Clean Cities Program could be a way to continue their efforts.

## Project # ED-010: Development of Hydrogen Education Programs for Government Officials

Shannon Baxter-Clemmons; South Carolina Hydrogen and Fuel Cell Alliance

**Brief Summary of Project:**
The objective of this project is to accelerate the ongoing development of a hydrogen and fuel cell economy in South Carolina and the Southeast by providing accurate and reliable information to state and local decision makers. Information dissemination tools include developing presentation tools and formats such as webinars; educational Internet browsing tools; and live presentations for state and local government officials, industry leaders, and stakeholders.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.9** for its relevance to DOE objectives.

- This project is leading to additional hydrogen and fuel cell projects in the area, which supports DOE's goal of reaching multiple stakeholders. Meetings with legislators help to make a case for continued funding for this project.
- The initiative serves as a focal point for information dissemination from within the state and region. It is poised for growth in research, development, and deployment.
- This project is very relevant to the mission and goals of the DOE Fuel Cell Technologies Program (FCT Program) because it has direct impacts and gets the word out about fuel cells and hydrogen.
- The principal investigator (PI) does a great job identifying influential people and reaching them with messages that resonate with them. The PI encourages support that is at a high level and helps DOE keep the focus on fuel cells and hydrogen.
- The project has addressed the objectives of the Education sub-program and has helped to create an environment where the FCT Program can be more successful. It was great to see that the PI was not only looking out for South Carolina, but was also helping the program through this project's activities.
- This project is right on. The FCT Program needs to continue outreach to political stakeholders, particularly in this critical time of early market deployment. The success of the material handling market and other early markets needs to get attention from influential political people to increase awareness and support. This outreach should not just be in South Carolina, but nationally and internationally as well.
- Education is critical to the emergence of a hydrogen economy, fuel cell electric vehicles, reasonable safety codes and standards, and the awareness of various fuel cell applications and the domestic jobs they create. This effort addresses all of those elements and does so at a strategic level for South Carolina. Incentives are necessary for the emerging hydrogen and fuel cell markets to grow, and this effort targets key decision makers who can help address the need for incentives. The Hydrogen 101 course helps bring the populace to a minimum level of understanding and counters any myths or rumors that may abound regarding hydrogen and safety. Business cases are very important to decision makers, and this effort focuses on a clear, articulate business case for a number of fuel cell applications.

### Question 2: Approach to performing the work

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

- This project has many activities aimed at reaching out to various audiences.
- The personal, well-focused approach of this project clearly worked. South Carolina is on the fuel cell map largely due to the efforts of the South Carolina Fuel Cell Alliance, and clearly this project contributed to that success.
- The PI has accomplished much with limited funds, limited staffing, and lots of challenges in South Carolina. The PI knows how to stay focused on the tasks that are achievable and part of the program plan.
- Assess, design, develop, deploy, and evaluate is a solid technical approach for delivering educational materials across a wide swath of the South Carolina populace. Greenway Energy is a good partner that produced detailed business cases. The technical approach was to "always focus on hydrogen from an economic point of view." This is a proven and effective practice.
- The project used written materials, direct materials, press releases, and a newsletter of high value. Of the greatest value were the direct meetings, briefings, and forums. This approach, with an emphasis on economic development and job creation, is appropriate.
- It is great to see that this project was able to employ website work, webinars, in-person work, and relationship building along the way. When lots of approaches such as those are employed and well coordinated, other residual benefits occur. That certainly happened with this project. This project shows the benefit of having a PI who understands more than education, including the industry, politics, and how those elements all go together.
- This is a four-year effort, so it is unclear how the original objectives were written. This organization is very committed to the project, and the mission aligns with the FCT Program. However, there does not seem to be a baseline set of information to judge the impact that this has had on employment and the number of fuel cells sold or companies established in the state. The major measure is the number of people "touched," which does not really measure much. On the other hand, this is being done for very little money.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has made excellent progress that is measurable with numbers.
- The materials are of very high value, including significant meetings with government agency personnel and policymakers.
- This project achieved many objectives and tasks, such as setting up websites and key meetings/presentations. The fuel cell forklift value proposition is a much-needed tool for the industry.
- More than 20,000 stakeholders were reached in 2011. This project reached out to key political decision makers including Senator Lindsey Graham, Congressman Wilson (Aiken and Columbia), and Congressman Clyburn. The project team met with the Advanced Research Projects Agency-Energy on South Carolina's hydrogen and fuel cell education efforts. The project was unable to focus on combined heat and power (CHP) or fuel cell backup power for telecommunications because the budget ran out.
- Even with a very modest budget, this project was able to reach a very impressive large list of influential South Carolina political interests. This project met or exceeded the project milestones and expectations. A hallmark of this approach was the success in getting the permitting act in place for South Carolina. (The permitting act was an accomplishment in the last reporting period, but it is worthy of mention this time as well.) While this single accomplishment makes this project very worthwhile, it also added to the success of the rest of the activities. South Carolina is on the fuel cell map largely due to the efforts of the Alliance, and clearly this project contributed to that success.
- This project has been able to accomplish a long list of achievements. It is great to see the outreach from the events, the handouts, and the recorded media hits and residual benefits from events such as a visit from the Secretary of Energy, which was a turning point for how DOE's front office has acted toward hydrogen and fuel cells. The project has been able to accomplish traditional outreach, which is critical, and create tools for the business community on the state of the technology and the value proposition for employing them.
- While the project can count "bodies" that attend events, this does not really measure impact, either on employment, products sold, or companies established. It would seem that this would determine whether a real,

measurable impact was achieved. This may have been the fault of how the request for proposal was written (and the amount of funds available), rather than the performance of this organization. I think it is useful to measure whether a key law or regulation was changed in the state that would impact the ability to use the technology in the state. This was done.

### Question 4: Collaboration and coordination with other institutions

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

- This project is working with fuel cell companies to distribute the forklift brochure, as well as working with many other South Carolina entities.
- This project has been very successful at teaming and collaborating with relevant stakeholders.
- The project has been coordinating well with the state and other states/regions doing similar activities.
- The PI has uniquely pulled together industry, government, academia, and non-governmental organizations. The PI looks for support and ideas in places that many programs ignore.
- The collaboration in South Carolina is very good. Regional collaboration with additional states is growing, with possible high-value rewards if such collaboration were to be funded and continued.
- The PI has collaborated with several organizations to accomplish the goals. The organizations are very diverse, and include universities, companies, and government. However, there appears to be some room for improvement in utilizing the strengths of the other organizations to accomplish even more. Perhaps this was done, but the presentation mostly focused on what the PI did.
- The PI promoted fuel cells and hydrogen in all parts of South Carolina. Every new member of Congress from South Carolina, as well as members of the state legislature, were identified and contacted with offerings of educational information on hydrogen and fuel cells, particularly the economic benefits. The PI is considered one of the most articulate spokespersons on hydrogen and fuel cells, combining a solid engineering understanding with great outreach skills. This combination has helped keep South Carolina in the forefront of states that are favorable to this technology. The Alliance has been very successful in the number of brochures and other handouts it has been able to disseminate across the state and region.

### Question 5: Proposed future work

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

- The Education sub-program, above all others, needs to be continued and should get more funding.
- The work that was focused on South Carolina activities could be expanded throughout the region and additional states as a regional information center.
- The award is ending, so the project will have limited resources. The project has good plans to expand to other southern states, but it needs more funding to continue the success of this program.
- The work that this project would like to accomplish would be extremely valuable to the industry, and especially to accelerating the industry in South Carolina. It is unfortunate that there is not enough money to be able to do everything on the project’s list.
- The PI will continue the effective dialogue with local and statewide officials in order to promote the use and support of hydrogen and fuel cells. There is not enough funding left on this project to go beyond January 2013, so a minimal level of effort will be applied. There will be no fuel cell CHP or telecommunications fuel cell backup power fact sheets or business case/outreach produced. The work the budget supports is laid out in a logical fashion for the remainder of this effort. The PI made a strong case for continued federal co-sponsorship in hydrogen and fuel cell education.
- More aggressive outreach to other states would be good. There is a need to tie together all states that are active in hydrogen technologies and to increase that number. South Carolina has established a very good track record and is recognized as being one of the few states that are successfully supporting and growing the deployment of hydrogen technologies. An activity where states start to work together on building a state-up (ground-up) partnership can clearly benefit from the leadership shown by the Alliance and the state of South Carolina.

### Project strengths:

- Reaching a wide audience of key stakeholders ripples out to others, making the DOE dollars count.
- The personal outreach from very passionate people in this field was clearly shown to be one of this project's strengths. This project should continue and broaden its focus to include other regional/state interests.
- This is a gregarious approach to facilitating meetings and forums, development of personal alliances, and openness to work with South Carolina partners of very high value.
- The organization and staff are deeply committed and strategic about how to create a relatively large impact on a very small budget. They seem to be very aggressive about contacting key decision makers.
- The strengths of this project revolve around the fact that the traditional outreach was exceptional, but so were the residual benefits. The PI is well connected and was able to leverage the resources of this project to do more than accomplish basic outreach goals.
- This project is pulling together many different partners and collaborators, sticking to the project plan, and not letting the scope creep. The team is reporting realistic results and paying attention to business.
- A wide range of leaders and other stakeholders have been contacted and educated. The focus on impact metrics (quality) rather than just activity metrics (quantity) is quite impressive and not seen in any other state effort. The PI is particularly qualified, articulate, and energized on the topic of hydrogen and fuel cells. A correlation exists between education and market adoption, as evidenced by this program (Fort Sumter, BMW plant, University of South Carolina hydrogen vehicles, South Carolina Hydrogen Fund, and Fort Jackson backup power).

### Project weaknesses:

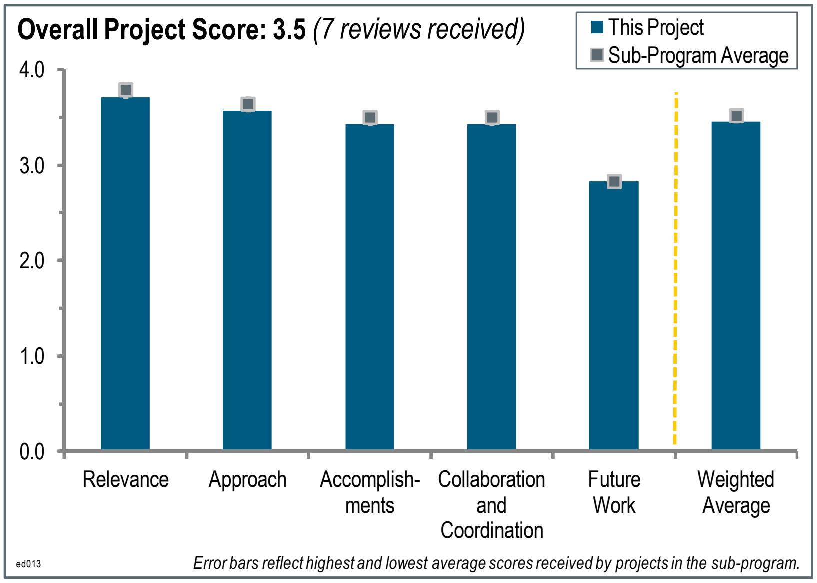
- This project's weaknesses are dependent on resources.
- This project needs more funding to do this important work.
- The project's major weakness is that it is coming to a close.
- The time and resources needed for long-term program administration for effective policy development is a weakness of this project. This is a long-term program that must be supported for long-term productivity.
- The one place that seemed relatively weak was the collaboration with other organizations. It was unclear how exactly they were utilized. In a world of constrained resources it would have been good to hear how the PI was resourceful in utilizing the resources of other organizations to enhance the benefits this project was able to provide.
- This project needs to really measure the impact of employment, sales, and start-up companies, even if this is done as a baseline (2008) and ending (2012). It would be a better measure of impact than the number of bodies who have sat through a briefing.
- This project has no weaknesses.

### Recommendations for additions/deletions to project scope:

- This is a great project that should continue.
- This project should not spend too much time on the zero emission vehicle (ZEV) credits issue. The original equipment manufacturers are now focused on triggering the Clean Fuels Outlet regulations rather than complying with ZEV.
- It is recommend that education programs such as this receive funding so that they can continue, especially as fuel cells and hydrogen become more mature and prevalent.
- The project should figure out how to replicate this program in other states—highlighting South Carolina's successes as a model to attract business and change policy to assist fuel cells and hydrogen. These successes need to be publicized more.
- This project needs to increase the scope for a regional approach. This project should continue to be funded for a two- or three-year contract and expand technical targets as a portfolio for increased flexibility and choice.
- This is the end of this four-year project. Funds should have been added to ensure a good baseline measure of what existed at the beginning of the project in the state (employees in fuel cell companies, sales, number of companies/suppliers).

## Project # ED-013: Raising H2 and Fuel Cell Awareness in Ohio

Pat Valente; Ohio Fuel Cell Coalition




### Brief Summary of Project:

The overall objective of the project is to educate state and local government officials in Ohio about the potential economic and environmental benefits of current and future hydrogen and fuel cell technology, thereby accelerating the deployment of clean energy solutions. The project's goals include: (1) compiling educational materials, (2) marketing and conducting nine forums around Ohio, (3) publishing a bi-annual newsletter, and (4) measuring the increased awareness using the metrics from the U.S. Department of Energy (DOE) Hydrogen Education Sub-Program 2004 Baseline Study.

### Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- This project clearly addresses DOE goals for education and increased deployment.
- This project is focused on the education of local and state officials and decision makers in Ohio, which is a key state because it is home to much of the industry's supply chain and research.
- Outreach programs such as this one are right on. As the deployment of hydrogen technologies accelerate, we need to be more aggressive in educating the class of stakeholders targeted by this project.
- This effort is tied directly to what the DOE Hydrogen and Fuel Cells Program is trying to accomplish: easing the non-technical barriers to ensure that the research and development pays off in the adoption of fuel cell technology.
- This project has very specific and measurable objectives. It is good that the principal investigator (PI) is clear about meeting DOE's objectives of reducing oil and greenhouse gases and increasing jobs in an area of the country where manufacturing is a large part of the economy. Many programs focus on fuel cells as tools for meeting environmental regulations, while this PI focuses on building fuel cells as an economic opportunity.
- The project not only addressed the objectives that were originally planned, but, with the expanded scope of the project, DOE was able to realize additional value. The PI was able to address the traditional objectives related to outreach, mixed messages, etc. The project was also able to support the supply chain, help companies reach new audiences to do business, and also show the role that the Ohio supply chain plays in the national fuel cell business arena.
- Educating key officials as well as the general public is critical to the success of hydrogen and fuel cell adoption, and raising fuel cell awareness is a good means for doing that. The project itself does not attempt to reach out to as many stakeholders as similarly funded programs in other states. The breadth of this effort does not cover a large fraction of the key state personnel that could be educated. Manufacturing represents 17% of the gross output in the state of Ohio, and the state is home to Rolls Royce Fuel Cell Systems, NexTech, Lockheed/Tools & Metals Inc., and a number of other fuel cell and component manufacturers. This makes the case for an education effort that is focused on the importance of fuel cells to the state of Ohio.

### Question 2: Approach to performing the work

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

- This project created a targeted list of selected audiences and used newsletters and forums to reach them.
- The approach (and changing approach) of this project clearly worked. The project met or exceeded its milestones and expectations.
- The follow-up and tracking is a great way to know that the PI is making progress. CleanCities programs need to include the benchmarks that this PI includes, and maybe the PI can share with them how to do this.
- The approach used for in-person events was very interesting, given that there is a significant shift toward webinars. It is good to see that the PI was paying enough attention to which method worked best and was able to modify the approach to utilize the methods that would yield the most results.
- This project seems to be very strategic and tactical in its approach. The team was careful to evaluate what works and what does not, and they modified their plans accordingly (e.g., kept the in-person rural briefings rather than doing ineffective webinars).
- This project has a good, solid technical approach that covers all parts of the state of Ohio. The Ohio Fuel Cell Coalition has many aspects to its education: decision makers, industries, local officials, and a number of other potential stakeholders. The approach does not take advantage of utilizing social media to reach out to the masses. Even webinars were only a small component of the effort. The approach of including matchmaking services for buyers, users, and suppliers can be very effective.
- The collaborative approach to establishing relationships through meetings, conferences, and other activities has been appropriate and of high value. Targeting information to community leaders is an excellent approach, and the numerous forums for community groups have reinforced the message for deployment. Matchmaking events for the supply chain have been of high value to improve manufacturing coordination, which is expected to reduce final original equipment manufacturer product costs. While more time consuming and resource intensive, the forums and symposiums (rather than webinars) appear to be a strategic choice with valuable results.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The forums and conferences are a good way to educate. Following up on matchmaking is key.
- This project exceeded goals and learned from other programs, shared with other programs, and created a follow-up process—all efforts were excellent.
- Having such a large volume of forums, symposiums, and supply chain exchanges is a very valuable accomplishment in regard to disseminating information and integrating industry, supply chain, policymakers, and end users.
- Only 1,200 people were targeted for this evaluation period, and the PI was only able to reach out to 745. Webinars were initially tried but discarded, which severely limits the number of people who can be addressed. Surveys were conducted with the people who received educational services, and the results were favorable. The Ohio Fuel Cell Coalition (OFCC) did 15 forums last year versus the 9 planned forums, but only 15 forums over a 12-month period is not very impressive, given the size of Ohio. Hydrogen 101 seminars were held, along with the creation of a database of state activities related to fuel cells.
- This project was very successful in going beyond expectations, particularly considering the modest funding. The original plan to hold 9 forums was exceeded by 38 (a total of 47 forums over three years). The goal of 1,200 attendees was passed with closer to 2,500 over the course of the last three years. The results of the self-evaluations were very positive, and the project clearly increased the basic awareness and knowledge of hydrogen technologies, as was the original plan for this project.
- The PI has been able to communicate not only the accomplishments originally set out to be achieved by this project, but he was also able to communicate the impact on the supply chain manufacturing industry in Ohio. Those are unique attributes to the state, so it is important to see that the PI was able to note those additional accomplishments as well. It provides a good understanding of the support needed to grow the fuel cell and hydrogen economy in a given state so that knowledge can be used to help grow the economy in other states and nationally.

- This project seemed to meet its goals, but the goals seemed unclear. First, using a 2004 baseline for a project that began in 2008 is problematic (the project was likely done that way because there was a 2004 survey). Next, the idea of measuring a 10% improvement in knowledge is not a very strong measurement. It would have been better to measure what existed in the state as a baseline, such as the number of suppliers (and therefore workforce) involved in fuel cells and the number of fuel cells sold, and then measure the impact of outreach. This is not necessarily the fault of the PI, because this may not have been in the scope (and arguably costs more money than may have been available).

### Question 4: Collaboration and coordination with other institutions

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

- This project team collaborated with many, many companies and organizations in Ohio and elsewhere.
- The strategy to collaborate with the manufacturing supply chain is excellent. A formal expansion to the region would be of value.
- It is great to see the PI utilizing the resources of collaborators to add to what the project was able to accomplish, such as utilizing space to save money when holding events.
- It seems like Ohio is doing a good job. There seems to be good coordination with other states doing similar efforts.
- Several industry and university partners/interactions were listed. There was a good turnout in rural areas, but a poor turnout in urban areas. The PI had fuel cell companies do some of the presentations, which adds another dimension of economic development. The project could not do webinars successfully, which is a key means of collaboration with the public.
- While the collaboration among local Ohio organizations and laboratories was good, it could have been better by reaching out to other organizations beyond just those local ones. Other national laboratories (such as the National Renewable Energy Laboratory, Argonne National Laboratory, Oak Ridge National Laboratory, and Sandia National Laboratories) could have been tapped for expertise and participation in the forums. That would have helped broaden the exposure for both Ohio and the other outside participating organizations.
- In addition to working with Ohio companies and state government, the PI has done the most in integrating with other fuel cell and hydrogen programs. The PI regularly shares ideas, processes, and expertise, and adopts what other programs do. The PI should be the poster child for how to collaborate.

### Question 5: Proposed future work

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

- A regional approach to address the full regional supply chain would be of value.
- A supply chain exchange is a key part of helping move industry forward, and it is good to hear that the success of the one held will lead to more.
- It would be great to see the PI accomplish more if more funding were available. It would be interesting to see how this work would translate if it were able to continue just outside of Ohio to the nearby region.
- The project's funding is running out. If CleanCities expanded its scope to include clean air technologies (such as stationary and off-road) or vehicles that are not yet commercially available (such as fuel cell electric vehicles and fuel cell buses), the PI could get continued financing, including applying for the new Clean Communities grant.
- Planning for the last six months of this effort includes the OFCC Supply Chain Exchange in May 2012. The PI invited the DOE sub-program manager to this event, which was deemed to be successful. An additional three forums are planned in Akron, Columbus, and Cleveland. A newsletter and compilation of survey results will also be accomplished in the time remaining until the project is over (6/30/2102). There is no goal for outreach numbers for the remaining time of this effort.
- This project should continue to embrace a larger stakeholder base beyond just Ohio, such as supply chain companies that have business in Ohio, but whose boundaries extend beyond the state. Also, providing leadership in a state-state-state-like consortium to help educate others who are potentially interested (or who should be interested) in emerging hydrogen technologies would clearly help accelerate the deployment of these technologies.

### Project strengths:

- There is a good industrial talent pool in Ohio.
- This project has a good focus on the supply chain, which is Ohio's strength, and good knowledge of what works in rural versus urban areas.
- The PI's process for staying in touch with Ohio companies is amazing and should be a model for other programs. The PI's ability to shift and change the program while still staying within the scope and the results is also a strength.
- Strengths include the high volume of forums; the supply chain exchange; and the symposium to educate, justify deployment, and improve manufacturing processes.
- A good cross section of stakeholders was targeted, including local officials, codes and standards officials, industry members, and statewide decision makers. The project held multiple forums across the state, utilizing existing planned conference/exchanges, universities, and other institutions as partners.
- The self benchmarking of this project was very good. It provided information to allow the project to improve its approach as the project evolved (i.e., the project first planned to use webinars as a communication vehicle, but as time evolved and webinars were found to be less successful than anticipated, that activity was stopped in lieu of the more successful personal outreach approach). This was nicely done.
- The knowledge of how outreach accomplished what it did translates to growing the fuel cell economy. Not only was the PI able to create great events to perform outreach, but one could see the economic impact of those efforts and how they had a growing impact on fuel cell businesses.

### Project weaknesses:

- The project is ending; this is a weakness.
- There is a lack of predictable resources for continued work.
- This project needs to take some of the findings on jobs, exports, etc. and make them available to others in the industry to continue to make the case.
- The measurement of impacts by number of people briefed is interesting, but not necessarily meaningful.
- The PI's program would benefit from someone who can get the state legislature to be more supportive.
- A low percentage of the Ohio populace was educated in this effort. Chances are there are still a lot of people that have yet to learn the fundamentals about hydrogen and fuel cells. There is no electronic means of information dissemination; webinars were used only sparingly and later abandoned as an outreach medium.
- There is a lack of information on the feedback gained. The project collected a lot of good feedback, but the results from that feedback were not reported. The project would likely have appeared even more impressive if that information was also included in the presentation.

### Recommendations for additions/deletions to project scope:

- This work should be continued.
- This project should continue as planned—great project.
- The project should figure out how to integrate with the CleanCities scope to get more funding and support.
- This project should increase its formal scope to the whole region. It should be provided with predictable funding for a two-to-three-year term. The project should develop a portfolio of technologies for increased flexibility for deployment.
- It would have been good to have better statistics on the industry in Ohio at the beginning of project, and then measure progress every two years.
- This project should continue supply chain exchange and expand to neighboring states, such as Michigan, to help the industry as a whole, as well as Ohio-based companies. More data on the supply chain would be great to make the case to Congress and the Administration that if the fuel cell industry expands, it will trickle down to help create jobs and opportunities for other companies that could supply fuel cell systems.
- There are no recommendations to be made, because the effort is almost over.

# 2012 — Market Transformation
# Summary of Annual Merit Review of the Market Transformation Sub-Program

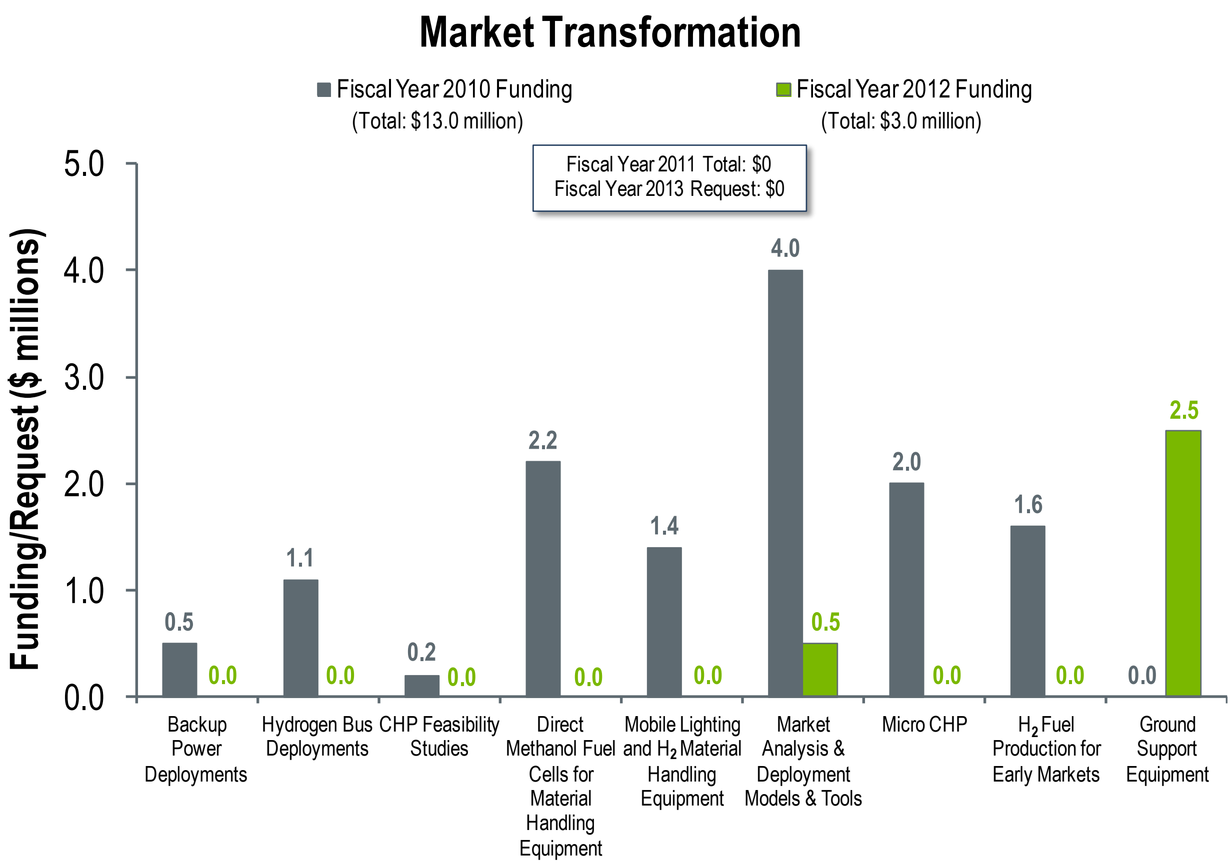
## Summary of Reviewer Comments on the Market Transformation Sub-Program:

The purpose of the Market Transformation sub-program is to spur market growth for domestically produced hydrogen and fuel cell systems. By supporting increased sales in key early markets, this sub-program helps identify and overcome non-technical barriers to commercial deployment and to reduce the life-cycle costs of fuel cell power by helping to achieve economies of scale. The current focus of the Market Transformation sub-program is to build on past successes in lift truck and emergency back-up power applications (part of the U.S. Department of Energy's [DOE's] American Recovery and Reinvestment Act of 2009 [Recovery Act] efforts) by exploring the market viability of other potential and emerging applications. Four projects were reviewed this year, and these projects are highly leveraged, with more than half of the funds provided by DOE's partners. This substantial commitment of external resources shows the high level of interest in exploring applications and markets where the hydrogen and fuel cell industry can expand and the technologies can play a valuable role.

Generally, reviewer comments on the sub-program were positive, and its activities were considered to be important to enabling the commercialization of hydrogen and fuel cells. Reviewers considered the sub-program to be well-managed and noted the extensive collaboration involved in the projects and the substantial leveraging of federal funds by cost-sharing. However, a number of reviewers felt that the Program lacks an overall cohesive market transformation strategy and that the current projects do not seem to be part of an integrated plan.

## Market Transformation Funding:

With the market successes that have been achieved by fuel cells in lift trucks and back-up power applications as a result of fiscal year (FY) 2009 and Recovery Act funding, the focus of FY 2010 funds was on new applications, such as micro combined heat and power (CHP) and specialty vehicles. As shopwn in the chart on the next page, no funding was requested in FY 2011. FY 2012 funding was leveraged by partnering with other federal agencies and stakeholders to deploy fuel cell systems in their operations. Although not reflected in the FY 2012 budget, DOE invested $43 million under the Recovery Act to enable the deployment of up to 1,000 fuel cells for early market applications such as forklifts and back-up power. The Market Transformation budget request for FY 2013 is zero.




### Majority of Reviewer Comments and Recommendations:

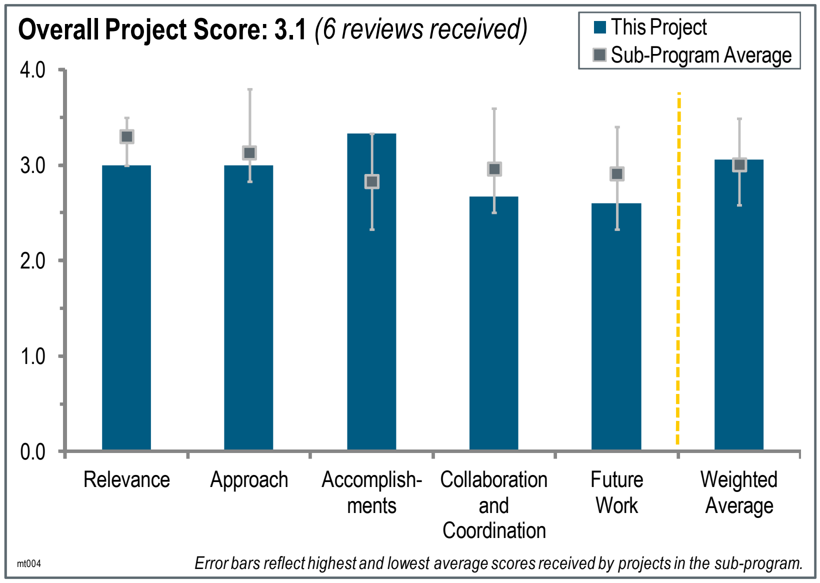
The Market Transformation sub-program's projects were rated average to high, and overall ratings ranged from 2.7 to 3.4, with an average score of 3.0. All projects were judged to be relevant to the DOE Hydrogen and Fuel Cells Program's (the Program's) activities, with good or adequate technical approaches used. Reviewers recommended that future data collected and analyzed from all deployment activities be used to develop business case reports that can be used to support further market expansion.

**Stationary Applications (Micro CHP):** One project was reviewed, with a score of 2.7. Reviewers commented that this project was clearly relevant and could help build significant market share for hydrogen and fuel cells in the near term. They also observed that this project was well designed for collecting and analyzing data. However, reviewers expressed concern that too much effort is being spent on modeling and more attention is needed on understanding the results of the fuel cell in the real world.

**Transportation and other Mobile Applications (Direct Methanol Fuel Cells for Material Handling Equipment and Hydrogen Production for Early Markets):** Three projects in this area were reviewed, with an average score of 3.1. In general, the reviewers were complimentary of the work being performed and pleased with the progress being made. While reviewers were encouraged by the relatively low cost to DOE and the high partner cost shares, they noted that the added complexity of multiple partners has caused delays on one project and schedule uncertainty on the others. Several comments were directed toward better aligning project objectives with Program goals.

# Project # MT-004: Direct Methanol Fuel Cell Material Handling Equipment Deployment

Todd Ramsden; National Renewable Energy Laboratory




## Brief Summary of Project:

The objectives of this project are to: (1) deploy and test fuel-cell-powered material handling equipment (MHE) using methanol in direct methanol fuel cells (DMFCs), and (2) compile operational data of DMFCs and validate their performance under real-world operating conditions. The longer-term objective is to help transform the market for fuel cells in material handling applications and provide information to help replicate successful deployments.

## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- It is extremely valuable to explore technologies that offer effective alternatives to hydrogen fuel cells. It is extremely useful to be able to test and operate in a real-world environment to develop the business case and make technological improvements. This type of project is also valuable for getting the word out as well as finding the “sweet” spots for further market adoption.
- The Battelle Early Fuel Cell Markets study had identified MHE as the most advantageous early market application of fuel cells because of its well-defined duty cycles, power plant size, and economics. This project can validate the results of that study as a major stepping stone to the commercialization of fuel cells for this and other applications.
- This work supports gaining experience on fuel cell operation in general; however, DMFCs only partly support the transition to a hydrogen economy.
- This project is clearly relevant to the mission; material handling in all its aspects seems to be gaining ground as a market for hydrogen fuel cells. Putting them on pallet jacks may be a “stretch,” but it had to be tried.
- Logistic-type fuels for commercial fuel cell applications are always an obstacle. This project, which uses methanol as a fuel, helps solve that issue. However, studies have shown, for autos, that methanol-fueled vehicles do not reduce $CO_2$; so, to be consistent with DOE’s $CO_2$ reduction goals, some effort should be made to show a $CO_2$ reduction for methanol in this type of vehicle. Perhaps this can be done by comparing the $CO_2$ emissions to the $CO_2$ emissions from the grid used to recharge battery vehicles.
- This project does provide a relevant application using the DMFC as a proxy for operational performance of hydrogen polymer electrolyte membrane fuel cell systems in forklift applications.

## Question 2: Approach to performing the work

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

- The project approach is well defined and understood based on several years of supporting hydrogen fuel cell forklift deployments. The one factor that could improve the project is to have several performers. By only having Oorja Protonics performing, the project is not getting the type of technical, cost, and performance-type data that are available in the hydrogen fuel cell forklift projects. This reviewer recognizes this may be a result of funding and/or market maturity, but it is a limiting factor in the project.

- Fuel cell operating data are being obtained by the fuel cell vendor, Oorja Protonics, and the National Renewable Energy Laboratory (NREL) is compiling and analyzing the data in a manner similar to what was done for the fuel cell vehicle technology validation study. Those results were well received by the research community.
- This work displays a generally straightforward approach, but some more issues could be addressed, such as the energy chain (where the methanol comes from, or the efficiency of the DMFCs), or direct comparison to hydrogen-MHE (business case, greenhouse gases). It is unclear if the operating strategy (battery charging) really adapted to the user profile, and if different operational strategies are being tested.
- This was a well-structured "sub-project"—a piece of the larger NREL pie that provides the community with useful data on a wide range of the hydrogen fuel cell markets. It seems that the project, when complete, could give a clear picture of the potential of deriving real revenue from this segment of the material handling market.
- This project has a good approach, particularly with regard to refueling.
- The testing data achieved are significant and, at 65% completion, the project is well underway to achieving completion.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project's performance and data collection and analysis seem to all be on track. An analysis should be conducted to address the emissions factor, and DMFC technology should be discussed in relation to the hydrogen fuel cell and battery technologies, which are alternatives. In addition, a very careful evaluation should be conducted to analyze the total cost. Prior work suggests that there are some assumptions that fall victim to real-world experience.
- All 75 units have been deployed, as of June 2011. Significant operating data and experience information are being obtained. A large number of methanol fills have been conducted, with no incidents being reported. Several issues have been identified (methanol concentration sensor, electronics, mixer temperature, fuel leak) and corrected. Illustrative data from "good" and "failed" stacks were reported. It is unfortunate that fuel cell degradation data, although obtained, were not reported.
- This project has made good progress concerning the deployment of MHE and data collection, but the impression is that more information and more conclusions on DMFC could be drawn from the project. Data evaluation and results presentation could be more systematic and clear.
- This work met the goal of installing DMFCs on 75 Class III forklifts. It displayed good tracking of operation and "events" (maintenance and other). So far, very useful data is being produced.
- This project produced lots of real-life data, but it needs more information on stack life and costs. This reviewer particularly liked the chart that shows the issues, approaches to solving the issues, and the results.
- The progress seems valuable and significant progress has been made. The demonstration clearly establishes a forum for determining performance and operation and maintenance issues. It is noteworthy that the project team (Oorja Protonics) made system improvements during the program—this showed commitment to the goal of producing a viable product and not just taking data.

### Question 4: Collaboration and coordination with other institutions

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

- This project has a good lineup of industry and government involvement. Having three different users is expanding collaboration and helping with the education and understanding in a wider population.
- Although collaborations with Oorja Protonics and the demonstration host sites were listed, this one being a DMFC project, there were no interactions listed with other fuel cell developers, even for similar applications.
- The collaboration seems limited to the strictly necessary work. Enhanced collaboration could increase the benefit of the project.
- This work has a solid relationship with Oorja Protonics and the three deployment sites.
- This project needs more input, in words, from customers who were using the equipment.
- It was good to see significant industry and private-sector collaboration among Oorja Protonics and the food distributor end users.

### Question 5: Proposed future work

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

- The demonstration will be continued to its normal termination. Future activities will include maintenance and reliability analysis. Future work will also include an analysis of the cost of ownership.
- The main intention of the project should be explained more clearly.
- The team seems very well focused on wrapping this project up in a manner that will result in very useful data.
- This work is reasonable, but it needs more information on cost assumptions and life-cycle cost.
- There does not appear to be a plan for reuse after demonstration, though the refueling systems and some portion of the 75 units should be able to continue service.

### Project strengths:

- The project appears to be well executed.
- This project has a pragmatic approach, real-world application, and conducts testing under challenging conditions.
- This project has good planning, a good team, and good execution. This reviewer is looking forward to seeing the final wrap-up.
- This project demonstrates several allocations with real operating information and real refueling experiences.
- This work demonstrates a significant number of units in a real-world environment.

### Project weaknesses:

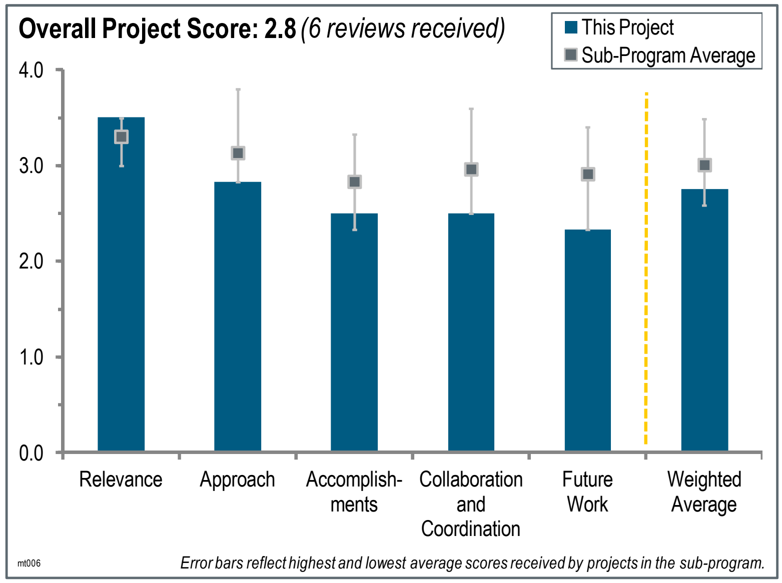
- The project needs more information on life and costs.
- Some of the results are not being released due to proprietary considerations. These include fuel cell degradation data, which is unfortunate. It is not clear if similar restrictions will also apply to the results of the analyses listed for future work.
- This work has an incomplete approach; many additional questions could have been addressed with the same budget. DMFC as an auxiliary path to the hydrogen economy should be compared directly with hydrogen MHE, including through an environmental impact study.
- One reviewer would like to have seen a bit more focus on classic technology readiness level analysis. Given the relatively large number of maintenance events, this reviewer would like to know how close DMFCs are to being really ready for prime time.
- Another reviewer felt there were no weaknesses.

### Recommendations for additions/deletions to project scope:

- The project should add a direct comparison of cost und well-to-wheels efficiency of DMFC and hydrogen fuel cells. This reviewer also suggests that the project test different operational strategies (such as battery charging or permanent fuel cell operation) and their effects on durability and fuel economy, and study the impact of methanol use on the environment.
- While manufacturers and owners of Class III forklifts are important, and their input seems to have been taken into account, in the end, the users are the key to product acceptance. If possible, it would be useful to collect data from this level on whether the benefits of refueling versus changing batteries outweigh adding a great big box on top of the battery pack.
- The project should maintain more information on costs, life, and customer experiences. The project also needs more information on the details of the fuel cell. This reviewer also suggests that the team carefully record the impurity levels in methanol and monitor air quality around units and emissions from the units, particularly trace organics.
- Another reviewer had no specific recommendations.

## Project # MT-006: Fuel Cell Combined Heat and Power Industrial Demonstration

Dale King; Pacific Northwest National Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) demonstrate combined heat and power (CHP) fuel cell systems, (2) objectively assess their performance, and (3) analyze their market viability in commercial buildings. Fuel cell system durability, efficiency, production, and economics will be evaluated against stated manufacturer specifications. Commercialization "bottlenecks" will be identified to determine where industry needs to apply the greatest effort to achieve high market penetration.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This Pacific Northwest National Laboratory (PNNL) project fully meets DOE research, development, and demonstration objectives and fully supports the eventual commercial development of stationary fuel cells. While the project is necessary, this judgment is based more on knowledge of the industry rather than the presentation itself. As presented, an observer might not get the same impression.
- This project aligns with the goals of the DOE Hydrogen and Fuel Cells Program (the Program). Demonstration of the real-world performance of "commercially ready systems" is key to validating the cost, reliability, and durability of the technology. Furthermore, operating data offers a resource to assess the cost/benefit of any DOE research and development funds spent to develop the demonstrated fuel cell system.
- This project is highly relevant to the objectives of the Program, especially when taking into consideration that CHP systems are likely to become commercialized early and on a larger scale than other fuel cell technologies.
- The project deploys "commercially available" fuel cell CHP systems in a wide variety of applications and geographical locations. Real-world cost, performance, and durability/reliability data are being obtained.
- This project is clearly relevant to the broad goals of the Program.
- Small-scale CHP is a critical application for fuel cells.

### Question 2: Approach to performing the work

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

- The fit of fuel cell CHP for a given application is assessed by comparing heat and power requirements to what is provided by the fuel cell system. Assessment of fuel cell types includes polymer electrolyte membrane (PEM) and solid oxide fuel cell (SOFC). The project compares manufacturer claims with field data. Data collection and reporting are comprehensive and understandable.
- The presentation had no real discussion of critical barriers; it was more a presentation of data than a presentation of the program. In short, the grading of this program and its presentation could have been better if the project, and/or the report of the project, had focused more on programmatic issues than a report of observations.
- The approach is an excellent one—enlisting manufacturers and users in a comprehensive study of what it takes to get hydrogen fuel cells out into the real world. Long-term detailed monitoring of site performance may be "overkill," but in the end it will provide traceability of both good and bad performance.

- This project team spent too much time modeling and explaining the modeling; more time should be spent on understanding the fuel cell real-world results. Applications should be chosen where the building uses all the fuel cell electrical output and does not count on selling into the grid. Selling into the grid varies by state and at any time the local public service commission could change the rules.
- This project is well designed to collect operating data. Its creation of the small and large building model offers a reference to compare real operational data with the expected needs of buildings.
- The approach is well thought through and effective. Including a timeline in the “Approach” slide would be useful for monitoring progress in the future.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- So far, the deployments include PEM fuel cells from only one supplier. It would be interesting and instructive to include deployment of other fuel cell manufacturers and other types of fuel cells such as SOFC.
- The amount of data clearly indicates that PNNL focused on the objectives, but an absence of discussion on the barriers leaves too many questions. It is not clear to this reviewer what challenges existed in accumulating the data, if any.
- The amount of work that has been done on the project is truly impressive, but the “Overview” slide is very confusing. This reviewer questions how the project can be 33% complete, with 100% of the money spent. Clearly there is more work to be done, since the group says so in the “Future Work” section, but to imply that two thirds of the project is incomplete at this point seems very wrong. If it is really correct, then “Accomplishments and Progress” should be ranked as “poor!” The modeling work was good. A direct comparison with data from “large offices” (e.g., college, school, medical center) and “small offices” (e.g., grocery, laundry shelter) should be attempted. The cost analysis was particularly illuminating, though depressing—it is unclear how long subsidies of 50% will be required.
- The project has progressed from 5% (2011) to 33% (2012). Protocols for recording and analyzing data are in place. The remaining success in obtaining significant data for analysis will largely depend on the performance and availability of the fuel cell system.
- System acquisitions appear not to have been completed yet. Fifteen units in operation represents 40% of the total number of systems originally planned (38). It is not clear whether more systems will be coming online. System monitoring appears to be going well. The downward trend in the performance of virtually all units is worrisome; an average power loss of 20% over approximately 3,500 hours of operation is very high.
- The presentation was so poor that this reviewer could not tell what was accomplished.

### Question 4: Collaboration and coordination with other institutions

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

- The discussion was too short on this critical review area. Except for an obligatory slide listing some partners, there was zero discussion about what anybody else did. In short, this project is apparently a PNNL-only affair.
- The group seems to have been able to enlist a large number of participants on the “user” side, and it is doing better on the supplier side. This reviewer would still like to see more fuel cell suppliers onboard.
- Four partner sites are adequate for diversity of operational data for the 15 fuel cell systems. Future presentations should comment on external site factors (non-fuel-cell-system factors) that impacted the fuel cell systems’ operation or data (e.g., utility outages, site maintenance), as these are real-world factors.
- Collaboration with manufacturers and customers is good. Partners with expertise in fuel cell stack and system performance diagnostics may be needed soon, given severe performance losses to date.
- The presentation needs more words and opinions from the supplier and from the host sites.
- Collaborations and coordination are, appropriately, with fuel cell suppliers and deployment sites.

### Question 5: Proposed future work

This project was rated **2.3** for its proposed future work.

- The project's score continues to suffer because the presentation focused on data delivery rather than a complete review of programmatic issues. This reviewer does not recollect any discussion of future improvements.
- The presentation offered a limited definition of proposed future work at this time. If future sites and systems are added, the selection should be based on expanding the type and operating environments.
- Proposed future work concentrates almost entirely on data acquisition and analysis monitoring. More focus on understanding the causes of performance degradation is needed, including advanced diagnostics of individual cells in fuel cell stacks.
- The presentation needed more information on life and costs.
- The effort to broaden participation of other fuel cell suppliers and sites continues. The discussion is somewhat vague.

### Project strengths:

- The installation of 15 units in the fourth quarter of fiscal year 2011 was an accomplishment. Performance monitoring has already yielded interesting data and pointed to possible durability issues with the systems. Data collection, analysis, and reporting are strong.
- This project's significant population (15) of fuel cell systems and five-year project duration offers a good base of data.
- This project shows a tremendous amount of data delivery, and the presenter was clearly in an area of strength.
- This project has a good handle on performance data and expected results.
- This project has a good variety of applications and a good choice of technology.

### Project weaknesses:

- Having only one original equipment manufacturer limits the variability of systems and the relative performance of the PEM unit.
- The lack of diagnostics of the causes of performance loss is a weakness—especially in the context of a prohibitively fast performance loss of already installed systems.
- It is important to include other fuel cell types and systems from additional suppliers.
- The presentation of the project was almost a book-report-type presentation—a lot of discussion of what, but no discussion of what was, what will be, what could be, what should be, why, who, or what is next.
- The project's management approach seems a bit diffuse.
- The project's results were very poorly presented.

### Recommendations for additions/deletions to project scope:

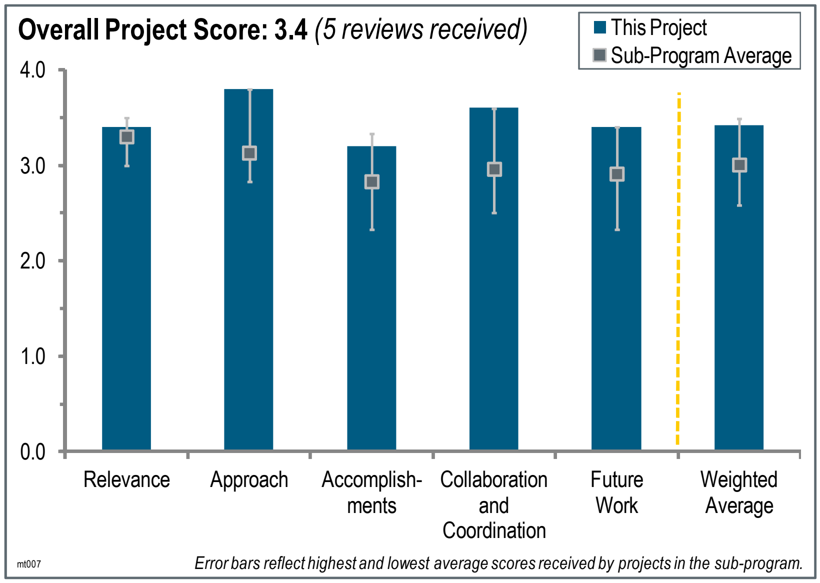
- A clearer picture of the funding for this project, and how it is being used, would be very helpful in evaluating the bottom line. The project still needs to enlist more than one fuel cell maker. The modeling work was good. A direct comparison with data from "large offices" (e.g., college, school, medical center) and "small offices" should be attempted.
- Provide less information on the model and more information on operating results and life-cycle costs, including capital, today or projected, and maintenance.
- The reasons for performance degradation should be identified, which will likely require monitoring of individual cells in stacks. Maximum acceptable performance loss should be established. If performance loss of the 15 installed systems continues to be a problem, units from alternative sources should be acquired and deployed as soon as possible.
- The presenters should take a better look at what they present during a review and the criteria to be judged against. The PNNL work is too important for necessary elements to be overlooked (or ignored).
- This reviewer did not have any recommendations to add or delete to the project scope.

# Project # MT-007: Landfill Gas-to-Hydrogen

Shannon Baxter-Clemmons; South Carolina Hydrogen and Fuel Cell Alliance

## Brief Summary of Project:

The objectives of this project are to: (1) validate the business case and technical feasibility of using landfill gas (LFG) as a "distributed generation" option for hydrogen production, and (2) transfer "lessons learned" that may be applicable for other candidate waste streams. Commercially available equipment will be surveyed to determine the economic viability of the LFG-to-hydrogen approach for potential end users, and the technical viability of current systems to produce sufficiently pure hydrogen for use in motive or other applications will be demonstrated.




## Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- This project is spot-on in that it is focused on market transformation rather than a science project. Over the course of the presentation, it became clear that the project's initiative was to truly advance the marketplace for fuel cell technology.
- This project has the objective of generating renewable hydrogen from LFG, and using this hydrogen to fuel the fuel cell material handling equipment (MHE)—i.e., forklift trucks, with commercially obtained equipment and components. Further, the project is being hosted by a large company, BMW, which would be interested in scaling up the project for its commercial use once the concept is proven at the project scale and a viable business case can be established. It was stated that the LFG source should be good for more than 20 years. This is a very worthwhile market transformation project; it has all the right ingredients.
- This project has a good future perspective and a big field of application, including a pilot implementation that can be spread to many other locations afterwards.
- Establishing LFG as a source of hydrogen is not really in the "mainline" of the DOE Hydrogen and Fuel Cells Program, but it certainly does support the production of hydrogen from renewable resources.
- This project uses LFG to generate on-site hydrogen for forklifts.

## Question 2: Approach to performing the work

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

- This was one of the best planned, best presented projects this reviewer saw at the 2012 DOE Hydrogen and Fuel Cells Program Annual Merit Review. From selecting the "perfect site" (a plant that uses hydrogen MHE and is located virtually on top of a landfill) to working hand-in-hand with a very influential partner, South Carolina Research Authority (SCRA), the project team has done a superb job. The team showed great foresight and flexibility to adapt to changes out of the control of the immediate project team. The project had good focus on putting together a "real-world" example of exactly what the project is supposed to accomplish—demonstrating the utility of LFG-to- hydrogen production to serve the needs of an industrial user.
- The project began with a feasibility study and a business case analysis, which were completed in four months. From the beginning, the potential customer for the technology, BMW, was involved in settling the parameters for

the study, and it had a lead role in approving the project to proceed to Phase II. The project is currently in Phase II, conversion of LFG to hydrogen, where the purity of the product hydrogen will be monitored for two months to determine the variability in the process over this extended period. The final phase will be to conduct side-by-side trials of MHE fueled by the LFG-derived hydrogen and the current, commercially sourced hydrogen. This should validate the technical approach with a high degree of confidence in the results of the project.
- The project takes advantage of existing LFG infrastructure at BMW for the demonstration. BMW has direct experience with hydrogen fuel cell forklifts. However, the discussion of hydrogen purity requirements is lacking. This project builds a system from known technology components so no new science or breakthroughs are required.
- This project displays a very stringent approach that is focused on a feasibility study and business case analysis. With this approach, decision makers can easily evaluate if this is an option for their application. Limitation to commercially available technology limits the technological risk for implementation.
- This work is clearly and directly focused on meeting the goals of market transformation, not to keep working, but to transform business opportunities.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The major accomplishment to date is the completion of the feasibility study, which showed that at a large enough scale, perhaps 500 kg $H_2$/day, the LFG-derived hydrogen would be cost competitive with industrial gas hydrogen, based on a 10-year analysis. Longer analysis periods and larger scales of production would further reduce the cost of LFG hydrogen. In other progress, it was reported that the LFG gas clean-up skid has been built and tested by team member GTI, and that it will be shipped to the BMW host site shortly.
- This project had good results from the feasibility study, which will provide a solid basis for future decision making on further steps.
- This project seems to be on track, and it is likely to meet the July milestone. Considering the relatively low cost and high (50%) "leverage," this reviewer recommends funding Phase III of this project if this milestone is met.
- The progress of this work is difficult to gauge based on the information provided. It is not entirely clear if the project is on schedule.
- This project displays clear and definitive progress toward implementing a useful product.

### Question 4: Collaboration and coordination with other institutions

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

- This project is clearly partnering with BMW; BMW is not just listed as a project participant but is a focal point of the project. Other principal investigators should take a look at how SCRA partnered with BMW.
- It would be good to have the MHE supplier on the team. Otherwise, all expertise is present. BMW involvement as the site host is advantageous.
- This work displays very good communication with project partners, which was essential for project success because partners had to be convinced of the business case in order to proceed with the next phase.
- This reviewer was impressed with the way the project brought BMW on board, and pulled together the rest of the team.
- The project team includes industry and not-for-profit organizations.

### Question 5: Proposed future work

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

- SCRA fully meets the definition of "Outstanding"; the future work will continue to execute the project as planned, which is the appropriate path to follow.
- The project is taking further steps based on the existing hydrogen-MHE infrastructure, and the new hydrogen source will be incorporated into the system. A comparison between different hydrogen sources will be of great value.

- Future work plans are logical and consistent with the project timeline.

### Project strengths:

- This work had quite a number of strengths, but two stood out: (1) SCRA's partnering with BMW indicated that the two entities were very much together in this project and (2) the focus on achieving a fundamental transformation, rather than on advancing a single narrow objective that would hamper real progress.
- This project has a multifaceted, well-coordinated project team and strong involvement from BMW, the host organization. The presentation displayed a good understanding and discussion of the critical issues, as given in the reviewer-only slides.
- This work is combining a new hydrogen source with an existing hydrogen infrastructure, which will be a reference for future projects.
- The project's strengths include excellent planning, a tight focus on execution, an impressive team, and a convincing business model.

### Project weaknesses:

- The project's potential problem is in getting a valid "side-by-side" comparison of locally produced hydrogen with trucked-in gas. This reviewer questioned if the plan is now to pipe the reformed gas to the outdoor refueling site.
- One reviewer felt that no significant weaknesses were evident.
- Another reviewer felt there were no weaknesses.

### Recommendations for additions/deletions to project scope:

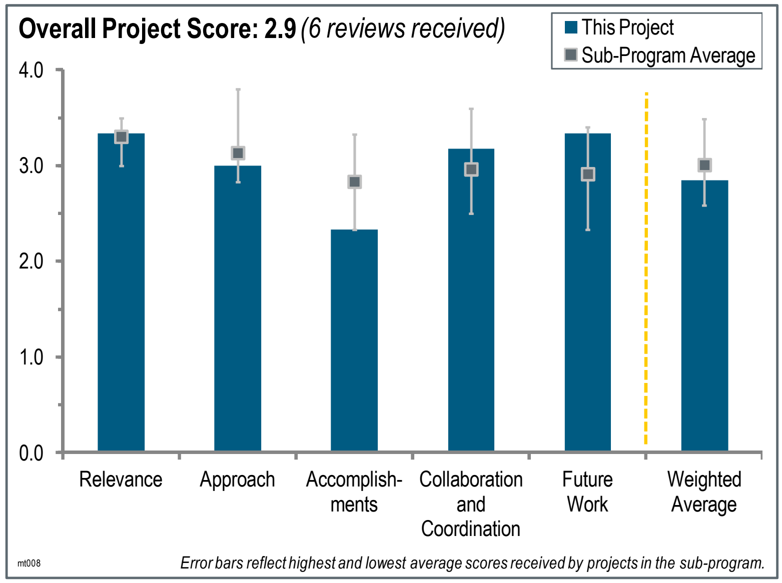
- One reviewer recommended extending the project to study the effects of landfill hydrogen not only on MHE fuel cells, but also on the existing refueling infrastructure.
- Another reviewer recommended funding Phase III if the July milestone is met.
- A third reviewer did not have any recommendations and commended the team on a good and complete job.

## Project # MT-008: Hydrogen Energy Systems as a Grid Management Tool

Mitch Ewan; Hawaii Natural Energy Institute

### Brief Summary of Project:

The objective of this project is to evolve energy systems through a four-step process of: (1) developing and validating rigorous analytic models for electricity and transportation, (2) developing and modeling scenarios for deployment of new energy systems including additional renewable energy systems, (3) identifying and analyzing mitigating technologies to address systems integration (grid stability) and institutional issues, and (4) conducting testing and evaluations to validate potential solutions to facilitate utility acceptance.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The concept provides a way to combine and levelize renewable energy (geothermal and wind) through electrolysis hydrogen production. The current project is to use the hydrogen generated by hydroelectric hydrolysis to power vehicles. The hydrogen could also be used for grid leveling through fuel cells. The electrolyzer could also be used for grid management by reducing electrolyzer load when the renewable source (such as wind) is in a lull.
- The activities in this project are to: (1) demonstrate the use of a dynamically responsive electrolyzer and to characterize its performance, ostensibly when it is hooked up to a variable source, such us a wind turbine; (2) provide hydrogen to shuttle buses; and (3) conduct performance/cost analyses to assess the benefits of the integrated system, including grid services and off-grid revenue streams. None of these activities appears to be in direct support of DOE's Fuel Cell Technologies Program.
- This is the perfect opportunity to demonstrate the integrated energy system of the future. If this project is a success, it will serve as an example for larger-scale applications.
- The integration with other renewable/non-polluting sources of energy is of importance to the acceptance of hydrogen power technology. This project addresses key issues by creating and monitoring real-world deployments.
- This project covers a variety of DOE Hydrogen and Fuel Cells Program technologies and objectives, electrolyzers, buses, grid interaction, and fuel cells. The project's goals are ambitious.
- This work is extremely relevant to advancing a hydrogen economy, an effort necessary to tie a variety of initiatives into accomplishing a useful end result.

### Question 2: Approach to performing the work

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

- The discussion on slides 6 to 8 refers to grid management to mitigate the negative impacts of intermittent renewable energy sources.
- As far as the technical approach goes, co-locating electrolyzers and fuel cells with renewable (wind, geothermal, solar) sources on-site is one way to maximize efficiency. This project is well focused on fostering acceptance of

hydrogen power technology. As far as the management approach, planning for the project could have been much better—some of the delays might have been foreseen.
- The approach is system orientated. Every step takes into account the overall objective. The approach could possibly be more focused to optimize the use of limited project resources.
- The approach utilizes renewable geothermal energy to produce hydrogen for fueling hydrogen-powered fuel cell electric vehicles (FCEVs). The plan is logical and methodical and does not require technical breakthroughs.
- This is a comprehensive, focused project that identified a number of critical challenges and investigated means to achieve a successful result.
- The approach is good; however, given the number of partners and parties involved, it is not surprising that there have been delays in getting actions initiated.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.3** for its accomplishments and progress.

- Of the eight accomplishments listed on slide 21, four are administrative activities—contract awards, started (but not completed) environmental assessment, memorandum of agreement with Puna Geothermal Ventures 95% complete; two are vague—developing site design and replaced Ford buses with an El Dorado bus; and two are lining up additional funds. It is difficult to accept any of these as significant accomplishments. The presentation at the 2011 DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) and the current presentation at the 2012 AMR were not all that different.
- This project has made good progress toward the objectives; the modifications on the way (e.g., switching from hydrogen internal combustion engines to FCEVs) are consistent, but it has had delays due to authorities outside the project.
- Some pieces are still needed to bring this work together. Completing tasks 4 through 7 in 2012 seems aggressive; however, the project is worthy of continuing its effort to fulfill demonstration.
- Progress on this project has been slow, primarily because of non-technical issues such as liability assignment and partner negotiation. A no-cost extension is under consideration.
- This work has achieved tremendous results specific to the project and in the team’s overall efforts.
- This project is seriously behind schedule.

### Question 4: Collaboration and coordination with other institutions

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

- Most of the relevant entities are (or will be) engaged, including a geothermal energy producer and a hydrogen fuel cell bus owner/operator. The performance and durability of the electrolyzer for this dynamic service seems to be unverified. This is a key part of the system concept. The project is well coordinated and depends on other related projects.
- While Hawaii Natural Energy Institute (HNEI) alone was the presenter of the project, it is apparent that HNEI also represents a well integrated and complete team effort by an organization with true partners; HNEI and their partners were together every step of the way.
- Collaborations listed on slide 22 include five that are either the principal investigator’s (PI’s) own organization or the project sponsors or managers. These only faintly qualify as collaborators in the technical sense. An additional one is listed as an “Interested Observer,” which is vague and not very informative.
- Good collaboration is essential in a project such as this, but delays are due mainly to coordination and collaboration issues. Nevertheless, this reviewer gives the project a good rating because in this project the coordination with authorities seems to be difficult.
- This project has shown excellent “leveraging” of DOE funding and outstanding integration with local government and commercial partners.
- This project has displayed a high degree of collaboration among federal, state, and private entities. Given the complexity of getting all of the pieces together, the success to date is admirable.

### Question 5: Proposed future work

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

- The proposed future work is still the bulk of the project. However, with the addition of significant new financial resources, and the painful lessons learned in the first two years of the project, the team seems to have a better handle on how to do the job. The team should pay closer attention to project milestones, and in particular, the establishment of "backup" plans that can help overcome potential problems.
- The proposed future work to collect data from the various systems is promising. All will be predicated on the completion of the various tasks to get systems online.
- This is a complete and comprehensive project that presents a step-by-step approach to achieving meaningful goals to advance a hydrogen economy.
- The proposed work shown on slide 23 appears to be consistent with what will be needed to carry out the project.
- The proposed future work is very consistent with the overall project goals.
- The plan is logical and appropriate.

### Project strengths:

- This project shows on a small scale what can be the energy grid of the future. Showing the alternative options of using hydrogen is another strength.
- This project displays an excellent understanding of how to work in the unique local environment and shows good project "vision."
- This is a complete and overall worthy project to accomplish serious goals. The PI is highly enthusiastic.
- The strength of this work is the diversity of its activities.

### Project weaknesses:

- Extended time for permitting, etc., should have been foreseen. The presentation had a lack of clarity on the budget. On the "Summary" slide, the "total project funding" was listed as $1,796,515, yet in the same quadrant, fiscal year 2011 funding in the amount of $2.6 million was listed from various sources. Apparently, the $1,796,515 is just the DOE share, because it is approximately (but not exactly) the same as the DOE budget listed in the 2011 presentation. This seems like a minor point, but it calls into question the ability of the team to focus on the bottom line—despite its very impressive ability to leverage DOE funding.
- The electrolyzer performance has not been established for sustained cyclic operation, and the electrolyzer supplier does not seem to be a formal partner. The source of water for the electrolyzer is not identified. Operation near a volcano could have a negative impact.
- It is not clear how the project addresses "Barrier J, renewable electricity generation integration," which is, presumably, meant for variable energy sources, such as wind or solar. It is not clear just which non-technical issues preventing full commercialization of hydrogen are being addressed by this project.
- This is a very ambitious project with a small budget. Only one option of hydrogen use will be demonstrated. The mobile refueler solution (small trailer) is only applicable to very special cases such as this; it is pragmatic, but without relevance for larger-scale applications.
- This project's weakness is its complex administrative setup before real-world demonstrations can begin.
- One reviewer felt there were no weaknesses.

### Recommendations for additions/deletions to project scope:

- One reviewer recommends adding projection for future extensions; for example, feeding electricity back to the grid and implementing a real vehicle refueling infrastructure with permanent refueling stations. This reviewer also recommends adding a study for transferring the results to bigger grids.
- Another reviewer recommends paying closer attention to project milestones, and establishing "backup" plans that can help overcome potential problems.

- A third reviewer recommends establishing electrolyzer suitability for the intended service.
- A fourth reviewer found it difficult to offer any specific recommendations, because the technical or market transformation objectives of the project are not very clear.
- Two reviewers did not recommend any changes.

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# 2012 — Systems Analysis
# Summary of Annual Merit Review of the Systems Analysis Sub-Program

## Summary of Reviewer Comments on the Systems Analysis Sub-Program:

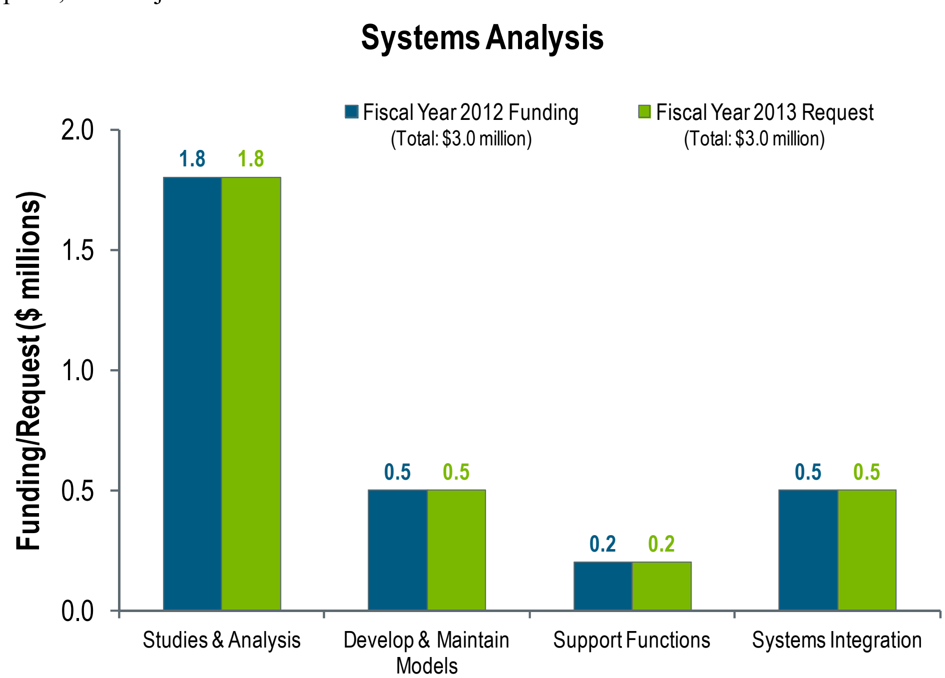
The reviewers considered the Systems Analysis sub-program to be an essential component of the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program's (the Program's) mission. The projects were considered to be appropriately diverse and focused on addressing technical barriers and meeting targets. In general, the reviewers noted that the Systems Analysis sub-program is well managed and demonstrated the ability to address immediate analytical needs such as analyzing the impacts of the substantive drop in natural gas prices and the greenhouse gas emissions of new natural gas production ("hydro-fracturing") processes.

Some reviewers commented that the sub-program is effective in providing analytical support and key insights for the Program's research and development (R&D) efforts and that it is helpful in appropriately directing R&D efforts to address key barriers. Reviewers also commented that the analysis and model portfolio was making good progress toward understanding the issues, challenges, and opportunities to achieve the Program's technical targets. Some reviewers commented that the sub-program's focus was good, considering the limited resources.

Key recommendations for this sub-program included the following: (1) conduct a dynamic analysis of intermittent renewable electricity (primarily wind and solar) compared to the dynamic electricity load in a given region to quantify benefits of hydrogen storage, (2) develop modeling pathways for hydrogen infrastructure cost to achieve the $2–$4/gge hydrogen threshold cost, and (3) analyze high temperature polymer electrolyte membrane (PEM) fuel cells.

## Systems Analysis Funding:

The FY 2012 appropriation for the Systems Analysis sub-program was $3 million. Funding for the sub-program continues to focus on conducting analysis using the models developed by the sub-program. In particular, analysis projects are concentrated on infrastructure development for early market fuel cell introduction, the use of hydrogen and fuel cells for energy storage, employment impacts of manufacturing and installation of fuel cells, and the petroleum and greenhouse gas emission reduction benefits of the Program's technology portfolio. The FY 2013 request-level of $3 million, subject to congressional appropriation, provides greater emphasis on analysis of hydrogen for energy storage and transmission, early market adoption of fuel cells, biogas resources, and other impacts, such as job creation.



## Majority of Reviewer Comments and Recommendations:

The maximum, minimum, and average scores for the Systems Analysis projects were 3.8, 2.7, and 3.1, respectively.

**Infrastructure:** The analysis projects reviewed in this topic area were rated favorably for assessing the costs of hydrogen infrastructure development and understanding the hydrogen infrastructure costs compared to other alternative vehicle infrastructure. Reviewers acknowledged the insights gained from a wide array of stakeholders. Suggested next steps included the following: document the results and key findings, calibrate the findings with key stakeholders and other studies, and expand the analysis projects to a more comprehensive and integrated study of vehicle/infrastructure rollout.

**Model Development and Systems Integration:** One project involving model development for assessment of jobs impact was reviewed and received a score of 3.6. This project received very favorable reviews and was regarded as well aligned with the current program goals and objectives. Reviewers commented that the model provides valuable economic and job creation information for project funding justification. Reviewers recommended that the model be expanded to include assessment of job impacts for infrastructure construction and the model continue to be validated with original equipment manufacturers (OEMs) and industry to improve the model's credibility.

**Programmatic Benefits Analysis:** The reviewers commented that the analysis project to assess the Program's benefits (in terms of reducing greenhouse gas emissions and petroleum use) is relevant to the Program's objectives and provides valuable projections of the impact of fuel cell electric vehicles (FCEVs) and hydrogen in the U.S. transportation mix.

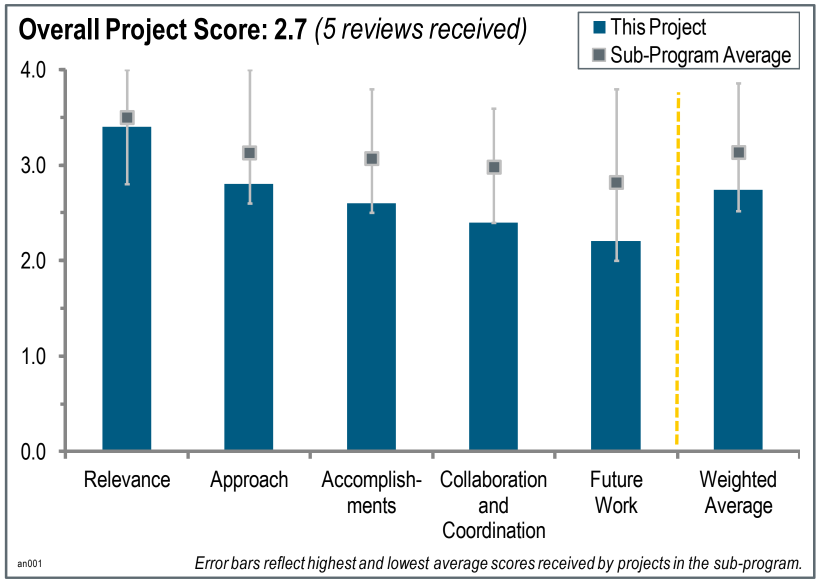
**Resource Analysis:** This project received a favorable review for assessing resources for hydrogen production. Reviewers specifically appreciated the insights the analysis provides about the impact renewable hydrogen production would have on the renewable resources, and they commented that the presentation of the geographic data is very powerful and makes the model results very useful. Suggested next steps included: expand the analysis beyond the light duty vehicle fleet, include regional variations, and expand the analysis collaboration to industry stakeholders.

**Scenario Analysis:** These analysis projects examine the potential market penetration of FCEVs compared with other alternative vehicles and include sensitivity to key program variables such as the costs of fuel cells, hydrogen production, and hydrogen storage. Also, the impact of infrastructure availability and consumer choice was examined. The reviewers felt that the projects successfully showed the benefits of subsidies and compared FCEVs to other technologies. Reviewers suggested future studies recognize the compliance to U.S. regulatory standards such as CAFE and include input from automobile OEMs.

**Studies and Analysis:** Ten analysis projects were reviewed, with an average score of 3.1. In general, the reviewers felt that the projects supported Program goals, but they also agreed that the analysis projects need to perform the following activities: (1) involve more collaboration with industry to calibrate information with actual operation and experience for some of the projects, (2) be peer reviewed prior to issue and publication, (3) be more inclusive, and (4) use a consistent set of inputs and assumptions.

## Project # AN-001: Infrastructure Analysis of Early Market Transition of Fuel Cell Vehicles

Brian Bush; National Renewable Energy Laboratory




### Brief Summary of Project:

The objectives of this project are to use Scenario Evaluation, Regionalization, and Analysis (SERA)—a suite of tools for studying the cost implications of regional build-outs of renewable energy infrastructures—to: (1) generate self-consistent vehicle adoption and hydrogen demand scenarios relevant to the early market transition of fuel cell electric vehicles (FCEVs); (2) determine optimal regional infrastructure development patterns for $H_2$, given resource availability and technology cost; (3) geospatially and temporally resolve the expansion of production, transmission, and distribution infrastructure components; and (4) identify niches and synergies related to refueling station placement and early FCEV adoption areas.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- Conceptually, this is outstanding, but there are too many limitations associated with the many assumptions and the lack of inclusion of carbon capture and sequestration (CCS).
- This project will generate a cost model for infrastructure costs. A model of this type is needed to help DOE direct development. However, the fidelity of the model is only as good as the assumptions. The end use of the tool should be a key component in its development.
- Having the SERA model investigate approaches and issues related to the regional development of $H_2$ infrastructure is valuable to the DOE Hydrogen and Fuel Cells Program. Focus on this approach should continue.
- This project is an attempt to assess the regional build-out of $H_2$ fueling stations using data from various sources of projected FCEV introductions. Another objective of the project is to examine which $H_2$ production and delivery pathways would provide fuel at the lowest cost for a given demand level.

### Question 2: Approach to performing the work

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

- This project has too many questionable assumptions.
- It is good that the SERA model integrates assumptions and data from multiple sources and related modeling efforts. However, with so many components and related assumptions involved, ensuring consistency, accuracy, and other factors becomes harder to control and of more importance.
- The approach is well designed to date, but it lacks regional and political reality. For example, truck transport within the major eastern cities may not be as attractive as the presentation shows due to the availability of distributed natural gas for distributed generation, congestion of the electrical grids and the desire of the electric utilities to increase non-peak base loads, and transportation restrictions for hazardous materials through heavily populated areas.

- The researchers need to coordinate with others such as the Strategic Analysis, Inc. (SA, formerly Directed Technologies, Inc.) work that showed that on-site steam methane reformers would dominate the choice of fueling option for many years if one chose the least costly option. The dominance of pipeline $H_2$ in slide 14 is striking, although the principal investigator said that that slide should have been labeled 2050; showing an earlier year might be instructive. The researchers also need to fold in the projected costs of $H_2$ from Air Products; they estimated costs of $4.50/kg–$5/kg for pipeline $H_2$ (see TV-007). Air Products also estimated costs between $3/kg and $5/kg for renewable $H_2$ from a wastewater treatment plant (see TV-006), compared to the results here of costs never dipping below $6/kg despite the preponderance of pipeline $H_2$.
- While the cost components of the project are consistent with the Hydrogen Analysis (H2A) models and other sources, such as the U.S. Energy Information Administration Annual Energy Outlook's energy price forecasts, the fueling station build-out is based on fuel cell vehicle rollout scenarios from the National Academy of Sciences (NAS) analysis. The level of confidence in those scenarios is relatively low, as admitted by the project itself through the downward adjustment of the rollout by 50% for the early years (but matching the high penetration of the NAS study in later years). Even the 50% reduction is not substantiated by any data, except to say that it is based on the California short-term estimates. In the absence of stated plans by original equipment manufacturers (OEMs) to introduce FCEVs in the United States by 2015, the new FCEV introductions shown on slide 19 appear to be overly optimistic. Because the demand for $H_2$ is directly related to the numbers of FCEVs introduced into the automotive fleet, the results of the fueling station build-out will have a low confidence level. The project also uses the geographical distributions of hybrid electric vehicles (HEVs) by postal zip code to project FCEV distribution. The growth of HEVs in the United States has slowed, if not stopped, suggesting that FCEVs are not likely to find customers in similar numbers. With the shift in focus by the OEMs for the U.S. market toward plug-in hybrid electric vehicles, at least for the near- to mid-term, the projected numbers of FCEVs are not likely to be realized over the analyzed time period.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- Progress toward DOE goals is questionable at best because the long-term cost of $H_2$ is above $6/kg, which is double DOE's targets.
- The accomplishments and process are as expected. A review of the assumptions to reflect a more complicated reality may be in order. The fuel targets, if accurate, would indicate $H_2$ is not a viable fuel. However, current non-taxed $H_2$ is often less than the $8/kg price quoted.
- Adjustments made to the nationwide scenarios based on the California plans are a good start, but California tends to be unique compared to other national approaches and initiatives. It would also be good to compare to other regional plans. In the future, as these regional plans are implemented, it would be good to validate model findings with real-world results. The researchers should be better able to determine which consumers and areas should be considered for refueling station placement as the modeling utilizes consumer preference research more (as was indicated in plans for future work). Sensitivity analyses for availability and the prices of different feedstocks should be considered. The graph presented showing the optimal choice of production technology displayed the influence of low natural gas costs. Natural gas costs are at historical lows today, but they may not be in the future years analyzed by this model. Also, central coal gasification is shown as preferred due to economies of scale, but changes in the regulatory scene for coal plants might completely alter the picture.
- Subject to the uncertainties in the underlying NAS FCEV introduction scenario, the demand for $H_2$ has been modeled for the 600 largest urban areas. The garaged HEV distribution is not likely to be a good predictor of future FCEV distribution. Results for the delivered cost of $H_2$ show a leveling out at about $6/kg, which would require more than a decade to achieve a break-even cumulative cash flow. The significance of the study results is undermined by the low level of confidence in the underlying assumptions of FCEV introduction.

### Question 4: Collaborations and coordination with other institutions

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

- The researchers should coordinate with SA and Air Products' projections.

- It is good to see that collaboration has expanded. However, it could be expanded more (as the project progresses) to other stakeholders, such as industry and/or people involved in developing other regional $H_2$ infrastructure development initiatives.
- The project has collaborators from national laboratories, universities, and a not-for-profit partnership. It would be very helpful to involve FCEV OEMs and the industrial gas companies. Although such industry collaborators are not likely to share privileged information or data, they can still serve to identify potential errors in assumptions and they may be able to offer guidance in business-related aspects of infrastructure build-out.
- There is room for improvement in the collaboration to increase the fidelity of the model. Potential collaborators include the U.S. Department of Transportation Pipeline and Hazardous Materials Safety Administration, The New York/ New Jersey Port Authority, and the Massachusetts Highway Department.

### Question 5: Proposed future work

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

- The proposed future work is thin; additional examples of opportunities would help.
- Comparing FCEVs and battery electric vehicle (BEV) market penetration would be valuable.
- The proposed tasks are good, but the researchers need to make sure consistency checks, sensitivity analyses, and validating results are part of the efforts. It would be good to think outside the box a bit and not look at typical/expected deployment patterns. Instead, researchers should use this tool to investigate the "what ifs" of things such as unexpected, extreme developments and constraints; competition from other advanced technologies; and potential synergies.
- The proposed future work is to continue to update SERA, apply SERA to more complex deployment scenarios (in regional detail), and use SERA for multi-fuel (e.g., FCEVs versus BEVs) analyses. Because the results of the study are heavily influenced by the FCEV rollout scenario used in the analyses, it would be useful to examine a range of such scenarios, from the highly optimistic to highly pessimistic. It may also be useful to see what can be learned from the FCEV introduction programs and projections in other parts of the world (e.g., Germany, Japan, and Korea).

### Project strengths:

- This project has good modeling skills.
- There are outstanding resources available.
- The strengths of this project are the knowledge and dedication of the project members.
- The model is customizable to meet a variety of user and analysis needs and can be used to investigate the regional issues and approaches to $H_2$ infrastructure development.
- The SERA model maintains rigorous self-consistency between scenario parameters, such as FCEV vehicle introduction, stock turnover, and vehicle-miles traveled to project $H_2$ demand. The SERA model is consistent with H2A assumptions, parameters, and results.

### Project weaknesses:

- This project needs to incorporate more sensitivity analyses.
- There is a lack of collaboration or coordination with other modelers (or explanation of large differences).
- This project has too many questionable assumptions and a lack of accounting for CCS.
- The weaknesses appear to be the lack of real-world input.
- The major weakness is the uncertainty in the FCEV rollout scenario, which forms the basis for all of the results of the analysis. The U.S. automotive market is highly dynamic, and the OEM's plans for advanced technology vehicles are not included in these analyses. The results presented here and from the proposed future work do not suggest a high level of confidence.

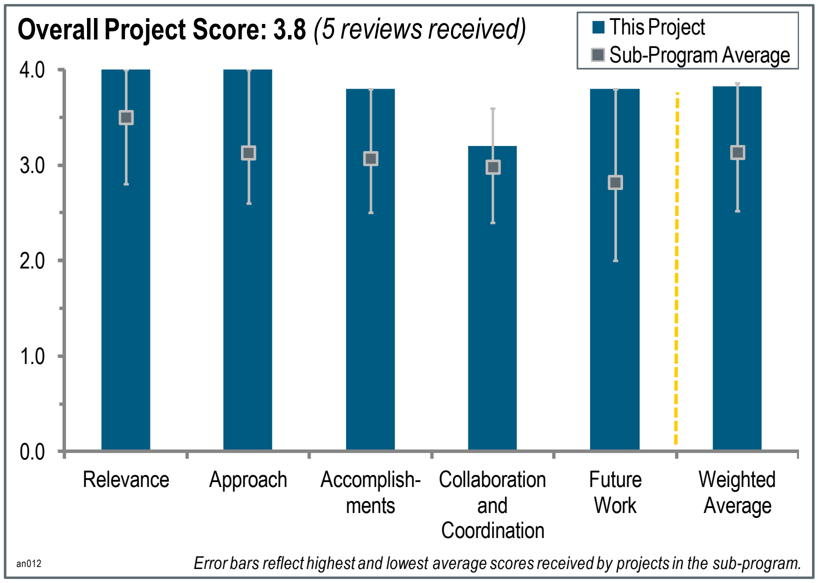
### Recommendations for additions/deletions to project scope:

- Industry input, if it can be arranged, would be very helpful in strengthening the results of the study.
- This project should have more real-world technical and political input into the modeling.

- CCS should be included as an option. Parametric studies, which are valid regardless of assumptions, should be done.
- This project could be more closely coordinated with AN-020 (Melania), Hydrogen Refueling Infrastructure Cost Analysis. Findings and approaches of both these projects can help both teams. The researchers should also incorporate consumer preferences as well as behavior and sensitivity analyses.
- It would be valuable to explore the preponderance of $H_2$ pipelines in slide 14 by analyzing under what circumstances $H_2$ pipelines would be economically viable compared to trucked-in $H_2$ or on-site production. Presumably there would have to be large quantities of FCEVs to justify adding pipelines, but this would likely occur in large cities where the cost of installing a pipeline distribution network might be excessive. This reviewer asked how to get from on-site production and trucked-in liquid or gaseous $H_2$ to the situation shown in slide 14 with a preponderance of $H_2$ pipelines.

# Project # AN-012: GREET Model Development and Life-Cycle Analysis Applications

Michael Wang; Argonne National Laboratory




## Brief Summary of Project:

The objective of this project is to develop and update the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET) model to assess the energy and emission benefits of hydrogen ($H_2$) fuel cell electric vehicles (FCEVs) and other fuel cell systems. Development of GREET allows for: (1) fuel-cycle analysis of $H_2$ FCEVs with various $H_2$ production pathways; (2) vehicle-cycle analysis of manufacturing $H_2$ FCEVs; and (3) life-cycle analysis of $H_2$ and petroleum infrastructure build up. Studies using GREET can now provide life-cycle analysis results for the U.S. Department of Energy's (DOE's) Fuel Cell Technologies Program activities and reports, such as the Multi-Year Program Plan. Additionally, the updated GREET model can be used to support and interact with stakeholders to address the energy and environmental benefits of $H_2$ and fuel cell systems.

## Question 1: Relevance to overall DOE objectives

This project was rated **4.0** for its relevance to DOE objectives.

- The determination of life-cycle costs for various fuel generation technologies is relevant to the DOE objective.
- GREET is the most recognized program to authoritatively estimate well-to-wheels (WTW) greenhouse gas emissions (GHGs).
- This model has served over the years as the key tool for investigating the life-cycle emissions and energy use of various pathways and scenarios that the DOE Hydrogen and Fuel Cells Program (the Program) has considered in developing and validating various technologies.
- GREET modeling has continually provided excellent data on the emissions and GHG performance of the various fuel cell technologies and product applications.
- GREET compares alternatives in an objective manner and is useful, necessary, and the only way to separate hype from reality in most cases (e.g., the effects of shale gas production and comparison of $H_2$ production from shale and natural gas).

## Question 2: Approach to performing the work

This project was rated **4.0** for its approach.

- Adding shale gas and biogas to the model is very relevant and timely.
- Every year the project has done a great job of adding new analyses and features, greatly expanding the capabilities of the model and providing value on the subjects of importance to the analysis community and industry, while also uncovering important factors.
- GREET continues to be responsive and provide important data, giving dimension to the environmental benefits of fuel cells versus alternative energy technologies.

- The approach was always appropriate, but it has been honed over the years to be more realistic. Potential industry changes are addressed proactively.
- The apparent approach taken, generating computer model modules using an existing program, appears to be cost effective. Expanding the models to address emerging energy supplies and updating the modules for the updated data would be wise.

### Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.8** for its accomplishments and progress.

- Shale and manure models were added and the overall model was cleaned up. The impact of platinum (Pt) loading and plant construction was determined. GREET.net is a step forward and will open up usage.
- The addition of fuel-cycle analysis of renewable sources for $H_2$ pathways, specifically conventional and shale gas supply, to the model is helpful. The addition of vehicle-based, life-cycle analysis for FCEVs, battery electric vehicles, and baseline vehicles is interesting but the values may be challenged.
- The new set of analyses performed through the model this year has uncovered important details (such as the impact of Pt loading reduction, plant construction costs, methane leakage, etc.) that typically may be missed but are revealed through comprehensive analyses such as this. These new bits of information obtained will help focus the Program's technology research and development activities.

### Question 4: Collaborations and coordination with other institutions

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

- More detail on information sources would add greater credibility, as industry stakeholders is too vague a term.
- It is unclear as to who collaborated on this activity. "Industry stakeholders" is a nebulous term that often translates to consultants. It is unclear what specific industry stakeholders were involved in the collaboration and what the sources of the information were from which these conclusions were drawn.
- There is good collaboration with both analyses of other models and with industry stakeholders to ensure comprehensive data input. Going forward, more collaboration with specific user groups of the model would be suggested to make sure it is meeting stakeholders' needs and to receive important feedback on what other capabilities and components need to be added. The new GREET.net platform should enable this more.

### Question 5: Proposed future work

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

- The proposed future work appears rational.
- The list is growing shorter and includes more second-order efforts.
- The future work outlined holds promise to both make the model more user-friendly and allow for more detailed, granular analyses.

### Project strengths:

- A strength is the comprehensiveness of the model.
- A strength of this project is the objective analysis of alternatives in a comprehensive manner.
- The principal investigator is very experienced with a long history of detailed WTW GHG calculations and models.
- The ability to present credible, objective, and unbiased data is of tremendous value to understanding the potential benefits of fuel cell technologies.
- The model includes a diverse array of technology options, and it has been enhanced each year to be able to perform more detailed analyses and to address emerging issues of concern. The model is well established, widely known and used, and is usually accepted as the go-to standard model for emissions/energy-use analyses. The recent development of the GREET.net platform will enable the use and understanding of the model by a wider range of stakeholders and in a more focused way.

### Project weaknesses:

- This project has no weaknesses.
- The substantiation of assumptions is weak and needs more detailed industry input.
- As the data keeps adding more components, it might become more difficult to ensure consistency, accuracy, and validation; more care should be given to this as the project advances.
- The primary weakness is the apparent lack of visibility of the underlying data and assumptions. This leads to questions on the fidelity of the conclusions drawn from the model.

### Recommendations for additions/deletions to project scope:

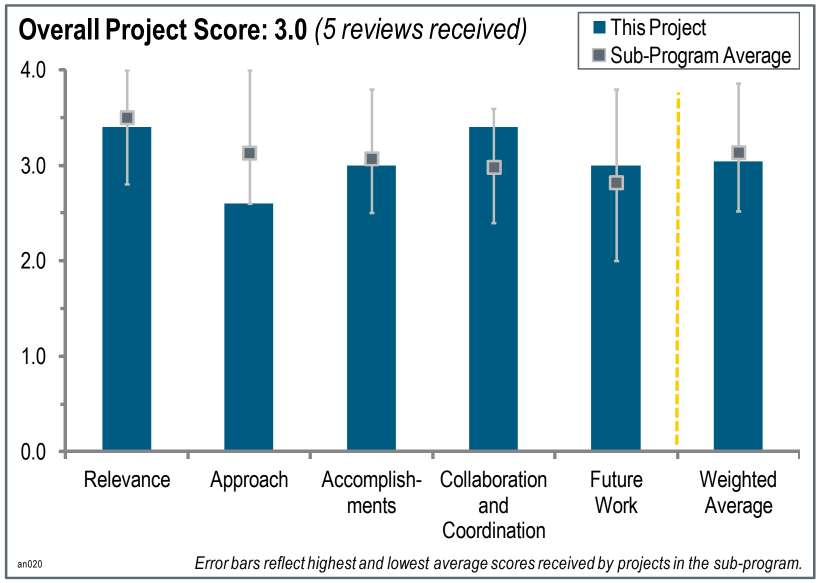
- The researchers should add industry peer reviews of assumptions and verifications of results.
- The research should clarify who the collaborators are and the sources of the data.
- This project should continue to provide data to support new commercial fuel cell products, such as fuel-cell-powered mobile light stands and generators.
- This project should coordinate more with the SERA model to enable more detailed energy/emissions analyses related to potential regional $H_2$ infrastructure development scenarios, to facilitate a more detailed, enhanced understanding of what these developments might mean and where efforts should be focused.

## Project # AN-020: Hydrogen Refueling Infrastructure Cost Analysis

Marc Melaina; National Renewable Energy Laboratory

### Brief Summary of Project:

This project identifies cost metrics for near-term markets for hydrogen ($H_2$) fueling station for commercial and private vehicles. Specifically, the objectives are to: (1) identify the capacity (kg/day) and capital costs associated with early commercial $H_2$ stations and (2) identify cost metrics for greater quantities of $H_2$ fueling stations across the United States as well as for larger capacity fueling stations.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- A credible estimate of $H_2$ station costs is essential to move the $H_2$ and fuel cell electric vehicle (FCEV) market forward.
- This project is extremely relevant because a refueling infrastructure is critical to FCEVs.
- This activity is relevant to the DOE objectives; the limited responses to the survey are unfortunate but expected. As fuel cell technology is maturing toward deployment, the costs of putting the fueling infrastructure in place are becoming a key factor in the overall commercialization process for the technology.
- At a time when research and development is improving technology, niche markets are developing, and automakers are coming out with strong statements regarding commercialization in a couple years, it is important to understand what the costs and required level of effort might be for early market infrastructure development. This project addresses those questions both by gathering stakeholder input and by attempting to quantify stakeholder feedback.

### Question 2: Approach to performing the work

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

- The approach lumps all types of $H_2$ production and delivery together. This makes the approach and results questionable at best.
- The approach is adequate, but the execution may have areas where it can be improved. For example, multiple short surveys may be more conducive to getting the necessary responses instead of a large, single survey, which may intimidate potential responders.
- It is nice to see the four station types compared side by side. The calculator gives respondents flexibility while also maintaining a structure consistent with the Hydrogen Analysis (H2A) model. While the stakeholders providing input to the cost calculator represent diversity, a big portion (50%) is from universities and the government. It would have been better and closer to reality if a larger percentage of respondents was drawn from industry.
- Using estimates from companies involved in building early stations is valuable. The downside is that mobile refuelers, trucked-in gaseous or liquid $H_2$, and on-site steam methane reformers or on-site electrolyzers cannot be distinguished. In addition, it is not possible to add variable costs, because it is not known which fuel is used. The capital cost estimates can only be used to bracket the cost of $H_2$ stations going forward.

- In this project, qualitative input from a handful (11) of respondents, not all of whom have provided complete input, is being converted to quantitative results (from the Hydrogen Station Cost Calculator model [HSCC]). The researchers at the National Renewable Energy Laboratory (NREL) did not have direct interaction with the respondents, so they could only have access to aggregated data. The approach is further weakened by the fact that only half of the respondents were from or related to industry, the other half being from universities and the government.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The results allow good initial estimates of what early market costs might be and how infrastructure development and cost reduction may be accomplished.
- The results on capital costs for a generic $H_2$ fueling station are valuable. Using a third party to shield the suppliers is a good solution to protecting proprietary cost estimates.
- Considering the level of funding, this was a good effort. However, even with a good effort the results have limited value.
- The progress to date is as expected. The identification of compression costs as an issue is obvious. However, the lack of discussion on the insufficient flow measurement issues shows that there are areas for potential improvement. The limiting of polling to a single U.S. DRIVE Partnership technical team limits the input and potential value of the survey. The fuel cell and codes and standards teams (small in situ reforming, component, and instrument development) may be additional relevant information sources.
- Although there were 10 slides of accomplishments and progress in the presentation, it is difficult to assess the usefulness of some of the results shown. For example, slide 14 shows costs for station sizes and projected time frames, but there does not seem to be any connection with the development of the corresponding demand for $H_2$ (i.e., plans for the manufacture or sale of large enough numbers of FCEVs in the same time frames). There was nothing shown on the potential validation of the cost equation (slide 18) with the fueling installations for fuel cell forklift warehouses, for example.

### Question 4: Collaborations and coordination with other institutions

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

- It is good to see interaction with groups such as the California Fuel Cell Partnership and U.S. DRIVE.
- NREL cannot reveal the companies involved and must trust that the third-party vendor chose an appropriate mix of equipment vendors.
- Publishing the results would help with collaboration efforts.
- With the exception of the comment on the U.S. DRIVE teams, the collaboration and coordination with other entities appears to be appropriate.
- Given the constraints of proprietary restrictions on hard data, this project is attempting to work with, and obtain information from, as broad a group of stakeholders as possible. Thus, the project has good collaboration, even if it is through International Data Corporation Energy Insights, rather than directly.

### Question 5: Proposed future work

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

- It is not clear what will be done with this information.
- It is good to see that this analysis related to infrastructure is going to be tied to vehicle rollout and that other models such as the Scenario Evaluation, Regionalization, and Analysis (SERA) model will be utilized.
- The proposed future work appears slim. Further examination into the station cost drivers to help direct DOE was expected.
- It would probably be better to have one or two comprehensive projects rather than many “small” projects. The principal investigator presented three of these small projects in one session.

- The project is scheduled to end by September 30, 2012; not much was said about plans for future work. The two slides on the proposed future work (slides 20 and 21) contained only generic statements on what is needed, but no specific future activities were given.

### Project strengths:

- The project quantifies qualitative feedback from stakeholders.
- The project features a good team of investigators with years of experience in this space.
- This research team has very competent personnel and resources.
- The strength is the importance of collecting and analyzing the field data.
- A quantitative tool—the HSCC —has been developed that can be used to assess the effects of varying station parameters on station costs. The concept of four types of stations (one current, three future) is useful in assessing how fueling stations might be implemented to support the introduction and deployment of FCEVs.

### Project weaknesses:

- A more diverse set of respondents (fewer from government and academia and more from industry) could have been used.
- There is too little value in the results of any of the small projects.
- All of the project's outcomes depend on the input received from a relatively small number of respondents. Further, only half of them were from industry, and most of them provided only partial responses. Thus, the degree of confidence in the results is not very high.
- The weaknesses are the lack of guidance to DOE on specific weaknesses and areas of needed development to support the DOE programs. A computer model, if not used to address specific issues, is of questionable value.
- This approach does not allow a desegregation of costs for different fueling stations. Hence, it is not known if the results are skewed to the low side by including mobile refuelers, for example, or skewed to the high side by including more expensive electrolyzers. This approach also excludes variable operating costs, because it is not known whether the stations use natural gas, electricity, or trucked-in $H_2$ as the source.

### Recommendations for additions/deletions to project scope:

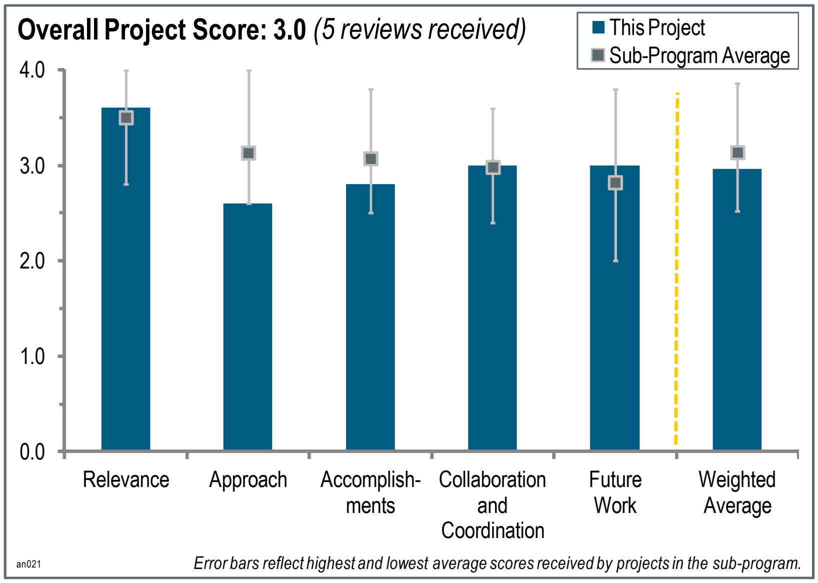
- Stop funding projects that have so many assumptions that they have little value.
- The project will come to an end soon. There is nothing specific to suggest for the remainder of the project term.
- Researchers should generate results and feedback to DOE for specific areas of future focus.
- Ideally, similar data should be obtained for each type of $H_2$ station, but this may be difficult given that so few stations of each type have been built.
- It would be nice to conduct a follow-on study in a couple of years, during the 2014–2016 "early commercial" time frame identified, to see if calculations and projections match with real-world deployments and developments to help understand the path forward more clearly.

## Project # AN-021: Comparing Infrastructure Costs for Hydrogen and Electricity

Marc Melaina; National Renewable Energy Laboratory

### Brief Summary of Project:

The objective of this project is to compare retail infrastructure costs on a common transportation energy service basis, namely, per vehicle mile traveled. The project compares capital costs between advanced vehicle types, assuming market adoption challenges of vehicle types have been met.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- This effort is relevant to the DOE objectives in that, if done correctly, it will result in guidance on where to focus research.
- This project is very relevant because these are two major means of reducing petroleum usage.
- This project aims to compare the cost of the fuel supply infrastructure for fuel cell electric vehicles (FCEVs) and electricity (for battery electric vehicles [BEVs] and plug-in hybrid electric vehicles [PHEVs]), but not including generation and transmission for the latter. It may be commonly assumed that the electric infrastructure has little additional cost, but this study is addressing this issue in a systematic manner.
- Vehicles are more ready in the deployment scheme, but they are now waiting for the infrastructure to develop. In addition, FCEVs and PHEVs have their challenges, but proponents and opponents of each side tend to pull the arguments relating to feasibility and deployment timelines in various directions. An analysis such as this helps lay the groundwork for clarifying and outlining issues in a more objective way and also helps guide research and development investments.

### Question 2: Approach to performing the work

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

- The approach had too many major assumptions and too little actual data.
- Some assumptions are somewhat optimistic. The industry is new and there is a lack of data, so it is harder to perform these analyses. This approach at least sets a framework for further refinement as more reliable data becomes available. Conducting some sensitivity analyses or investigating a couple different cases/assumptions could help the project.
- The approach is valid, but some of the assumptions made may invalidate the conclusions. The concept that a PHEV has a fuel mileage of 141 miles/gge, while an FCEV only has a fuel mileage of 59 mi/gge, is questionable. The current FCEV mileage of fielded units without regenerative braking has been demonstrated to meet or exceed this value. To make an FCEV with energy storage and regenerative braking, as is done currently on transit buses, will increase the mileage. Analysts should remember that an FCEV is an electric vehicle.
- This analysis assumes an established light-duty vehicle (LDV) market size of 10% in a city of 1.5 million people and 1.2 LDVs. Then, the 120,000 electric vehicles are either all FCEVs or all PHEVs (20%–30% BEVs and 70%–80% PHEVs). Using an assumed 10% interest rate and a 12-year lifetime, the results for either case are expressed as capital costs as dollars/mile and city service costs as dollars/city. Similarly, different sizes of $H_2$ fueling stations and different electric charging equipment configurations are being analyzed.

- The method of comparing FCEV and BEV infrastructure costs may be misleading. Other sources have shown that installing electrical infrastructure for BEVs and PHEVs could cost many times more per vehicle than $H_2$ infrastructure. For example, the McKinsey & Company report for the European Union (EU) estimated that electrical infrastructure would cost five times as much as $H_2$ infrastructure for the entire EU, while slide 13 shows an electrical infrastructure cost being less than the $H_2$ infrastructure cost. The electric vehicle supply equipment (EVSE) estimates also seem low. One of the most credible estimates is from Kreider & Associates, which obtained three competitive bids for Type II EVSE systems to retrofit an existing 300-car parking garage in Boulder, Colorado. The average of the three bids was $3.72 million, or $12,400 per PHEV, compared to slide 12, which shows a spread with a maximum of only $6,751 per PHEV. In addition, slide 11 indicates that 43 PHEVs will recharge at work, implying that each PHEV could charge for only 11.2 minutes in an 8-hour day, which the principal investigator (PI) said would require a switching system. However, this would add significant cost to the EVSE because a switching network to switch 43 plugs at 240 volts would be expensive.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Results give some initial insights but are based on a variety of generalized assumptions and would need to be further refined.
- The results to date are interesting but highly dependent upon the assumptions. Applying tolerances to the assumptions for a sensitivity analysis may help with the fidelity of the results. Additionally, the researchers should add some reality to some of the assumptions, such as peak time energy costs, grid saturation (brownouts), and not-in-my-backyard (NIMBY) issues with new transmission and distribution power lines. The Electric Power Research Institute (EPRI) may be able to help here.
- Subject to the assumptions used in the study, it was determined that:
    - City-wide annualized capital costs are 3.1, 3.0, and 3.2 cents/mile for FCEVs and PHEV-home and PHEV-robust scenarios, respectively (i.e., these costs are essentially the same for all three scenarios).
    - Overall costs can be reduced by implementing a combination of fast-charging public systems and slow-charging home systems.
    - Sensitivity analyses indicated a capital-cost variability of 2–3 cents/mile for all systems.
    - Fuel costs for PHEVs are 15%–20% lower than for FCEVs, but they have slightly greater uncertainty.
    - Overall, FCEV and BEV costs per mile are comparable, while the cost per mile for PHEVs is about 10% lower.

### Question 4: Collaborations and coordination with other institutions

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

- This project has excellent collaboration.
- This project could expand its collaboration to also include some industry feedback.
- Collaboration with multiple entities has consisted mostly of reviews of the study results by the collaborators, rather than active collaboration and input during the course of the study.
- The collaboration within the National Renewable Energy Laboratory (NREL) staff, the U.S. DRIVE Partnership Fuel Production and Integration Technical Team, DOE staff in the Vehicle Technologies Program, and academic consultants is a start. Input from other related venues will probably offer additional insights. Researchers should leverage the other tech teams to avoid the appearance of a nepotistic viewpoint. Researchers should also leverage other organizations, such as EPRI, the Gas Research Institute, the American Gas Association, DOE Federal Energy Regulatory Commission, and the U.S. Department of Transportation Pipeline and Hazardous Materials Safety Administration.
- The PI showed a graph from the McKinsey & Company EU report; part of collaboration implies a blending of opinions or results from other groups, which this report does not seem to accomplish. Another example, slide 17 implies that a BEV costs only slightly more than an FCEV, but Kromer and Heywood at the Massachusetts Institute of Technology estimated that, in mass production, an FCEV would cost $3,600 more than an internal combustion engine vehicle, while a BEV would cost $10,200 more. Similarly, the McKinsey & Company report estimated that, by 2030, the total cost of ownership of an FCEV would be less than the cost of either a BEV or a

PHEV. Slide 17 shows the PHEV costing less than the FCEV. The McKinsey result has particular credibility, because 10 major original equipment manufacturers shared their proprietary vehicle cost data with McKinsey in a clean room.

### Question 5: Proposed future work

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

- The future work planned looks very good, but it is not likely to be relevant, with current funding levels.
- Looking into factors such as consumer preferences and regional/geographic diversity, as is proposed, will be key to clarifying and providing more realistic results from this research.
- The proposed future work is appropriate. Focusing on the fidelity of the assumptions based on external input and real-world limitations (e.g., grid saturation and NIMBY) would improve the usefulness of the conclusions.
- In future work, the study will examine different cities/regions and the geographic variability in the availability and cost of low-carbon energy sources. The researchers will also examine factors affecting consumer/investor behavior. With the high level of uncertainty in the assumptions (a major one being the number of PHEVs as a fraction of all LDVs in a city), going to this level of detail does not seem to be very useful.
- Geographic variation is important. There is one caveat from the future work slide: the PI claims that EVSE would be used more efficiently in higher-density cities. This may be true for $H_2$ stations, where more FCEVs per station are needed, but that does not address the main issue with EVSE: only one car can be charged at a time, so more PHEVs wanting to use a station has no impact if each car has to charge for 30 minutes to an hour or more.

### Project strengths:

- This project has good resources and a good PI.
- The strengths are the knowledge and dedication of the project members.
- This project has a good approach and analysis. The researchers have done a considerable amount of work, given the limited resources for the project.
- This is a good team that has extensive experience in the $H_2$ and alternative vehicle fields, and access to NREL's large stable of vehicle models.
- It is useful to have a framework and effort that attempts to compare the different infrastructures involved with $H_2$ and PHEVs.

### Project weaknesses:

- There are a variety of general assumptions and not enough data at this point.
- There is a high degree of uncertainty in the basic assumptions used in this analysis.
- This project's weaknesses are the lack of tolerance or sensitivity analysis on the assumptions, external input, and real-world constraints.
- The apparent failure in this project is calibrating results with other major studies in this area (or at least explaining the large differences).

### Recommendations for additions/deletions to project scope:

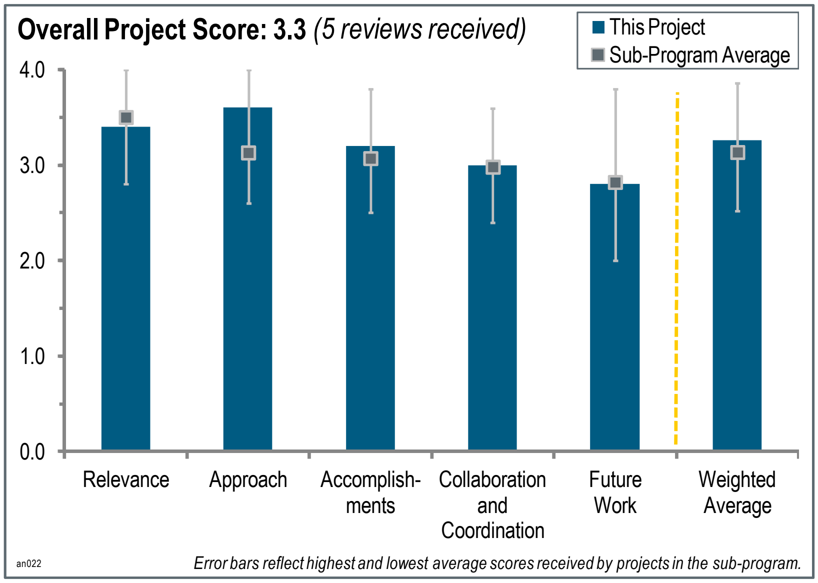
- If the researchers amend the approach slightly to address the perceived weaknesses, this project would be a home run.
- Rather than being a stand-alone analysis, it would be better if this effort was integrated into other modeling and analysis efforts such as the Scenario Evaluation, Regionalization, and Analysis (SERA) model, which already has more established assumptions and structures.
- This reviewer has no recommendations for this project.

## Project # AN-022: Infrastructure Costs Associated with Central Hydrogen Production from Biomass and Coal

Darlene Steward; National Renewable Energy Laboratory

### Brief Summary of Project:

This study seeks to elucidate the location-dependent variability of infrastructure costs for hydrogen ($H_2$) production and delivery, and the trade-offs inherent in plant location choices. By combining geographic information system (GIS) data with established cost models, the project maps resources, existing infrastructure, and centers of demand to construct cost correlations based on distance, terrain, and land use to determine the cost efficiency for hypothetical plants at any locale.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- This project is useful to the DOE Hydrogen and Fuel Cells Program (the Program) because it looks at details such as the location dependence of infrastructure.
- This project is extremely relevant because coal is an abundant resource and biomass is nearly carbon neutral.
- This project fully supports the goals of the Program and will enable $H_2$ production from biomass or coal. This project addresses barriers B, D, and E.
- This project's studies are very relevant and seem obvious on the surface, but the studies clearly must be done at some time and this seems to be an appropriate time. The choice of the two technologies is somewhat limiting, and a comparison with other methods of energy delivery is missing. The researchers should find out how these costs stack up to electricity distribution for charging or the present gasoline distribution system.
- This project is assessing the infrastructure costs of central $H_2$ production (and delivery) from biomass or coal with carbon sequestration. Such an infrastructure would be implemented only after the fuel cell electric vehicle markets are mature and the long-term demand for $H_2$ is established. The uncertainties associated with the requisite time period (considerably into the future) would lead to low confidence in the results.

### Question 2: Approach to performing the work

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

- This project's approach is as good as possible, considering the size of the project.
- The use of geographic data with cost analysis is powerful and important, specifically the mapping of availability of resources and correlating this with pipeline/railroad cost limit areas where practicality can be achieved.
- This is very nice work that demonstrates the advantages of these kinds of studies. As noted above, comparison with other modes of energy distribution would have been a major plus to these studies (unless of course the results show that $H_2$ is hopelessly expensive).
- For the $H_2$ from biomass study, the project is using GIS data and existing cost models to determine the infrastructure costs (production and delivery) across the United States. For the $H_2$ from coal study, coal delivery, $H_2$ pipeline, and $CO_2$ pipeline (to sequestration site) are included.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Progress on the two modes is satisfactory and valuable results are clearly obtained. The methodology may be a little simple, but it does lead to conclusions that are immediately useful to DOE.
- Some useful insights were gained from this detailed analysis because it considers various constraints in developing infrastructure that have not been considered in analyses before.
- This project gives a better of understanding of $CO_2$ reservoirs. However, of the three types of $CO_2$ reservoirs identified—saline, unmineable coal, and oil and gas reservoirs—only saline reservoirs will allow for net negative $CO_2$ sequestration. Cost advantages should be achieved with unmineable coal and natural gas fields where the $CO_2$ will displace natural gas and depleted oil fields of sufficient depth where enhanced oil recovery will occur. However, in all of the latter cases, this is not necessarily a net negative $CO_2$ repository. This project identified distinct geographic regions where coal or biomass would be feasible.
- Biomass delivery for 100-mile maximum transport distance would add \$0.11–\$0.26/kg of $H_2$. $H_2$ pipeline costs add \$0.21–\$2.70/kg of $H_2$, with terrain and protected lands affecting the costs. For coal-to-$H_2$ plants, the project has developed maps of carbon sequestration potential and new rail costs. Total infrastructure costs for biomass-to-$H_2$ and for coal-to-$H_2$ were assessed for several U.S. metropolitan areas.

### Question 4: Collaborations and coordination with other institutions

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

- It would be useful to also collaborate with other modeling efforts.
- The project collaborators include researchers from several other research teams.
- This project has excellent collaboration between industry partners and other national laboratories. The project integrates well with the Hydrogen Analysis sub-program. This project probably needs more collaboration with organizations outside of the National Renewable Energy Laboratory (NREL). The database is limited.
- The combination of NREL, DOE, and the Pacific Northwest National Laboratory is not a very strong collaboration.

### Question 5: Proposed future work

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

- The future work proposed will add value to the study.
- This project needs a comparison with present energy distribution systems.
- The proposed future work is primarily to examine sensitivities to various parameters and assumptions. Perhaps analyses to such detail are not very meaningful.
- The planned work looks good, but it may not be possible considering funding levels. Also, a pipeline for $CO_2$ should be included.
- If rail delivery of biomass is to be considered, the cost of drying it must also be included, as there may be significant disadvantages to transporting partially wet biomass. Possible cost advantages to certain $CO_2$ reservoirs should be considered, especially as the oil and gas industry is changing so fast, not just in terms of enhanced oil field recovery, but also the use of $CO_2$ for clathrate methane production.

### Project strengths:

- A great deal of work has been done with very limited resources. The analyses seem to be very comprehensive.
- This project has extremely good integration of geographic data with cost analysis.
- This project uses simple methodologies with well established methods to get results quickly. Given the funding levels, this is a wise strategy.
- This project's strength is its NREL resources.
- Constraints that were not considered in previous analyses give new insights.

## Project weaknesses:

- This project needs a much better understanding of the fossil fuel industry in general. It is totally based on cost, which is fair, but at some point $CO_2$ emission increases/decreases will have to be evaluated.
- This project's weaknesses arise from low funding levels. Better collaboration is needed and comparisons with present inefficient gasoline delivery systems would be most useful.
- These analyses really apply to situations rather far into the future. This time frame adds to the uncertainty in the assumptions and gives results that offer low levels of confidence.

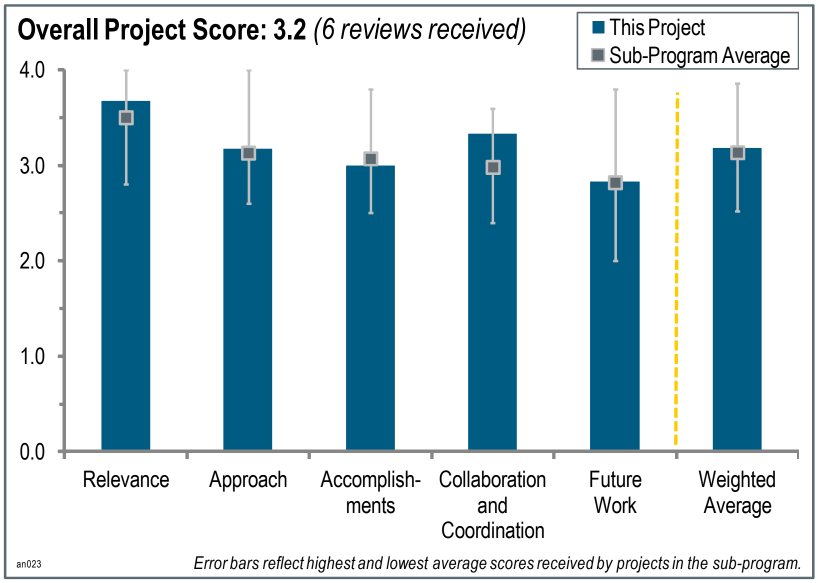
## Recommendations for additions/deletions to project scope:

- This project should add comparisons with present systems.
- This project should include $CO_2$ pipelines for coal.
- This project should integrate findings with the Scenario Evaluation, Regionalization, and Analysis (SERA) model and the Hydrogen Demand and Resource Analysis Tool (HyDRA). It could also look into other types of biomass resources and factor in coal plants that potentially might go offline in the future due to regulations to see what other limitations might occur.
- This reviewer had no recommendations.

## Project # AN-023: H2-Vehicles Market Prospect, Cost, and Social Benefit

David Greene; Oak Ridge National Laboratory

**Brief Summary of Project:**
The overall objective of this project is to understand market prospects, costs, and benefits of light-duty vehicle (LDV) hydrogen ($H_2$) fuel cells, and their sensitivity to fuel cell and battery improvement and other factors. Specific goals of this project are to: (1) conduct a market analysis by integrating output of various U.S. Department of Energy (DOE)-sponsored and other federal projects, (2) project market penetrations of $H_2$ vehicles under varied scenario assumptions, (3) estimate social benefits and public costs under different penetration scenarios, and (4) compare the cost-effectiveness among scenarios.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- This project is very relevant.
- This project will be of great importance now that the focus is shifting back to fuel cell electric vehicles (FCEVs) and $H_2$ fueling infrastructure.
- Understanding the relevance is of value for all stakeholders. It provides additional perspective for the original equipment manufacturers (OEMs).
- The project helps to identify the most probable pathway toward the utilization of $H_2$ in vehicles.
- This modeling work is crucial to projecting potential FCEV market penetration and to identify options to increase that penetration.
- This is important work to help define what the customers will prefer and helps guide where DOE should put its scarce resources. It is not quite clear how the customer preferences are measured or defined and the validity has not been adequately demonstrated, judging by this presentation.

### Question 2: Approach to performing the work

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

- This project uses a great model.
- This project makes many questionable assumptions, especially “cheap” batteries.
- The approach is OK, although there is confusion as to how the consumer preferences are measured. The presentation did not provide a description of the validation of the models.
- The approach is appropriate because it is taking data and input from a number of sources. How the results are factored into the conclusions when the technology is changing so quickly and the fact that there are so many uncertainties will ultimately determine the feasibility of the approach taken in this assessment.
- The model is used to analyze several factors likely to influence the competitiveness of FCEVs and the price sensitivity analyzed. The consumer preference part is always a little subjective. Perhaps only the hard parts of the study, such as fuel availability and range of vehicle, should be considered.
- Speaking from personal experience in the auto industry, automakers assign high importance to compliance with the corporate average fuel economy (CAFE) requirements, federal mobile source emissions regulations, and the

California Clean Car Program and zero-emissions vehicle mandate because they must comply with these regulations or face onerous penalties. Any study involving LDVs has to recognize the impact of compliance to the U.S. regulatory standards. Also important to any study of LDVs is recognizing the various vehicle market segments, such as B-class, C-class, and C/D segments. The buyer profiles of each of these segments are substantially different, and they are limiting factors in terms of vehicle choice. There are technology limitations that need to be recognized. As an example, it will be difficult for the automakers to sell battery electric vehicles (BEVs) in segments above B-class because the size and weight of the battery compromises the overall vehicle package. In terms of addressing the cost of ownership, while the purchase price premium for FCEVs is a recognized barrier, the study should also recognize the uncertainty of residual value as a cost premium. Other nuances are lease financing (probably about 25% of new car retail sales) and the commercial fleet segment (about 2–3 million vehicles annually).

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The study is off to a good start and this reviewer is impressed with the team that will be doing the project.
- The work pulled together a wealth of information from various sources and formed a framework from which to assess the opportunities. The scenarios covered were appropriate, yet it was impossible to assess the significance of the key pathways without knowing the key assumptions and the impact thereon.
- The project did a nice study of the effect of oil prices and concluded that FCEVs will be successful if the technological goals are achieved. The study successfully showed the beneficial effect of subsidies and compared fuel-cell-only vehicles to other technologies.
- The progress to date covers less than 50% of the project, so one will expect to see more useful results as the momentum increases in the second half of the project. Validation is necessary.
- This project's accomplishments are in question due to questionable assumptions.

### Question 4: Collaborations and coordination with other institutions

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

- This project has good collaboration between national laboratories, a university, and OEMs.
- Industry, national laboratories, and universities make for outstanding collaboration.
- The collaborative partners are appropriate, yet the only U.S. OEM fully engaged was not a collaborative partner (General Motors).
- The major collaborations appear to be with other national laboratories. The information that the laboratories have is hopefully sufficiently accurate and broad-based. The details about collaborations with the car companies and the fuel suppliers are not adequate.
- The team of people who will be doing this study is very well respected. However, this team should seek out the input of the automakers that will be challenged with selling these vehicles.

### Question 5: Proposed future work

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

- This project does not state much about its future work.
- The future work appears to be appropriate given the time remaining in the project.
- The cost estimates for electricity infrastructure subsidies should be added to compare with the $H_2$ infrastructure subsidy estimates.
- Completion of this project will be very important for directing future research and policy toward $H_2$ vehicles. The consumer preference part of this work must be critically evaluated before implementation, so as to not erroneously push the results toward a metric that is purely subjective.

### Project strengths:

- The people assigned to the task are a strength of this project.
- This project has an outstanding principal investigator (PI) and good resources available.
- This project's strengths are the experience of the PI and the team's modeling skills.
- The team clearly was proficient as evidenced from the available data and models utilized in the assessment.
- This project completed a comprehensive sensitivity analysis toward implementation of $H_2$ for vehicular applications.

### Project weaknesses:

- More emphasis should be placed on interactions with the auto industry.
- Trying to predict customer preferences is a losing battle.
- The researchers should use greater care in the consumer preference part of this project.
- This project does not have any specific weaknesses yet as it is only a one-year program. The field is immature and continuously evolving at this point in time. The output of this program should be able to adjust to such factors as they emerge.

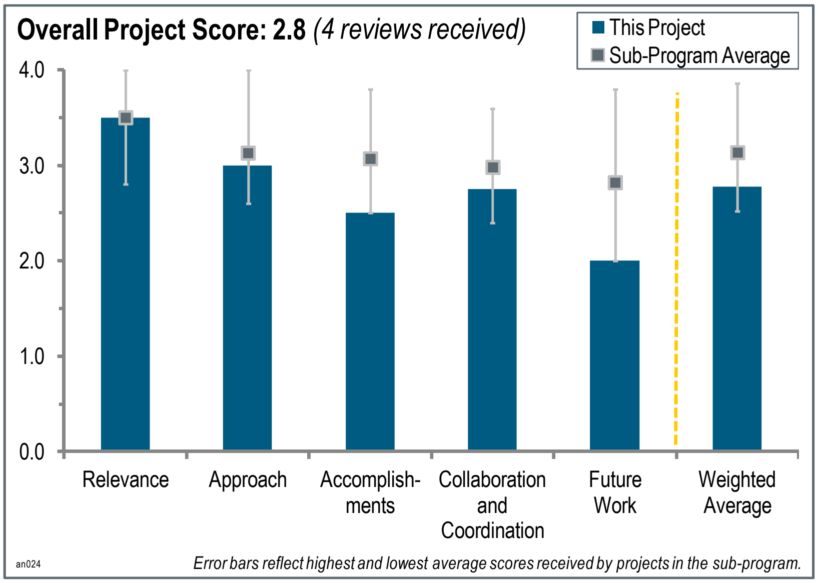
### Recommendations for additions/deletions to project scope:

- The researchers should conduct parametric studies when investigating customer preference impacts.
- The researchers should add estimates of the electricity infrastructure subsidies needed for BEVs and PHEVs.
- The conclusions at the end of this program should, if they are appropriate, become a baseline from which to gauge the key issues and progress of vehicle penetration over the next few decades.
- It may be worthwhile to address the commercial fleet market separately. The automakers can approach this market differently and like to do so in marketing alternate fuel vehicles.

## Project # AN-024: Issues Affecting Hydrogen Pathway Succession

Mark Ruth; National Renewable Energy Laboratory

**Brief Summary of Project:**
The objective of this project is to improve the understanding of options and trade-offs in the evolution of hydrogen ($H_2$) production and delivery infrastructure for transportation. Existing data is examined using various analysis models and tools, including the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation Model (GREET), the HyPro model, and the Macro-System Model (MSM). The anticipated result will be an improved understanding of how policy changes, market issues, and technology status may affect technology selection and emissions.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This is a very important study.
- It is important to model $H_2$ pathways because this is critical to the commercialization of fuel cell products.
- This project analyzes the likely succession of technologies that will occur during the ramp-up of use of $H_2$ as a fuel and thus overcome multiple barriers, especially future market behavior.
- This project is very relevant to DOE's goals. The development of scenarios of how $H_2$ will be produced and the implementation of the technology is critical. However, the relevance is somewhat reduced because of the uncertainties of the technologies available and possible competition from other fuel sources, such as breakthroughs in battery technology, etc.

### Question 2: Approach to performing the work

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

- This project has too many major assumptions and much of the work was done last year.
- The use of the MSM enables the use of many of the models developed for $H_2$ production, delivery, and utilization. Parameters in models can be varied in sensitivity runs to allow for future scenarios.
- A lot of the projections involve capital costs, and the source of the capital costs is not clear. No mention is made of collaboration with equipment manufacturers or construction firms that would have these numbers. Further, the carbon capture and sequestration (CCS) technology is not proven at the moment, so the projections may not be realistic. Similarly, the potential impact of breakthroughs in battery technology, liquid fuels, etc. is not included.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The work from this project has been generally very good. Especially useful is the incorporation of GREET emissions data.

- Progress is good for the goals that have been focused upon. The next phase should look at the effects of "left field" technology developments that are almost certain to occur in the time frame dealt with.
- The researchers made an excellent choice of comparison technologies, showing that steam methane reforming is still the most feasible near-term method for producing $H_2$. CCS is still not a proven technology and so the analysis of this is still a little hard to believe.

### Question 4: Collaborations and coordination with other institutions

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

- It does not seem that there was much collaboration; industry input is needed.
- This project has good collaboration with national laboratories and industry partners. The project would be strengthened with more industry and government input.
- This project's collaborations are with the same group of national laboratories and contractors. No mention is made of collaborations with equipment suppliers or construction firms, and the review by the technical team does not constitute collaboration.

### Question 5: Proposed future work

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

- No specific comments are offered on future work because the project is finished.
- The researchers should look at other pathways or better define or re-define the studied pathways.
- The project is complete; the proposed improvements are OK, but they hardly give a good reason to fund more of this.
- The use of learning curves would be highly valuable and, together with unforeseen randomness, could make the model more realistic.

### Project strengths:

- This project has focused and robust predictions.
- This project's strengths' are its use of multiple models and contrasting $H_2$ production methods.

### Project weaknesses:

- This project has ended.
- There is clearly not enough collaboration with industry players.
- There are too many major assumptions required.

### Recommendations for additions/deletions to project scope:

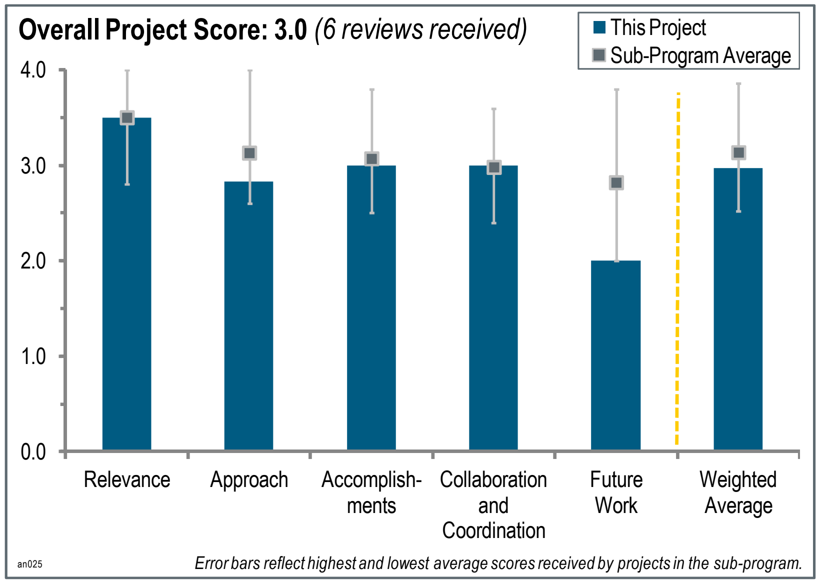
- The researchers should continue with the proposed new project.
- Future projects should bring in industry players as full partners.

## Project # AN-025: Impact of Program Targets on Vehicle Penetration and Benefits

Zhenhong Lin; Oak Ridge National Laboratory

### Brief Summary of Project:

The overall objective of the project is to estimate the impact of the U.S. Department of Energy (DOE) program goals on the market prospect, costs, and social benefits of hydrogen ($H_2$)-powered light-duty vehicles (LDVs). Specific goals of this project are to: (1) conduct market analysis by integrating the outputs of various DOE-sponsored and other federal projects; (2) project market penetrations of $H_2$ vehicles under varied assumptions of program goals for fuel cells, $H_2$ storage, batteries, motors, and $H_2$ supply; (3) estimate social benefits and public costs under different program goal scenarios; and (4) compare cost-effectiveness among scenarios.




### Question 1: Relevance to overall DOE objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This project is very relevant to the DOE activities.
- This study could be a major contributor now that more focus is shifting toward fuel cell electric LDVs and $H_2$ infrastructure.
- The Market Acceptance of Advanced Automotive Technologies (MA3T) model development/utilization could assist DOE in setting goals to achieve emission and fuel consumption reductions. Exogenous model development is critical to this objective.
- This project directly addresses the consequences of meeting the DOE Hydrogen and Fuel Cells Program goals, and it will be a very useful tool as these need future modification.
- This project has good relevance because it examines the consequences of missing technology implementation goals. This allows DOE to be aware of where the important bottlenecks are and the necessity to provide adequate resources to the critical activities.

### Question 2: Approach to performing the work

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

- This project explores various scenarios for fuel cell electric vehicles (FCEVs), battery electric vehicles (BEVs), and hybrid vehicles using multiple models and projections.
- The approach seems fair, but perhaps the source of data is too limited. There does appear to be adequate accounting for BEV development.
- The approach appears sound but the delivery is confusing. Unless the reader is familiar with the details of the analysis, the presentation curves have no relevance. More explanation of the assumptions made and maximum error bands may make the presentation more user friendly. In addition, listing the assumptions that are truly critical path (i.e., have more than a 25% effect on the total prediction) would be helpful for DOE.
- This project has a low budget to model a very complex situation with a huge number of variables and alternative scenarios. It is a laudable goal, but many would consider this to be pure speculation where the choice of the assumptions determines the outcome.

- Speaking from personal experience in the auto industry, automakers assign high importance to compliance with the corporate average fuel economy (CAFE) requirements, federal mobile source emissions regulations, and the California Clean Car Program and zero-emissions vehicle mandate because they must comply with these regulations or face onerous penalties. Any study involving LDVs has to recognize the impact of compliance to the U.S. regulatory standards. Also important to any study of LDVs is recognizing the various vehicle market segments, such as B-class, C-class, and C/D segments. The buyer profiles of each of these segments are substantially different, and are limiting factors in terms of vehicle choice. There are technology limitations that need to be recognized. As an example, it will be difficult for the automakers to sell BEVs in segments above B-class because the size and weight of the battery compromises the overall vehicle package. In terms of addressing the cost of ownership, while the purchase price premium for FCEVs is a recognized barrier, the study should also recognize the uncertainty of residual value as a cost premium. Other nuances are lease financing (probably about 25% of new car retail sales) and the commercial fleet segment (about 2–3 million vehicles annually).

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- Progress is good and as expected at this stage of a one-year project.
- The progress on the models is noted and appreciated. The fidelity of the results (effect of tolerances) is a start. In the future, a technology that shows where the assumptions and key parameters are should be selected so that the researchers can explain how they arrived at their predictions. It can then be assumed that a similar methodology was used for the other technologies. This would give the reader more confidence in the usefulness of the analysis.
- The team performing the analysis is doing a great job mathematically and analytically, but there is no way to verify this model, so its usefulness is uncertain. The assumptions dominate everything else in the model. The assumption that American Recovery and Reinvestment Act or similar funding will continue is very unlikely, and yet it dominates the model. The most capable leaders cannot predict what will happen next year and certainly not 5, 10, or 40 years from now. The model does show that DOE's goal accomplishment will influence the outcome, but the other factors and assumptions will dominate.
- The sensitivity analysis of one or more goals being met on time or delayed by 10 years has important implications for the synergy between goals and will allow for better planning of program goals. The results show that meeting all goals would reduce petroleum use by 80% and greenhouse gas emissions by 50%, as well as produce cost and market penetration benefits.
- This project is off to a good start, but it is strongly encouraged that the study team recognize the need for automaker compliance with emissions, CAFE, and other regulatory standards in terms of the modeling. Quite simply, the automakers are not free to sell any vehicle they want. Their fleets must comply with standards, or else they will face onerous penalties. The likely societal impact in the absence of FCEVs could be a sharply reduced number of vehicle choices for consumers. As an example, Ford terminated the Crown Victoria in part because they could no longer offset the CAFE penalty. As a result, people who wanted to purchase this class of vehicle no longer had that option.

### Question 4: Collaborations and coordination with other institutions

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

- The collaboration efforts appear to be appropriate for this task.
- The team is encouraged to seek out the input of the automotive industry in terms of modeling.
- A wider group of collaborators would be preferable, but it is hard to see how much more can be done in a one-year project.
- This project's collaboration with knowledgeable technical parties is extensive. There is not enough input from social scientists and psychologists.
- This project features good collaborations with national laboratories and universities. Industrial data are available, but this would be a better project if more industrial input was sought.

### Question 5: Proposed future work

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

- This project makes no mention of future work.
- Future work for this project was not addressed in the presentation other than to say the researchers would complete the last 50% of the work.
- This project will continue and is clearly of value, but future work is not clearly delineated.
- There does not appear to be any proposed future work; this is unfortunate. Suggestions, even if not funded, would support the justifications for the work. The researchers need to say what they want to do with the tool.
- The project's future work is limited and would benefit from making stronger efforts to collaborate with more stakeholders, particularly battery and fuel cell manufacturers, as well as the fuel providers.

### Project strengths:

- This project has a good model.
- The project gives recognition to the fact that studies such as this need to be done.
- Looking at the impact of the actual program goals is a strength of this project.
- This project is focused on important "what if" scenarios that DOE must know about.
- The strengths are the knowledge and dedication of the project members.
- This project's strength is a competent and knowledgeable team that is building a usable mathematical model, which yields quantitative results for assumed input.

### Project weaknesses:

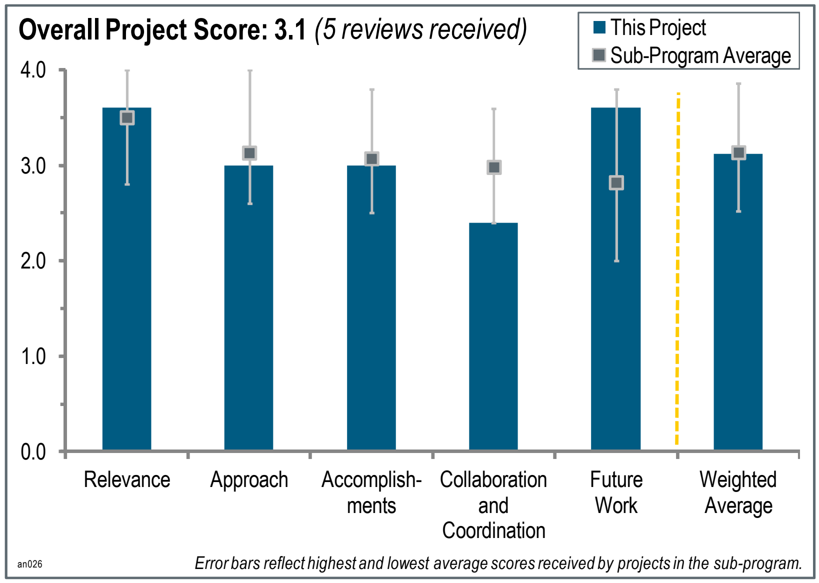
- The model results depend on assumptions for modeling that are ungrounded.
- More industrial feedback is needed for this project.
- This project is too short-term and needs wider participation by stakeholders.
- The weaknesses appear to be the lack of a clear presentation on the assumptions associated with the model and the use of this program.
- The researchers need to recognize the unique body of regulatory compliance that the automakers must address in their business and product planning.

### Recommendations for additions/deletions to project scope:

- The researchers should complete the model.

## Project # AN-026: Resource Analysis for Hydrogen Production

Marc Melaina; National Renewable Energy Laboratory




### Brief Summary of Project:

The objectives of this project are to: (1) understand the hydrogen ($H_2$) production requirements for a future demand scenario, (2) estimate the low-carbon energy resources required to meet the future scenario demand, (3) compare resource requirements to current consumption and projected future consumption, (4) determine resource availability geographically and on a per kg of $H_2$ basis, and (5) estimate fuel cell electric vehicle (FCEV) miles traveled per unit of resource.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- Anything that helps understand $H_2$ production is very relevant.
- The analysis supports basic calculations and estimates the DOE Hydrogen and Fuel Cells Program (the Program) needs in gaining a general understanding of future resource needs and related impacts.
- This project is critical to DOE's objectives because it will help to determine where the energy for 100 million FCEVs will come from.
- This project attempts to estimate the energy resources required to support the production of $H_2$ for a projected 100 million FCEVs in 2040. The project attempts to determine if sufficient energy resources will be available, and if not, how much more will be required. The project looks at three renewable energy sources (biomass, wind, and solar) and three traditional sources (natural gas, coal with carbon capture and sequestration, and nuclear). The project estimates the hypothetical need to supply 50% (10 million tons of $H_2$ annually) of the assumed 2040 demand. Such information is required to determine the viability of an FCEV transportation system. If the fuel is not available, significant resources will be required to develop the production infrastructure, which would hinder acceptance of $H_2$ FCEVs as a viable option.
- Although it may be a little premature to be looking at $H_2$ supply scenarios 30 years out, the study's design parameters were clearly spelled out: in the 2040 time frame, assume 20 million tons of $H_2$ per year will be needed to satisfy the demand from 100 million FCEVs. Given this set of conditions, the study will analyze different low-carbon sources needed to individually meet half of this demand (i.e., 10 million tons of $H_2$ per year from natural gas, coal, nuclear, biomass, wind, and solar). This would then be compared to the current consumption of that resource to put the fuel resource situation in perspective.

### Question 2: Approach to performing the work

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

- Considering the funding limitations of this project, the approach is excellent.
- The approach of using simple energy balance calculations for plant-gate $H_2$ production is appropriate for the scope of this project. It is hoped that this analysis will feed into a complete well-to-wheels analysis as well.
- Based on assumed energy conversion efficiencies (e.g., 46 kWh per 1 gge of $H_2$ [roughly 1 kg of $H_2$]), the model uses current and predicted future capacities to model the number of FCEVs that could be supported by the

different energy options. The approach was very limited in detail (in part due to the limited funding: $15,000 in fiscal year [FY] 2011 and $25,000 in FY 2012) and was somewhat confusing in the presentations.
- Simple energy balance and conversion (reforming, gasification, and electrolysis) efficiencies (with most values taken from Hydrogen Analysis [H2A] case studies) are used to determine the volume of the resource required to meet the 10 million tons of $H_2$ production value. Analysis covers production up to the plant gate only and does not include delivery or storage. This level of analysis would provide a good first-cut at resource needs.
- The approach is OK for getting a rough idea of how far a resource would need to be stretched, and if it would be enough to meet future demands. This is useful for trying to envision if there are any major limitations. However, a more useful analysis would be looking at cases where each resource has competing demands from other uses.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project has made good progress given the level of support.
- This project's accomplishments are understandably limited.
- These updated estimates are valuable and these results are a good starting point, but the analysis needs to be taken a step further to employ a more granular approach.
- The somewhat sobering analysis—that in order to utilize renewable $H_2$, renewable capacity will have to be ramped up dramatically—is critical. Using geographic data is very powerful and makes the model results much more useful. It is a little hard to see how $H_2$ competes with other energy carriers in these results.
- The results were presented in an easy-to-understand and useful format. One such format was to determine the factor increase in the use of a given resource to fulfill the target $H_2$ demand compared to the current use of that resource. Thus, for non-renewable resources, consumption of natural gas, coal, and nuclear would need to increase by factors of 1.05, 1.10, and 1.44, respectively. The corresponding factors for biomass, wind, and solar would be 1.33, 2.53, and 6.75, respectively. For the renewable resources (biomass, wind, and photovoltaic solar), the geographic distribution of the resource was shown on county-level maps for the United States. This representation of the resource serves to visually show where the resource is versus where $H_2$ is needed (i.e., raises the question of how best to transmit energy over long distances, as electricity or as $H_2$ for wind and solar). The results are based on an updated and consistent calculation of the technical potential of the individual resource.

### Question 4: Collaborations and coordination with other institutions

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

- This project has no external collaborators. Even though this is a very small effort, it would be good to see additional national laboratory and industry input.
- In this project, there is indirect collaboration with a broad group of researchers and stakeholders through making these analyses consistent with H2A case studies, and having the results reviewed by knowledgeable researchers.
- The collaboration in this project would be better if the report had been accepted for publication.
- It would be useful for this project to collaborate with other model developers, so that values and findings from this analysis could feed into more detailed modeling and analyses conducted through other efforts (and vice versa).

### Question 5: Proposed future work

This project was rated **3.6** for its proposed future work.

- The proposed future work to expand beyond light duty vehicles (LDVs) and look at regional variations and constraints is very exciting.
- It is good to see that this initial analysis will be relied upon more and investigated on a more detailed basis for future scenario analyses.
- In addition to publishing the results of the analyses already conducted, future work will address regional variations, constrained scenarios, and inclusion of non-LDV transportation energy demands.

- This project uses realistic and consistent units. With the level of support and effort available, this project should not be listed as a separate, stand-alone project. It should have been a task folded into one of the other $H_2$ analysis projects. The researchers should either increase the support to enable a strong effort, or fold it into another project.

### Project strengths:

- The project addresses resource availability, which is a key question.
- This project has a simple, straightforward, and analytical approach.
- This project's strengths are its energy balance approach and the current regional data.
- A considerable amount of work has been done with very limited resources.
- This project gives an overall framework and scope for thinking about resource issues as they relate to $H_2$ production.

### Project weaknesses:

- This project needs more input and resources.
- There is not much substance in the results to date.
- This project makes a rough estimate on a national scale. Taking the research a step further by looking at regional variations, other constraints, the variety of uses for $H_2$, etc. will provide more value.
- The current (2012) and projected (2040) consumption data is presented in different units (trillion cubic feet for natural gas, ton of coal, GWe for nuclear, ton/year for biomass, and kWh/year for wind and solar). Future comparisons should use consistent comparisons, at least for those that directly produce electricity (nuclear, wind, and solar). The presentation quotes the increases in projected consumption that will be needed as being from 1.05 to 6.75 (slides 1 and 12), but then lists the increase as a percent increase on slide 14. The researchers need to be clear whether it is a 6.75% increase or a factor of 6.75. The comparison for wind and solar is based on installed capacity, not actual production, which is considerably lower. The authors should base their analysis on actual production.
- This reviewer sees no weaknesses.

### Recommendations for additions/deletions to project scope:

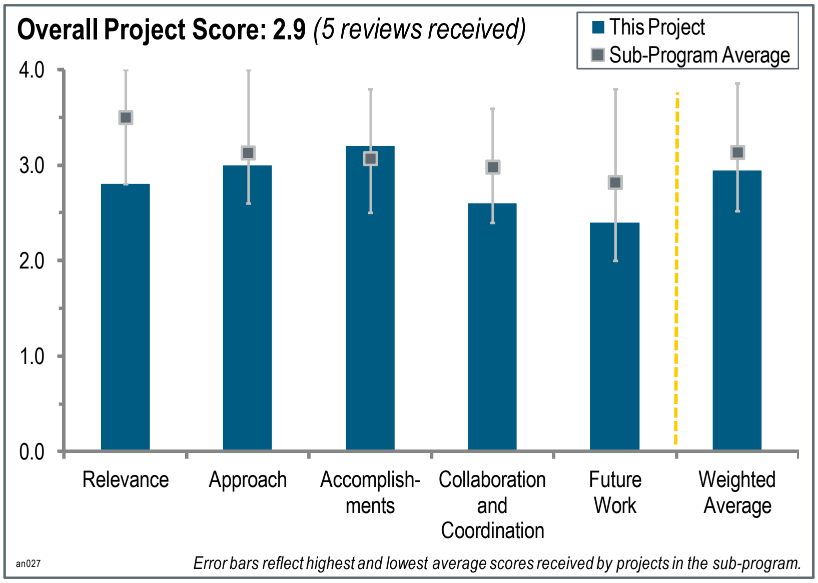
- The researchers should concentrate more on comprehensive projects instead of numerous small projects.
- The researchers should continue this effort by going into more detail and using those details to inform other Program models and analyses.
- The principal investigators should attempt to use consistent units to estimate current and projected consumption especially for resources (nuclear, wind, and solar) that all produce electricity (kWh). For nuclear, it is easy to project 102 GWe to kWh/year (roughly 700 billion kWh/year, assuming 80% availability).
- This reviewer has no recommendations.

## Project # AN-027: Cost, Energy Use, and Emissions of Combined Hydrogen, Heat, and Power Tri-Generation Systems

Mark Ruth; National Renewable Energy Laboratory

### Brief Summary of Project:

The objectives of this project are to: (1) quantify levelized cost and greenhouse gas (GHG) emissions from tri-generation (combined heat, hydrogen, and power [CHHP]) systems for various fuel cell types, building types, and building locations, and (2) develop a methodology for Macro-System Model (MSM) users to create optimized CHHP scenarios easily.




### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.8** for its relevance to DOE objectives.

- CHHP is needed to round out complete models of alternative energy generation approaches and fuels.
- This project evaluates the possibility of using tri-generation to produce heat, electricity, and hydrogen ($H_2$). This is a potentially exciting possibility, but is perhaps not as critical to DOE as other approaches.
- This project models the overall system cost and GHG emissions of a commercial building tri-generation fuel cell system capable of providing $H_2$, heat, and power from natural gas. This includes regional costs, upstream energy usage, and emissions in the fuel cell model.
- The relevance of this activity is not clear. Optimizing a product for an application that would not be cost effective to the owner is confusing. It does not make sense to operate an expensive product in a manner that would not meet the owner's perceived needs. Evaluating the use of a product in regions where it would clearly be at a severe disadvantage does not seem useful.
- This project is an attempt to assess the cost of $H_2$ and GHG emissions from CHHP systems using high-temperature molten carbonate or phosphoric acid fuel cells (MCFC or PAFC). This appears to be a forced fit of using fuel cells to produce $H_2$ as an extra credit to reduce overall costs. As the results show, the overall costs are not likely to be reduced. With the current low prices of natural gas (as a source of $H_2$ by conventional steam methane reforming [SMR]/pressure swing absorption), the added complexity of CHHP systems would argue against such systems becoming commercially attractive.

### Question 2: Approach to performing the work

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

- This project is a very good approach to using the MSM suite together with the fuel cell power model.
- This project is a good application of existing models in an MSM that does not require the development of individual models.
- Model construction approach using the MSM framework is good, but the choice of the sample fuel cell type and size/application is unrealistic. Many of the assumptions made are difficult to verify.
- The project approach was to add a fuel cell power model (for PAFC and MCFC) for CHHP to the MSM. An analyst could then use the MSM to assess regional (Seattle, Los Angeles, Chicago, and Baltimore) costs, upstream energy use, and emissions.
- The approach appears to be valid. However, the assumptions on how a product would be optimized invalidated the results. The customer will normally endeavor to maximize the energy savings for CHP devices in order to

reduce the overall operating costs. Exceptions are usually limited to special applications, such as isolation from the electrical grid, premium power (computer systems), electrical grid stability/reliability, high demand for low-grade or moderate-grade thermal energy (hot water and forced hot air), etc. Regional uses of CHP products (barring the exceptions) are usually predicted by the spark gap. The spark gap is the cost of electricity from the grid versus the cost of heating and electrical generation from alternative fuels (usually natural gas). Currently, this generally limits the market to the Boston to Washington corridor and California. With the advent of shale gas and coal syngas, this may grow to include the Midwest. Areas where the predominant energy source is hydro/coal are usually non-completive.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- The project showed that capital was the number one cost driver for all fuel cells. The differences in load following ability and heat generation became apparent from the model, allowing systems choices to also be determined by application.
- The progress to date is questionable due to the assumptions. However, if the assumptions were reevaluated and revised, and the models rerun, the results would be interesting if not outright useful.
- The models were completed and exercised for two fuel cell types with reasonable results. The sizing of systems for lowest $H_2$ cost was not optimal. The difficulty is the balance of $H_2$ generation, electricity use, and heat generation. GHG reductions are minimal and not large enough to be a market driver. $H_2$ generation costs are lower at low rates compared to SMR.
- Progress is reported to be at 80%. The results were presented for two different size buildings/power requirements (320 kW and 1,440 kW) and two classes of fuel cells (PAFC and MCFC). The results presented focused on four different regional locations (Seattle, Los Angeles, Chicago, and Baltimore) that contain a variety of electricity costs, climates, and heat load scenarios. Comparisons were made that indicate MCFC systems are effective for load following, while PAFC systems are optimum for $H_2$ and power generation. Capital is the primary cost driver for these units, and variable costs (rent and labor) are secondary for the smaller MCFC units.
- The researchers have exercised the updated MSM in a variety of analyses for the two fuel cell types and several different building applications. For all of the systems, the capital costs were the major cost drivers. Minimizing the cost of $H_2$ resulted in significantly higher overall costs for heat and electricity for all of the scenarios analyzed. These overall costs ranged from 20% to 231% higher than the base case of using grid electricity for power and natural gas for heat and $H_2$. The corresponding reductions in GHG emissions range from a high of 40% to a low of -17% (i.e., the GHG emissions for that case were actually higher with the CHHP than the base case).

### Question 4: Collaborations and coordination with other institutions

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

- The collaboration appears to be inadequate. It appears to have been limited to data on a product rather than expected or demonstrated customer usage.
- Collaborators included industry (Fuel Cell Energy), national laboratories (the National Renewable Energy Laboratory and Argonne National Laboratory), and the U.S. DRIVE Partnership's Fuel Pathway Integration Technical Team.
- This project has good DOE interactions and collaborated with one industry player. The researchers should have also collaborated with UTC Power.
- The project coordinates with other national laboratories, but it desperately needs input from stack manufacturers of the various technologies to see if what is proposed is feasible now or in a short time frame.

### Question 5: Proposed future work

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

- The proposed future work would justify additional funding, especially the load leveling applications.

- The project's report should be finalized and the MSM should be updated.
- The only specific future work presented for this project was finalizing the project report. In general, the researchers expect to continue to further develop and exercise the MSM for a variety of applications.
- Additional funding is not planned for fiscal year 2013. The plans are to wrap up the project, even though additional options were proposed.
- The lack of proposed future work is disappointing. Correcting the assumptions with a focus on applications where CHHP is cost effective (hospitals, nursing homes, college dormitories, sports facilities, etc.) would be interesting. The researchers should focus on applications that have the need for thermal energy and have a favorable spark gap.

### Project strengths:

- The strengths are the knowledge and dedication of the project members.
- The models appear sound, but verification is left undone.
- Focusing on tri-generation was certainly an interesting scenario that needed to be looked at.
- Integrating existing sub-models to optimize the use of tri-generation concepts is valuable and is needed to guide the development of novel concepts.
- The researchers completed quite a bit of work with limited resources. The results are illustrative of the challenges in implementing CHHP systems in a manner that would be competitive with conventional technologies.

### Project weaknesses:

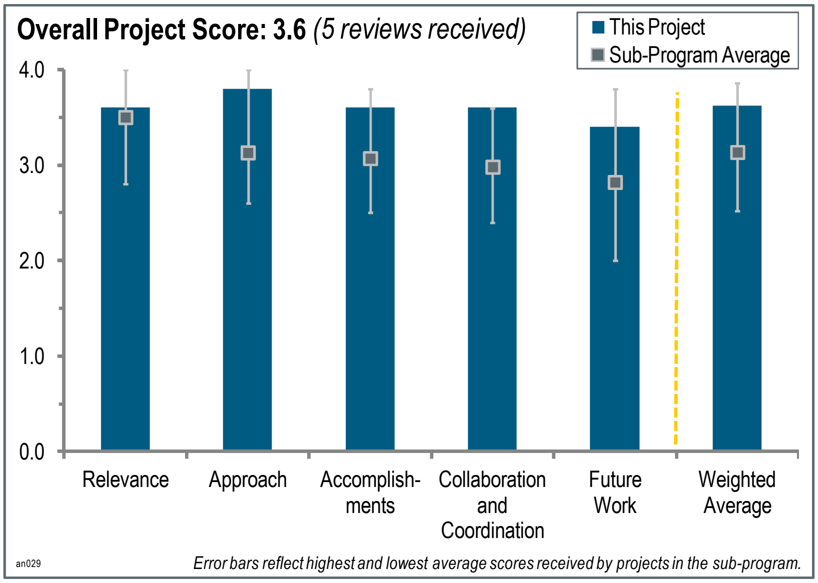
- There are weaknesses, but they can be quickly corrected.
- This project really needed some industrial input.
- The choices and assumptions made, such as the fuel cell systems, are a weakness in this project.
- While modeling is valuable, the results suggest the application of tri-generation systems only makes sense if GHG emissions are factored in. Based on the cost of $H_2$, power, or heat, a tri-generation unit does not appear to be cost effective. This may be a strength or weakness depending on the use of the models to assess a given pathway. Proponents of tri-generation may not see the results positively.
- This project had no weaknesses.

### Recommendations for additions/deletions to project scope:

- This effort should continue to be supported.
- CHHP should remain a possibility for a system approach, but other techniques for optimizing heat, $H_2$, and electricity balance should be addressed.
- The project is nearing completion, so there are no recommendations for additions or deletions to the scope of the project.

## Project # AN-029: Employment Impacts of Early Markets for Hydrogen and Fuel Cell Technologies

Marianne Mintz; Argonne National Laboratory




### Brief Summary of Project:

The objective of this project is to produce the Jobs and Output Benefits of Stationary Fuel Cells (JOBS FC) model, which provides a means for calculating employment and other economic implications of fuel cell investments. The JOBS FC model translates investment and operations expenditures into direct, indirect, and induced jobs and economic activity.

### Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.6** for its relevance to DOE objectives.

- Given where things stand on a deployment phase of hydrogen ($H_2$) technologies, getting a handle on the impact on job creation is very relevant to the mission of the DOE Hydrogen and Fuel Cells Program and will help the community to understand the economic (jobs) impact.
- This model is definitely needed, because jobs and exports have become a focal point for legislators and administration. Helping raise the profile of the potential of fuel cells and related jobs is important for continued support as well as the implementation of actual installations and deployments.
- Employment and jobs calculations are critical criteria for project valuation and continued public funding. This value attribute is often qualified, but not often quantified. This project will provide an opportunity for uniform quantification and more accurate project valuation.
- It is crucial that industry and government continue to support modeling jobs. The PI has done a wonderful job of creating a transparent tool for stakeholders to use. It is unclear whether demonstrating economic benefits of fuel cells is part of DOE's program plan or research and development (R&D) objectives, but it should be.

### Question 2: Approach to performing the work

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

- This project has very thorough models that seem to be taking in user and tester feedback and addressing the issues raised.
- So many of these types of tools are too hard for people to use. The principal investigator (PI) and research team made this tool accessible and easy to use.
- This is a very good application of the JOBS FC tool. The data input is quite thorough, with its high-fidelity treatment of the input supply chain. However, the modeling is somewhat limited in its ability to provide understanding of regional differentiations. Admittedly, this is driven mostly by a lack of relevant data. The PI would still be encouraged to address this issue.
- The Regional Input Output Modeling System (RIMS) model is an appropriate substitute for the Impact Analysis for Planning (IMPLAN) model for direct, indirect, and induced estimates. Targets for forklifts, fuel cell backup power, and combined heat and power (CHP) are also appropriate, but they could be expanded as needed. Online offerings of the model will provide opportunities for universal and consistent project valuation. Regional variations and the use of supply chain information is of high value.

### Question 3: Accomplishments and progress towards overall project and DOE goals

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

- This project is on schedule to complete the tasks and has made impressive progress since the 2011 Annual Merit Review (AMR).
- This tool has grown in scope since the presentation at last year's AMR. It is important that scope creep does not keep the tool from being finished.
- This work (and results) is very timely as $H_2$ technologies increase their presence in the marketplace. Counting jobs is important to government leadership, so having a handle on this metric is important.
- Refining the project to use RIMS as a substitute for IMPLAN appeared to be challenging, but necessary. This modeling is appropriate and is now nearly ready to estimate economic and job creation impacts of the American Recovery and Reinvestment Act (Recovery Act) and other public funding of projects. Supply chain information and regional variations are complete and ready for online access.

### Question 4: Collaborations and coordination with other institutions

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

- This project has a wide reach of stakeholders for peer review/testing.
- This project has an impressive list of collaborators and stakeholders.
- This project has good input from industry and is working with state and regional programs for data and validation.
- Collaboration and coordination for this project is very good, but the nature of the model will require continual updating, additional testing, and refinement of model variables with stakeholders, original equipment manufacturers (OEMs), and institutions. This work will be necessary initially to validate the model and of continual value to confirm consistency and accuracy over time.

### Question 5: Proposed future work

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

- It is good to increase past Recovery-Act-funded projects.
- This project definitely needs to expand to include solid oxide fuel cells (SOFC), $H_2$ stations, and transportation as proposed. Anaerobic digester gas/biogas is a definite market to include as well, because that could be used not only for utilities and municipalities, but also for factory farms, wineries, and other industrial processes that are dealing with image and waste problems.
- This project needs to get into position to provide information on jobs and economic impacts of the infrastructure rollout in aggressive states such as California and New York. When asked, the PI recognized this and suggested that movement in that direction was on the table, but this reviewer would like a more concrete plan of action for doing this. This work focuses on jobs created by current deployment activities. It would also be good to make educated projections on the job market impact driven by the planned rollout of $H_2$ technologies. The impact on job creation by the early development of infrastructure elements and the introduction of hundreds of thousands of vehicles would be very interesting. This project should work to get a handle on the future job impacts as well as the current impacts.
- User training, data valuation, and documentation are on schedule. Comparing job-creation value with other technologies (photovoltaic, wind, and conventional, etc.) would be of value. Expanding the models for other applications, such as non-CHP electric only, SOFC, transportation, high-temperature baseload polymer electrolyte membrane fuel cells, and combined heat, hydrogen, and power is appropriate and of value. The location of the tool at other job estimation websites (i.e., Jobs and Economic Development Impact [JEDI] sites) might allow for comparative analysis for various technologies. Additional information on sensitivity analysis would have been helpful.

### Project strengths:

- This project is producing clear, easy-to-use web-based tools and has good data transparency and coordination with industry and regional programs.
- This is a much-needed tool to advance industry and helps position the technology as more than environmentally friendly, which is important in the current climate.
- This is a great application of the JOBS FC tool, with solid high-fidelity data input. Including first- and second-order effects is very important to understanding the ripple-down effect on jobs from introducing this technology.
- This model provides valuable economic and job creation information to justify public funding. The work combining clean energy, economic development, and job creation is of high national value. The initial targets are appropriate and future work is consistent with DOE's goals for technology deployment.

### Project weaknesses:

- The researchers need to ensure the tool gets in front of the right people: a wide audience with potential customers, lawmakers, etc. A marketing/promotion/education plan was not addressed.
- As with every good project, scope creep can make the project never ending. It also becomes easy to get too detailed. The researchers should be cautious of this.
- This task should broaden its geographical reach to include regions that are particularly aggressive in their rollout plan, such as New York and California. This project should embrace extrapolation into the future to get a handle on the job impact by the planned 2015 infrastructure and vehicle rollout.
- Validation through testing with end users and OEMs is needed for continual model refinement. A more detailed explanation of sensitivity analysis would have been helpful. More funding will be needed for model updates and refinement.

### Recommendations for additions/deletions to project scope:

- This project should include $H_2$ stations.
- Marketing/education, expansion of fuel cell types, and applications are needed.
- Researchers should establish model sites with other job calculators/estimators (i.e., JEDI) to allow comparative analysis and track hits on the model. A white paper on comparative analysis with other technologies for job creation would be of interest. Researchers should update the model as appropriate and expand the model's scope for additional technologies including SOFC, non-CHP SOFC, transportation applications, and high-temperature PEM fuel cells. The researchers should also consider the expansion of a sub-program for financing.

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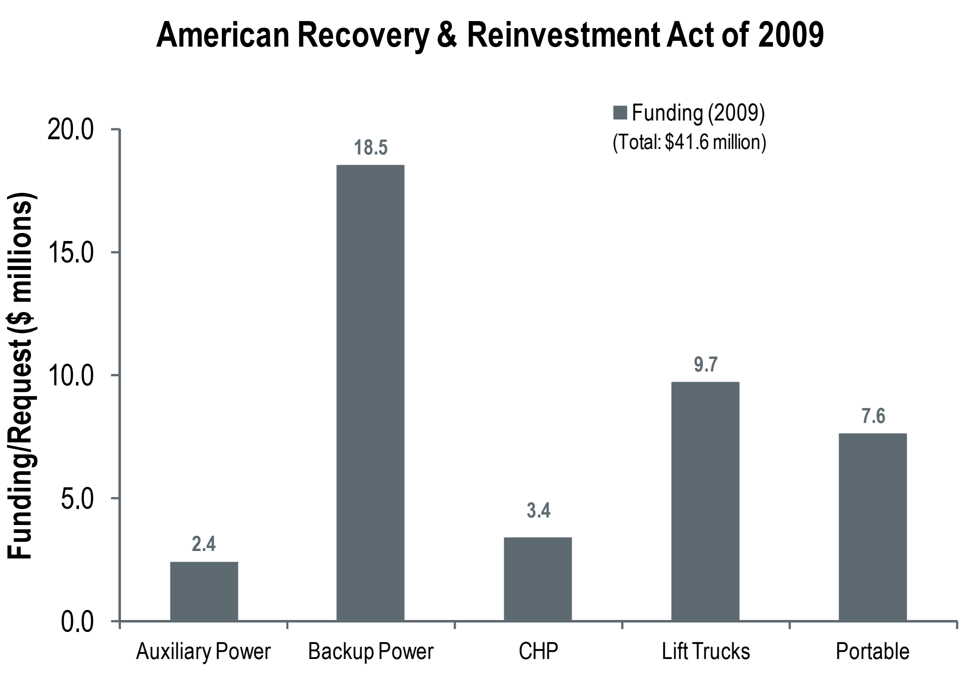
# 2012 — American Recovery and Reinvestment Act
# Summary of Annual Merit Review of American Recovery and Reinvestment Act Activities

## Summary of Reviewer Comments on the Recovery Act Activities:

This review session evaluated the projects funded under the American Recovery and Reinvestment Act of 2009 (Recovery Act) for enabling fuel cell market transformation. The Recovery Act projects included the development and deployment of a variety of fuel cell technologies including polymer electrolyte, solid oxide, and direct-methanol fuel cells in auxiliary power, back-up power, combined heat and power (CHP), lift truck, and portable-power applications. The Recovery Act projects are considered by reviewers to be well aligned with the goals and objectives of the Recovery Act and the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program. In general, the projects were judged to be making significant progress toward fuel cell development and deployment, especially considering the additional industry-funded (i.e., without DOE funds) deployments completed or planned.

## Recovery Act Funding by Technology:

In April 2009, DOE announced the investment of $41.6 million in Recovery Act funding for fuel cell technology to accelerate the commercialization and deployment of fuel cells and to build a robust fuel cell manufacturing industry in the United States with accompanying jobs in fuel cell manufacturing, installation, maintenance, and support services. Twelve grants were competitively selected and awarded to develop and deploy a variety of fuel cell technologies. These projects (denoted at the Annual Merit Review by the label “H2RA”) are addressing the aforementioned objectives as well as the overall Recovery Act goals of creating and saving jobs, spurring economic activity, and investing in long-term economic growth. The cost share provided by the project teams is approximately $54 million, more than 56% of the total cost of the projects.




## Majority of Reviewer Comments and Recommendations:

Seven of the 12 deployment projects and one data collection and analysis project in the Recovery Act activities had oral presentations; two projects had poster presentations. Of these, only five of the projects were reviewed, because the remaining projects were either completed or nearly complete (see fiscal year [FY] 2011 and FY 2010 proceedings for prior year reviews). In general, the reviewer scores for the Recovery Act projects were good, with scores of 3.7, 3.0, and 2.1 for the highest, average, and lowest scores, respectively. Three of the five projects had a score of 3.0 or higher. The scores are indicative of the technical progress that has been made since the project grants were awarded in late FY 2009 or early FY 2010.

**Auxiliary Power:** One project in this area, involving the development of a diesel auxiliary power unit to power hotel amenities for use on Class 8 sleeper trucks, was reviewed, receiving a score of 3.0. The project was seen as being on a clear path to commercialization and addressing a huge potential market for fuel cells, especially with the anti-idling regulations in many states. The reviewers felt the delays in the project and having only one test unit were hindering progress. It was also recommended that the project team investigate additional market applications.

**Backup Power:** Two projects addressing 72-hour backup power for cellular communication towers and U.S. Department of Defense sites were reviewed, with an average score of 3.1. Overall, the reviewers thought the deployment of a large number of fuel cells into the market was a significant contribution from these projects. It was recommended that the project with deployments at cellular towers look into reformer-based, on-site hydrogen production technologies as a potential way to open up additional deployment sites.

**Combined Heat and Power (CHP):** One project in this area, addressing residential and light commercial applications, was reviewed, receiving a score of 2.1. The project was seen as a good potential avenue for reducing energy use in homes. The reviewers were concerned with the membrane electrode assembly failures and recent no-go business decision by the project lead on commercial deployment of these units, but recognized the company's efforts in salvaging the project by transferring deployment responsibilities to another company. It was recommended that more cost/benefit analyses be done for this market.

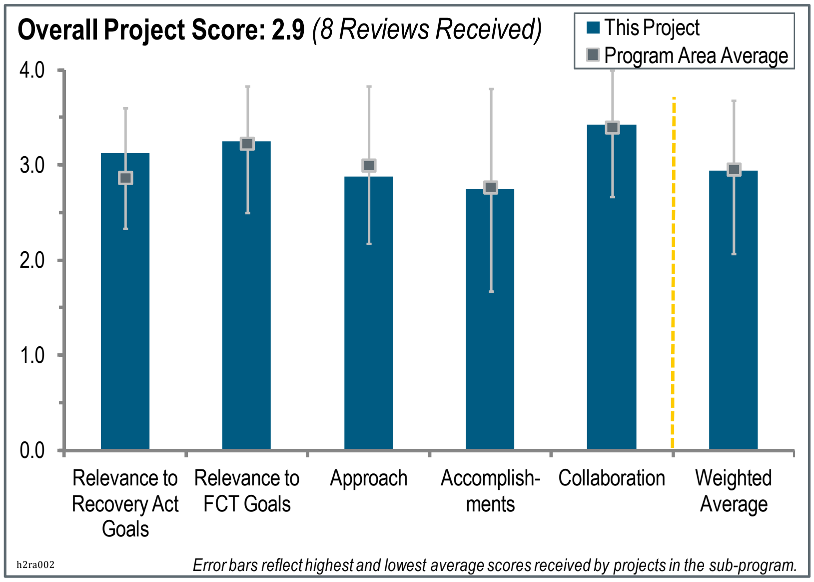
**Data Collection and Analysis:** One project in data collection and analysis was reviewed, receiving a score of 3.7. The reviewers thought the project provides valuable data on a number of fuel cell deployment sites and partners through an easily understood set of products. It was recommended that this analysis effort continue, because it is seen as a huge benefit to the fuel cell industry. The reviewers also recommended data collection and analysis be performed for internal combustion engine and battery applications.

## Project # H2RA-002: Solid Oxide Fuel Cell Diesel Auxiliary Power Unit Demonstration

Dan Hennessy; Delphi Automotive

### Brief Summary of Project:

The objectives of this project are to: (1) define system specifications and commercial requirements for a solid oxide fuel cell (SOFC) auxiliary power unit (APU) to be used with a diesel engine; (2) design, build, and test the diesel APU system; (3) demonstrate a vehicle using the system for one year; and (4) analyze the data from the demonstration.




### Question 1a: Relevance to overall American Recovery and Reinvestment Act of 2009 (Recovery Act) goals

This project was rated **3.1** for its relevance to overall Recovery Act goals.

- This project can significantly affect the trucking industry and create a new aftermarket manufacturing product.
- The project is relevant and will make contributions to Recovery Act goals.
- This could get fuel cells and some companies into a new market.
- This project states that it will create/retain nine jobs. Because of a no-cost extension, these jobs are being retained longer than the original duration. The project is making relevant contributions to Recovery Act goals.
- Delphi claims a 40%–50% fuel efficiency improvement as compared to conventional diesel APUs. If true, and if life-cycle costs can be held comparable or better than conventional APUs, this should be attractive to tractor trailer owners and provide environmental benefits that would make it attractive outside the United States as well as domestically (which would benefit net U.S. job creation).
- The project's relevance to Recovery Act goals is fair. This is less of a "shovel-ready" project than a new product engineering effort. On the plus side, the retention of engineering/technical jobs is a worthy thing, particularly through stimulating $2.4 million of in-house investment. The highly structured, and very professional, approach seems to give it a good chance of creating some manufacturing jobs in the future—if the market accepts the product.
- The commercialization of SOFC APUs for long-haul Class 8 sleeper trucks would add a significant number of jobs because there are many of these vehicles on the road. This is not just a potential small niche product being addressed with Recovery Act funding. Long-haul vehicles are a principal means of moving goods in this country. There are also 30 states with anti-idling legislation that do not allow the drivers to leave their diesel engine on. The Technology Readiness Level of SOFC APUs was and still is too low to expect significant economic activity and job growth in this industry any time soon.
- Delphi went from 18 jobs created/saved in 2011 to nine jobs created/saved in 2012 for this project. With the significant economic potential that was orally described by the presenter (expected cost competitiveness with internal combustion engine [ICE] APUs due to system simplification and newly approved regulations, 2014–2018 National Highway Traffic Safety Administration/U.S. Environmental Protection Agency standards that basically guarantee a market), the number of jobs created/saved is small. The potential is there, but it was not presented or emphasized in the presentation.

## Question 1b: Relevance to the U.S. Department of Energy (DOE) Fuel Cell Technologies (FCT) Program's Recovery Act project goals

This project was rated **3.3** for its relevance to the FCT Program's Recovery Act project goals.

- The project is relevant and will make moderate but significant contributions to FCT Program Recovery Act project goals.
- The project has laid out a reasonable execution plan to achieve the stated goals. The project is behind schedule due to desulfurization capabilities limiting the implementation of the product.
- The DOE FCT Program has both Technology Validation and Market Transformation sub-programs, both of which assist emerging markets through demonstration and business case validation. This is a large potential market that could jump start the SOFC industry and provide environmental benefits as well. The risk to industry is still too great to invest solely in this technology—especially given the need to desulfurize the diesel fuel and the lack of data on how a fuel cell APU could stand up to the rigors of real-world road conditions.
- The project is clearly relevant to FCT Program goals and is squarely aimed at commercialization. This is an interesting niche market, and the presenter provided a very clear development and deployment plan. This reviewer believes that truckers and truck manufacturers will accept the unit. Sales could become quite significant as "anti-idling" regulations take full force in the next few years.
- DOE's Recovery Act seed money has induced Delphi to invest beyond its obligation to develop and demonstrate this technology. Without that seed, it was not apparent at what rate (or if) this technology would have been developed. Demonstration testing and follow-on life-cycle cost estimates will provide insight into the timing and market potential of the product and, ultimately, jobs that might be created/saved. It appears, at this point, that Recovery Act funding did accelerate development.
- SOFC APUs have the potential for significant market penetration, far beyond truck APUs; so this project is critical to help advance the technology beyond the research and development stage and into a marketable product that Delphi can sell or license.

## Question 2: Development and deployment approach

This project was rated **2.9** for its development and deployment approach.

- The appropriate milestones and schedule were identified, and barriers and risks were addressed. The effort is likely to achieve project goals, but the approach could be improved.
- The project approach is good, but there was no "Plan B" for handling supplier issues and delays. Good project management has contingency plans for this type of issue, such as alternate suppliers or technical paths.
- The timeline laid out for the project is reasonable but has been readjusted due to desulfurization issues that are delaying deployment of the technology.
- The desulfurizer risk probably should have been recognized earlier, but this reviewer liked the solution of removing the desulfurizer and going with reduced performance.
- The project has well-defined milestones; however, it has encountered technical difficulties that have created substantial delays. Current progress appears to follow the updated schedule fairly well. The project has an approach to resolving the current delay due to desulfurization, which provides some confidence that the project will be completed in the second quarter of calendar year 2013.
- Expanding the project to more than one unit for demonstration testing would be desirable because of the risk associated with unit failure, which will delay the project significantly. Two to five units would give better data and provide an average, plus speed up lessons learned if failures of specific components are common or incidental. Barriers and risks were adequately addressed; although, due to the delay, the presenter was not completely clear what the potential new barriers and risks are because it will fall outside the time frame for Recovery Act/DOE input. In addition, this reviewer suggests giving consideration to testing the unit in (hybrid) transit buses, which are at a similar weight class as Class 8. These buses have a 16–20 hour/day operational cycle in large cities; return to the same base at the end of the day; and have significant APU load for inside lighting, air conditioning, heating, etc.
- The approach of building the APU based on PACCAR-generated requirements and testing it at various levels with an ultimate year-long road test demonstrates the seriousness with which Delphi wants to commercialize this technology. The fact that overcoming barriers and risk management are a consideration, coupled with the fact

that this project has had significant delays, shows that perhaps the risk of balance-of-plant component issues—in particular the desulfurization problem—were not managed as well as they could have been. Teaming with PACCAR—one of the top four original equipment manufacturers (OEMs) for Class 8 sleeper trucks—was a great approach that will provide a chance for the truck OEM to understand and witness the performance of a SOFC APU. The fact that it will be a Wal-Mart truck testing this APU gives a major user of long-haul trucks a chance to evaluate the technology. Should the demonstration be successful, Wal-Mart could use its market power to insist on clean APUs for the trucks that carry their products.

- The technical development plan seems well thought out; the reformer-solid oxide approach is now mature enough that a product development effort such as this one should have a high probability of success in the hands of a highly professional organization such as Delphi. However, the significant schedule slippage causes great concern. Better planning might have identified the potential for problems with the desulfurizer and reduced the slippage caused by this and other "balance of plant" problems. Commercial competition with combustor-based APUs is a big concern. These could be less costly (depending on scrubber technology) and probably would have comparable or better fuel efficiency. This reviewer felt that answers to questions on this regard could have been better answered. Perhaps a slide explicitly comparing the total life-cycle costs of both approaches side-by-side could be included next year.

### Question 3: Technical accomplishments and progress

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

- Overall progress is acceptable.
- The project has experienced significant delays but appears to be on track to complete the last 12 months. Delays and job creation have been quantified.
- The project made significant progress toward objectives and overcoming some barriers. The issues of desulfurization and start-up need more attention.
- Progress toward achieving the project's objectives and milestones was clearly demonstrated. It was not completely clear to this reviewer how fuel economy is improved compared to running on a comparable, commercially available ICE solution. Also, the number of jobs projected could have been explained better.
- This project was a good demonstration of 28% efficiency. One reviewer believes this is probably due to operation on low-sulfur diesel, and wants to know how much the efficiency will drop once the desulfurizer is included into the system. This reviewer also wondered, if the functional life of the desulfurizer is only six months, what the expected cost is to replace it.
- Inordinate delays have been experienced. This was supposed to be a 30-month project that would have been completed in February 2011 and include a one-year road test. As of the DOE Hydrogen and Fuel Cells Program Annual Merit Review briefing, the road test had yet to commence. The project has performed well in other areas. Efficiency tests are complete, with 28% efficiency measurements validated. The load cycle test is complete. Operations instructions, service plans, and safety plans are all in place. The problems with mean time between replacement of sulfur absorbent beds should have been noticed earlier, and TDA Research, Inc. should have put a solution in place in a timely manner. Delphi is still committed to seeing the road test through. Delphi is also addressing the desulfurization and start-up issues in their next work plan.
- Impressive technical material was presented, but looking at the milestone chart, the project seems to have encountered considerable delays. It is not clear that simply pulling the desulfurizer out of the system and counting on the reformer to do the job will actually work. Thus, there still seems to be significant technical risk in the project, even at this late date.

### Question 4: Collaboration with other institutions

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

- It appears that there is great collaboration and support from the end-user community.
- The project has effective collaboration with other institutions that will enhance the probability of success of the effort.
- Signing PACCAR onto the project was an excellent choice and will prove to be a tremendous asset if the APU actually can be fielded. TDA Research, Inc.'s contribution seems to have been marginal.

- PACCAR and TDA Research, Inc. were identified as partners. Some comments were made regarding Wal-Mart's relationship for demonstration.
- It appears that partners PACCAR, TDA Research, Inc., and Wal-Mart are good choices; all are driven to improve fuel use by Class 8 trucks to be increasingly competitive.
- This project had a key collaboration with PACCAR, one of four Class 8 sleeper truck OEMs with significant market share. Having PACCAR develop requirements to include operator interface ensures the ultimate fuel cell APU product will have fewer customer-related problems as it moves toward commercialization. There is no collaboration with other entities beyond TDA Research, Inc. and PACCAR. This is understandable to a degree in the sense that Delphi wants to be first to market with this technology, but Delphi might have considered more than just one company (Wal-Mart) for testing the SOFC APU.
- The collaboration with PACCAR appears to be strong. Because (according to the slides) PACCAR has only 24% of the market, the principal investigator (PI) should explain the business plan going forward and whether PACCAR has an exclusive right to sell on this or if the other 76% of the market will also be able to buy this APU as an option.

### Project strengths:

- A strength of the project is the expertise in the manufacturability of fuel cells.
- This project has good commercial focus and its path to deployment is clear.
- The fuel cell product has market potential and there is a reduction in the number of components. Progress has been made, lessons have been learned, and decisions have been made on a new direction. The timeline appears to match the newly adopted emission regulations timeline.
- A strength is the size of the potential market that this project will address. The cost share from Delphi was significant ($2.4 million), which shows commitment. The methodical, disciplined approach shows a desire to go through commercialization steps. The team of Delphi, PACCAR, and TDA Research, Inc. covers all the major areas needed.
- The heavy-duty truck and aircraft APUs have been identified as high-potential applications/markets for fuel cells; this project significantly advances developing a product for and testing such potential.
- The SOFC technology is a good area to pursue a real-world market and provide leverage to get the technology pushed forward. The involvement of the user community is also notable.
- There is a clear market that is ready and (should be) willing to accept this product when it is introduced into the market. The project has a clearly laid out plan for bringing this to market.

### Project weaknesses:

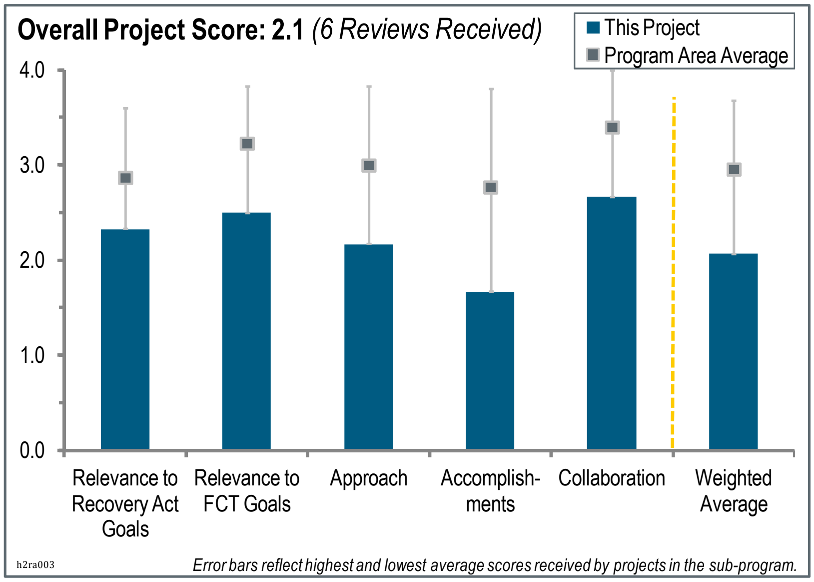
- The issue with fuel desulfurization is a weakness.
- A weakness was the 1.5-year slippage on a 3-year schedule. Also, there is still no clear solution to the sulfide removal problem.
- The project's weakness is risk management.
- There is a low number of test units and the project is missing job creation projections. Another weakness is the comparable APU ICE unit performance numbers (fuel economy, emissions, etc.) to the SOFC APU.
- Schedule slip continues to be a problem. Managing risk of desulfurization issues with diesel could have been done better, sooner. There are still no road test results more than three years after award of this "30 month" project. DOE needs this data to share with the public, regulators, the U.S. Department of Transportation, and other stakeholders. This could enable even more impactful anti-idling efforts because it would have been demonstrated that clean APUs are available or will soon be available. Everyone benefits from cleaner air—the sooner the better.
- Significant delays (1.5 years) have caused the energy-saving and jobs impact of this potential product to be delayed into the future. The project needs to be working with other suppliers or developers on a desulfurizer because this is such a critical technology. It is unfortunate that at this point the project should have already had 6 months of field experience with the prototype, but instead it is still not yet in the customer's hands.

### Specific recommendations:

- Desulfurization created problems, but the environmental problem was not an issue because the total sulfur content was less than 10 ppm.
- Commercial competition with combustor-based APUs is a big concern. These could be less costly (depending on scrubber technology) and probably would have comparable or better fuel efficiency. Perhaps a slide explicitly comparing the total life-cycle costs of both approaches side-by-side could be included next year.
- The PI should acquire real-world test data even if it means compromising on the first system's design and start providing more detail on component failures once testing starts.
- A recommendation is to compare fuel economy and emission improvements when presenting the product. In addition, the PI should consider additional units for demonstration/testing, consider evaluating other markets based on emissions regulations in the United States, and consider additional market applications and the potential to scale up the fuel cell to become the main power source for Class 8 or similar-sized vehicles.
- The team should work to incorporate the APU more fully with the truck electrical and thermal system in the future. There are many integration synergies that are missing due to the "aftermarket" approach currently being pursued. The team should evaluate whether this same system (or a variant of it) could be used on refrigerated trucks, recreational vehicles, emergency field hospitals, etc.

## Project # H2RA-003: Highly Efficient, 5-kW CHP Fuel Cells Demonstrating Durability and Economic Value in Residential and Light Commercial Applications

Donald Rohr; Plug Power Inc.




### Brief Summary of Project:

The objectives of this project are to: (1) substantiate the durability of 5-kW combined heat and power (CHP) fuel cells through system design and modeling; (2) verify the technology and commercial readiness of 5-kW CHP fuel cells through reliable fleet operation; (3) develop engineering models and train graduate students to use them; and (4) create new products, jobs, and markets.

### Question 1a: Relevance to overall American Recovery and Reinvestment Act of 2009 (Recovery Act) goals

This project was rated **2.3** for its relevance to overall Recovery Act goals.

- On the face of it, CHP is a good application for the technology and there should be a market in the future.
- The project, as originally configured, is relevant to saving and creating jobs.
- The project's basic (original) concept was relevant to the Recovery Act; however, its execution has decreased its likely contribution to Recovery Act goals.
- The project area was discontinued by Plug Power due to its focus on material handling only. An attempt has been made to transfer project learnings to ClearEdge Power, but this has not yet been completed. So, within the short term, it is unlikely to contribute to Recovery Act goals, unless University of California, Irvine (UC-Irvine), has gained enough knowledge to turn this into a researcher job position to support the project. It is good that project-related jobs at Plug Power could be absorbed within the Plug Power organization.
- This project preserved some jobs at Plug Power as it refocused on the material handling (forklift) market. It supported a number of postdoctoral and graduate students at UC-Irvine. It may lead to manufacturing jobs, but the path appears to be arduous.

### Question 1b: Relevance to the U.S. Department of Energy (DOE) Fuel Cell Technologies (FCT) Program's Recovery Act project goals

This project was rated **2.5** for its relevance to the FCT Program's Recovery Act project goals.

- This project is directly targeted at manufacturing and deployment.
- This project clearly addresses the goal of accelerating deployment (installation/maintenance/support) of fixed-site fuel cell power units.
- An attempt has been made to transfer lessons learned, but it is unlikely that related intellectual property (IP) will be transferred to ClearEdge Power without negotiation/terms of use. In addition, the membrane electrode assembly (MEA) used in a few units appeared to have short life/durability. Units in operation and lessons learned could help ClearEdge Power (if contracts between parties can successfully be agreed upon) accelerate the learning process.
- While the presentation discussed improvements (efficiency improvements and cost reductions), it did not give the status quo to compare them (e.g., costs decrease from $90,000 to $53,000, but what what would a

conventional diesel unit would cost in comparison?). One slide compared $CO_2$ savings to various vehicles, but it was unclear why it compared stationary use to transportation.
- Development issues have reduced the project's likely contributions to Recovery Act goals. Plans in response to those issues have had limited success in resolving those issues and fulfilling project and FCT Program Recovery Act goals.
- The plan appears good, but it was unclear why the plan did not catch the MEA issues and get control of them earlier. It is also unclear if there was a risk management plan in place.

### Question 2: Development and deployment approach

This project was rated **2.2** for its development and deployment approach.

- It does not look like the risks were adequately addressed, but overall the plan was sound.
- The barriers are all related to resolving the MEA issue and funding the process to achieve this.
- The project's technical and business risks were not adequately identified early in the project, and plans to resolve them have been ad hoc.
- The approach is on balance and well thought out. The issue is transitioning the program to ClearEdge Power. That does not seem to have happened up to now, and it represents a risk. Plug Power should certainly be commended for taking that step of working to transition the program. The question is whether it will reflect a program delay.
- It is not clear how realistic the vision is for replacing conventional electric power and gas-fired heating systems with fuel cell clusters. The project plan—a staged gate approach to managing risk—is a good one, and resulted in a rational decision being made at the third go/no-go point.

### Question 3: Technical accomplishments and progress

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

- Progress is slow due to Plug Power's lack of business interest.
- Progress has been severely hampered by technical issues (especially with the MEAs), resulting in only one of six deployed units operated in CHP mode. There was no apparent report of jobs.
- Slide 13 discussed that failures dropped from 10%–20% to 1%–2%, but this reviewer did not understand why improvements occurred (in slides or presentation). It appears stack reliability will be the key to CHP success. However, the continued assertion that a fuel cell CHP market is real is difficult to believe at this point, with project results as they are.
- Plug Power's fuel cell unit design had problems—hence the "no-go" decision on the large-scale deployment in the fourth quarter of 2011. The very "workman-like" job done in installing, monitoring, data logging, analysis, and modeling at the UC-Irvine installation saved this project from being a total loss.
- On one hand, the progress is poor due to MEA issues. On the other hand, there is some good data on the rest of the system. There is a hint of failure analysis in the presentation. It would be good to go further and provide more detail on key component failures. Also, the stack degradation mechanisms were not adequately addressed in the presentation. Mechanical problems, as identified by the presenter, do not seem to explain the chemistry difference between laboratory gas and reformate output.

### Question 4: Collaboration with other institutions

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

- The collaboration between Plug Power and BASF, ClearEdge Power, and UC-Irvine appears effective, but it is not conclusive until current negotiations to transfer the project result in an actual contractual transfer.
- The project appears to have a good partner list. It is unclear if the struggles were due to the inability to achieve the goals or if a partner change would lead to success.
- It is a good idea to salvage the program with ClearEdge Power, but it is not yet done, which slows things down.
- Collaboration with UC-Irvine seems to have been excellent, and quite productive. Handing the project to ClearEdge Power may be the right thing to do, but it was unclear how rigorous a selection process was used to

select this company, among all fuel cell makers, to take over. The project has several partners and collaborators with well-defined roles and participation. The project has brought in additional participants (ClearEdge Power) to compensate for deficiencies that have appeared.

### Project strengths:

- The project successfully uses the go/no-go decision process.
- This project has a good concept. It has the potential to reduce energy in homes. It includes the involvement of knowledgeable partners.
- This is a good idea for a program; it has a reasonable approach and is originally defined. This was a good concept for salvaging after Plug Power declared its lack of interest.
- Good fundamental modeling work arose out of the collaboration with UC-Irvine. This project has provided useful data on problems using the BASF stacks in the Plug Power design.
- Building systems and testing real hardware to determine real issues is the most difficult bridge to market. This project certainly helps build that bridge.

### Project weaknesses:

- The transition status with ClearEdge Power is a risk.
- Plug Power did not “go the distance.”
- Project weaknesses include IP transfer, funding shortage, and Plug Power refocus (including investment commitments).
- It appears that materials qualification, supplier issues, and the project’s inability to identify and resolve technical issues with MEAs have seriously delayed this project. The possibility of other internal design and quality issues exist.
- This project’s weakness is the way it manages risks. Even if there were no membrane and stack issues, it is unclear that there is currently an economic value.
- This was a very difficult project to review due to the recent no-go decision. It is difficult to ascertain if the struggles were due to the failure of MEAs from one company’s product or if the application itself will not work (not enough information was given to make that decision). Some of the comparisons were inappropriate (e.g., comparing stationary to transportation applications) and the information was incomplete (e.g., conventional application information).

### Specific recommendations:

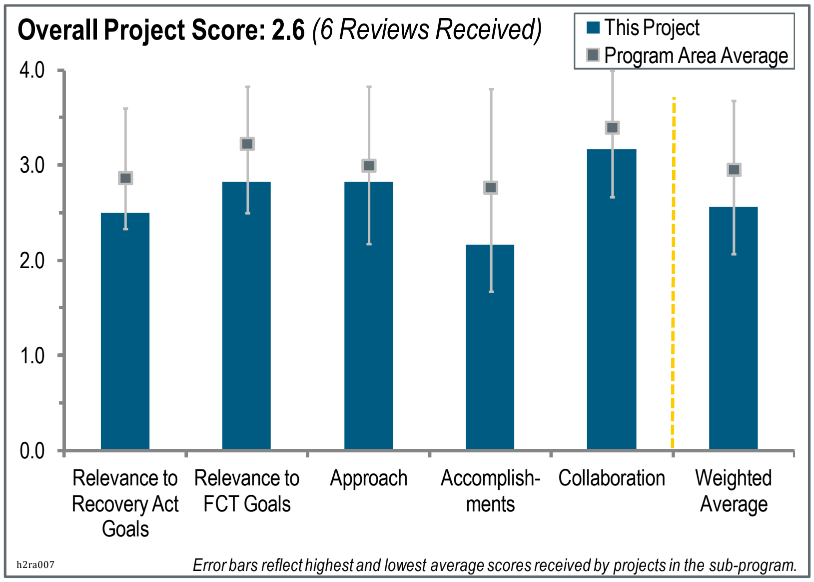
- The project team should try to save what can be saved (knowledge) and help ClearEdge Power and BASF learn from it.
- The team should move forward quickly with the transition, or simply terminate the program.
- There is a data set on component reliability that needs to be mined and reported. Also, this needs more market assessment information. It is unclear if this is really a viable market and at what fuel price it would beviable.
- This reviewer wants to believe that this CHP application makes sense, and it appeared last year to be on track. The setback at this point should be reviewed and reconsidered. If it can be argued that a different partner has a different approach or reason to show success where the first failed, then a continuation may make sense. But this continuation would need additional go/no-go milestones and heavy review (this reviewer suggests more than annual) to ensure it is a worthwhile endeavor.
- Turning the project over to another company is a good idea. When ClearEdge Power presents at the next DOE Hydrogen and Fuel Cells Program Annual Merit Review, a good justification should be made, “up-front,” as to why this particular company was the best choice. A cost/benefit analysis justifying the entire model (replacing conventional electric/heating services with local fuel cell installations) should be performed. It would be very instructive to see just how much the market must change before this vision can become a reality.

## Project # H2RA-007: Accelerating Acceptance of Fuel Cell Backup Power Systems

Donald Rohr; Plug Power Inc.

### Brief Summary of Project:

The objectives of this project are to: (1) demonstrate market viability and increase market pull of fuel cell systems within our government, customers, and partners; (2) maintain U.S. jobs both within Plug Power and outside through collaborations with the supply base; and (3) deploy 20 GenSys low-temperature polymer electrolyte membrane liquefied petroleum gas units (GenSys LT) that provide economically viable backup power in excess of 72 hours.




### Question 1a: Relevance to overall American Recovery and Reinvestment Act of 2009 (Recovery Act) goals

This project was rated **2.5** for its relevance to overall Recovery Act goals.

- The program as planned is well aligned with Recovery Act goals.
- This project's impact on jobs is not clear. It is further obfuscated by the subsequent involvement of IdaTech.
- This project is relevant to creating new jobs both within Plug Power and outside through collaborations with the supply base.
- This project preserved some jobs at Plug Power while it got out of the fixed-site business. It provided jobs for local contractors in California and Georgia.
- The original concept of the project supported Recovery Act goals; however, its execution and lack of technical and business risk analysis and mitigation planning have greatly reduced and hampered its contributions.
- The technical success seems doubtful, and relevance relative to employment is weak.

### Question 1b: Relevance to the U.S. Department of Energy (DOE) Fuel Cell Technologies (FCT) Program's Recovery Act project goals

This project was rated **2.8** for its relevance to the FCT Program's Recovery Act project goals.

- This project is very relevant and will make substantial contributions to the FCT Program's Recovery Act project goals.
- The program's objectives are well aligned with the FCT Program's Recovery Act goals and objectives.
- Backup power that requires continuous operation does not appear to be a strong value proposition. This reviewer believes that the plan is flawed.
- This project has the potential (if successfully completed) to further the Recovery Act goals of bringing additional fuel cells into the marketplace. This should compete favorably with diesel systems.
- Acceptance of fuel cell backup power at these locations could lead to wider U.S. Department of Defense (DOD) acceptance and increased sales/deployment/jobs in the fuel cell industry.
- The project's original concepts address the FCT Program's Recovery Act project goals to accelerate commercialization and deployment of fuel cell and fuel cell manufacture, installation, maintenance, and support services. Inadequate planning, preparation, and execution have greatly reduced its contribution to the FCT Program's Recovery Act goals.

### Question 2: Development and deployment approach

This project was rated **2.8** for its development and deployment approach.

- This project has a well-planned approach; however, provisions for "risk mitigation" could have been better.
- The project managed to deploy 20 GenSys LT units that provided economically viable backup power in excess of 72 hours, but the main barrier was cost, partly due to market volume.
- The original approach was well considered, and the transition to IdaTech makes a lot of sense. It is encouraging that IdaTech seems to have picked the program up. Plug Power should be commended for working to find a partner to absorb the work in the program.
- The project's presentation provided only a list of tasks that give little insight into the appropriateness or existence of technical and deployment schedule and milestones. Technical and commercial risks appear to have been addressed on an ad hoc basis, which has led to delays in systems deployments.
- The project's definitions of milestones and schedules are adequate. Barriers are understood, but they need to be better defined. It is not clear whether the project is near or far from commercialization.
- It is a good approach to take the best of the two different backup systems (instant on and extended runtime) and put them together into one system.

### Question 3: Technical accomplishments and progress

This project was rated **2.2** for its technical accomplishments and progress.

- The transition of the program has slowed things down a little bit.
- The rate of technical progress is slow because of software issues, manifold leaks, welding cracks, and other related problems. The approach to overcoming the barriers is in progress, which will help in a better future design.
- This project appears to have been a success, at least in producing very useful information about a real-world deployment. However, many of the fuel cell units seemed to have serious problems in living up to their specifications.
- The project has been delayed in achieving technical objectives by encountering a series of technical issues that have detracted from achieving Recovery Act objectives. The presentation reported the number of systems installed at Warner Robins Air Force Base and Ft. Irwin (none yet), but it provided no insight into jobs created/saved.
- The technical progress appears weak. Mean time between failures is poorly documented and a more quantitative assessment (not just a list) of components' reliability is needed.
- The net electrical efficiency and availability was disappointing. There appear to be many issues with design and implementation. In retrospect, this project appears to be much more of a development project than a Recovery Act or market transformation project. There appear to be some missing Recovery Act reporting requirements, such as jobs.

### Question 4: Collaboration with other institutions

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

- It was a good choice to have IdaTech absorb the program.
- The collaboration with IdaTech as a subcontractor with site supports (Fort Irwin, Warner Robins Air Force Base) is good.
- The relationship with the DOD demonstration sites appears to have been excellent, and the actual installation (e.g., interfacing with contractors) also looks to have been well managed.
- Working with a couple different facilities is good. More than one site and its various issues provide a broader range of user issues to discover.
- Glad to see that IdaTech has been brought in to help save this project, but perhaps other collaborations earlier in the project could have mitigated some of the issues that were discovered along the way.
- In general, this project has made excellent use of its partners and collaborators—especially to overcome planning and technical deficiencies that existed originally and that have appeared during the course of this project. It has

recently introduced IdaTech to provide site support, spare parts, and data analysis capabilities. One past project weakness was placing systems at Ft. Irwin in a bowling alley, but it has recovered, to some extent, by placing them in an engineering building.

### Project strengths:

- This project's strength was its fuel cell expertise.
- This project focuses on a valuable market segment of extended-run-time instant-on backup power systems.
- This project's strength was working dutifully to find a way to continue the program despite Plug Power's business shift.
- The project provided a good demonstration of what it takes to install and maintain a fuel cell "farm" for backup power. It displayed well-managed collaboration and contracting. This was very professionally done.
- The project explores one approach to backup power. This reviewer is not sure the approach can be successful, but it is getting hardware out there to provide evaluation.

### Project weaknesses:

- The project's weaknesses include software and welding technology.
- The project's weaknesses are its delays associated with the transition to IdaTech.
- This reviewer does not think this approach is going to hit the market needs. The useful data on durability and reliability were not adequately presented.
- This was a rather strange presentation, because Plug Power is getting out of the fixed-site market. It is not clear where this will lead in the future, and whether IdaTech will take over the sites and attendant business leads if the Plug Power units are decommissioned and removed.
- With the company's focus away from this product and toward material handling equipment, it appears they have let this product slide. The project has so far had disappointing results from the first field demonstration. This product does not yet appear ready for the marketplace, and the hydrogen community cannot afford to have a black eye from premature market introduction of any fuel cell technologies.

### Specific recommendations:

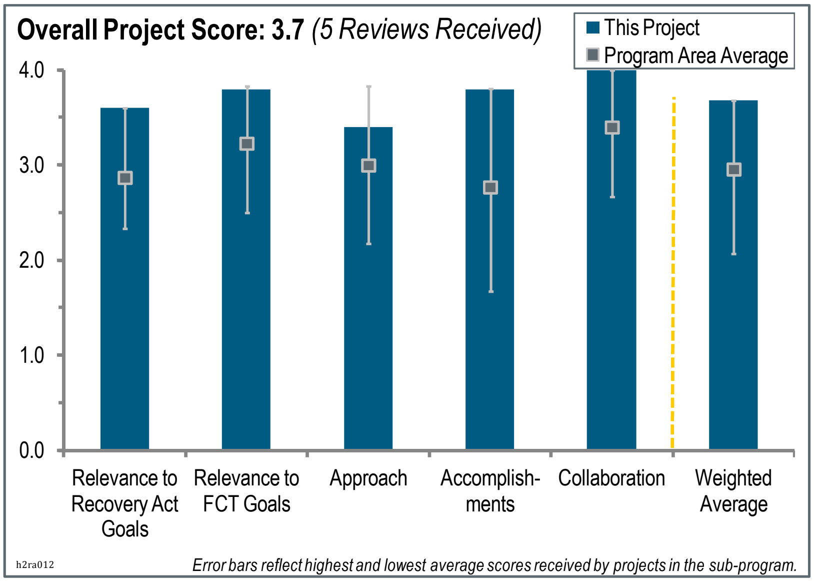
- The data set on component reliability needs to be mined and reported.
- The final report should contain a business case analysis for fixed sites such as this one. It would also be interesting to have insight into why Plug Power is getting out of this market.
- A recommendation is to use the remaining time and budget of this project to determine if this system makes sense to continue developing. The project should allow IdaTech to use the best aspects of this system for inclusion in their future products and avoid some of the problematic areas. The team should not try to bring this product to market if it is not yet technically mature enough to provide customers a favorable experience.

## Project # H2RA-012: Use of 72-Hour Hydrogen PEM Fuel Cell Systems to Support Emergency Communications

Kevin Kenny; Sprint

### Brief Summary of Project:

The objective of this project is to support job retention and creation by introducing hydrogen fuel cell technologies to new regions in the United States. The project is organized into three phases: (1) Site Survey: identify potential for a fueling station and negotiate with candidates; (2) Pre-Construction: acquire site, permits, approvals, and materials for construction; and (3) Installation/Commissioning/Project Closure: build and begin operation of the station, and collect and report the data.




### Question 1a: Relevance to overall American Recovery and Reinvestment Act of 2009 (Recovery Act) goals

This project was rated **3.6** for its relevance to overall Recovery Act goals.

- This project is an excellent example of the positive impact that Recovery Act projects can have on jobs, economic activity, and building relevant business relationships between companies for the future.
- This project supported building trades and electrical worker jobs in multiple states. Also, it produced employment in a number of collaborators' firms. Success by Sprint-Nextel will lead other cell phone services to follow suit.
- Utilizing a high-visibility customer in Sprint is critical in terms of customer acceptance of this new product and technology.
- Backup power production is an opportunity for fuel cells; cell tower applications are especially attractive. A successful demonstration of fuel cell capability and comparable or lower life-cycle costs in cell phone tower backup power could stimulate further (post-project) sales/deployments and industry expansion, thereby supporting Recovery Act goals.

### Question 1b: Relevance to the U.S. Department of Energy (DOE) Fuel Cell Technologies (FCT) Program's Recovery Act project goals

This project was rated **3.8** for its relevance to the FCT Program's Recovery Act project goals.

- This project has done an excellent job of actually getting fuel cells deployed into the field. While it has taken longer than the project team originally anticipated, there has been significant learning about site selection in the process.
- This appears to be an excellent application for fuel cells from a technical and use perspective. The product appears to be able to compete directly with the diesel status quo. It will be interesting to see if the economics make sense as well.
- This project is an excellent correlation with DOE Hydrogen and Fuel Cells Program goals. The large proportion of cost sharing is abundant evidence of the project's role in stimulating commercial deployment.

- This project would be outstanding, except for the fact that several sites have been found to be unsuitable for hydrogen ($H_2$) due to siting and codes. In this sense, it has helped to inform the relevant stakeholders (fire marshals, safety officials, etc.) of the importance of mitigating this non-technical barrier.
- Unlike some FCT Program Recovery Act projects that have become fuel cell research projects as much as deployment projects, this project has had few technical development issues, so it has focused on practical technical and commercial deployment issues in rolling out cell phone tower fuel cell backup power. This project has encountered and identified many practical issues and some solutions to issues associated with deploying fuel cells in this application. These solutions can provide guidance on implementation and significant risk reduction to others who would consider deploying fuel cells in such applications. This learning and risk reduction can "smooth the path" for others considering similar fuel cell applications and, thereby, accelerate commercialization and deployment in support of the FCT Program's Recovery Act goals.

### Question 2: Development and deployment approach

This project was rated **3.4** for its development and deployment approach.

- This project has an excellent approach (e.g., using desktop review first, then phased site review) to limit risks and narrow the opportunity list to the best site opportunities. The lessons learned appear to be well documented, which will improve the future process (e.g., identifying acceptable sites for footprint, setbacks, and the ability of truck access).
- This project has a very logical approach and is well thought out. The plan could have been slightly improved by paying attention to generic siting problems (e.g., setback, truck access) at the start.
- Other than the overall project time frame, the presentation provided no detailed set of project milestones and schedule. However, the presentation gave an efficient, detailed protocol for identifying appropriate sites and installing fuel cells, which is being pursued vigorously.
- This project has taken a very methodical approach to identifying sites, preparing for installation, and ultimately deploying units. The approach has been modified and improved during the project based on the initial experiences and setbacks.

### Question 3: Technical accomplishments and progress

This project was rated **3.8** for its technical accomplishments and progress.

- This project appears to be doing very well, making progress and improvements along the way. Most issues appear to be logistical rather than technical.
- This project, despite some unforeseen barriers, still seems to be on track. The diligence, commitment, and professionalism in working around the high attrition rate from the potential site to the Phase II candidate has paid off.
- The progress of this project has been well reported and quantified in terms of the number of sites in different phases of evaluation and installation. Specific numbers of jobs by job types were also presented.
- This project has deployed a large number of fuel cell units and employed many local tradesmen in the process, as well as jobs at both Altergy and ReliOn, which manufactured the fuel cell systems.

### Question 4: Collaboration with other institutions

This project was rated **4.0** for its collaboration.

- The project features a group of top-notch collaborators, and it is well coordinated.
- This project has a diverse group of stakeholders and partners. Lessons learned should be shared widely to support greater competition and other stakeholders into the application.
- This project includes more than a dozen partners and collaborators whose responsibilities appear to be well defined and coordinated.
- The connection with the fuel cell suppliers appears to be very strong. The collaboration with Air Products has developed a new rapid on-site refueling system that is very valuable for the $H_2$ community to have.

## Project strengths:

- This project has brought $H_2$ and backup power fuel cells into important new jurisdictions.
- This project has strong private industry support (cost share, commitment) and a business approach to success (go/no-go). It also has a strong process to limit unnecessary risks and weed out inappropriate sites early. Lessons learned appear to have been well used in this project, and should be useful in follow-on projects of same nature.
- This project represents a significant effort by a major "customer" for $H_2$ power. It has uncovered, and solved, several real-world problems that can be encountered in the large-scale deployment of fuel cells. The final report for this project, particularly the "lessons learned," will be extremely useful to others. The fuel-cell-powered transmitters that are being installed will be an enduring legacy of this project.
- This project has successfully deployed a large number of fuel cell systems. It has developed good relationships with fuel cell suppliers as well as a key $H_2$ supplier. This project has provided a shining example of operating fuel cells in the field for future executives to examine when evaluating whether fuel cells will actually work when placed in the field.

## Project weaknesses:

- No obvious weaknesses were identified from this project.
- It would be interesting to see additional partners (or similar projects using different technology partners) to focus on more cost reductions and competitiveness, as well as additional approaches to implementation.

## Specific recommendations:

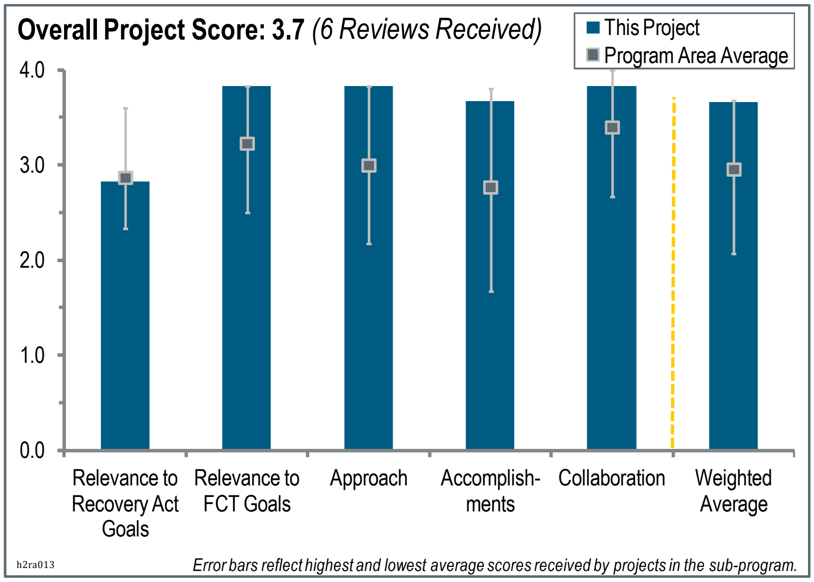
- The project team should keep up the good work.
- It was a good idea to explore reformer-based technologies for sites that would be difficult for $H_2$-based systems.
- This was a very good project that should be followed through. It is unclear if the economics will make sense (although it appears the technology does), which could result in follow-on projects open to additional competitive partners to encourage cost reductions, new approaches, and/or other improvements to a good project. The project team should complete and "advertise" benefits and opportunities.
- It would be good to document the learning obtained from the site selection (using Google Earth, etc.) to enable more rapid deployment by other $H_2$ developers and demonstrators in the future. The reviewer questioned if this could be produced as a fact sheet that is posted online and given out by DOE at future conferences. Another recommendation is to continue to operate these systems as long as possible to provide a rich data set to document the technical status and progress of the technology.

## Project # H2RA-013: Analysis Results for ARRA Projects: Enabling Fuel Cell Market Transformation

Jennifer Kurtz; National Renewable Energy Laboratory

### Brief Summary of Project:

The objectives of this project are to: (1) assess the technology status of fuel cells in real-world operations, (2) establish performance baselines, (3) report on fuel cell and hydrogen ($H_2$) technology, and (4) support market growth by evaluating performance relevant to the markets' value proposition.




### Question 1a: Relevance to overall American Recovery and Reinvestment Act of 2009 (Recovery Act) goals

This project was rated **2.8** for its relevance to overall Recovery Act goals.

- This project is good—more detail is needed on the safety aspects.
- There was no reference to job creation based on data collection or resulting economical impact from data collection. Under the definition of the Recovery Act goals, there was a very limited explanation about how this project meets these goals, although reviewers can guess that it does have an impact because there were a number of people involved performing the data collection and analysis.
- Even though the project assessed the technology status in real-world operations, there was no indication of creating new jobs or saving existing ones.
- It is not clear how this contributed to the rapid generation of new jobs—other than paying the folks who worked on the project. It may contribute to the long-term goal of creating a true $H_2$ power industry, which will eventually create jobs.
- This project documented the value proposition for material handling equipment (MHE) fuel cells on a cost-of-ownership basis. This has been a substantial benefit in validating the value proposition for MHE. The data helped improve the performance, quality, and reliability of MHE and backup power fuel cells. Codes and standards support greatly assisted the penetration into niche early markets.

### Question 1b: Relevance to the U.S. Department of Energy (DOE) Fuel Cell Technologies (FCT) Program's Recovery Act project goals

This project was rated **3.8** for its relevance to the FCT Program's Recovery Act project goals.

- This is a great way to compile the data and not just rely on each company to "market" its product.
- This is one of the most important and successful projects within the DOE Hydrogen and Fuel Cells Program (the Program) Annual Merit Review (AMR), because it supports all of the fuel cell activities by providing a neutral assessment and evaluation and a readily digestible review of the various projects. It helps not only to understand what is happening within the projects, it allows for comparisons and contrasts in various applications so that each can learn from the others.
- The subsequent boost in commercial sales for MHE and backup power fuel cells, without DOE investment, validates the relevance of the plan to the fuel cell industry.
- This project is relevant, but it is not overly clear how this data will be used besides that the project is an independent technology assessment.

- This project addresses the technology development plan of the FCT Program's Recovery Act project goals of accelerating the commercialization and deployment of fuel cells and fuel cell manufacturing, installation, maintenance, and support services.
- This type of data collection and analysis is indeed relevant to the mission of the Program at DOE and will be useful to a number of groups that study market readiness and maturity of fuel cell products. Given the overlap of graphic material between this and some of the other National Renewable Energy Laboratory (NREL) presentations at the AMR, however, there is some question as to what, exactly, this contributed to the whole effort.

### Question 2: Development and deployment approach

This project was rated **3.8** for its development and deployment approach.

- The presentation of data is very good. More data can be extracted to guide suppliers' feedback on how to optimize operation to maximize operating life.
- This project was an excellent addition to the "Hydrogen Safety Panel Final Report" (compared to the 2011 AMR project presentation). This will certainly increase the value of the assessment, especially during the adoption phase of the evaluated technology.
- This project always has clearly defined milestones, presents complex information and analysis in an easily digestible manner, and highlights significant challenges or successes. This works for multiple applications and areas using similar templates to increase familiarity.
- The development and deployment approach are impressive, as evidenced by the deployment of 1,111 fuel cell units throughout the United States by the end of 2011. Systems are operating reliably in 15 states with 99.7% successful starts. The unsuccessful starts included an emergency stop signal and system failures.
- This project has a very good approach—expanding tools developed for fuel cell electric vehicle market analysis was the right thing to do. However, there was a lot of commonality between the slides in this presentation and some others from NREL. This made it a little difficult to understand precisely how much was contributed by this specifically funded effort and how much was already in place from other projects.

### Question 3: Technical accomplishments and progress

This project was rated **3.7** for its technical accomplishments and progress.

- This project appears to have met all the timelines and milestones and does a very good job of identifying obstacles and progress/success.
- The Hydrogen Secure Database has gained the kind of acceptance needed to make this a very useful study. Continuing to collect and publish this kind of data is a very important way to help foster the acceptance of $H_2$ power.
- This is a great package so far, but this reviewer hopes to see more detail that leads to insight and customer comfort (and that cost and safety are clearly understood).
- The approach to establish capabilities under other technology validation activities (NREL Fleet Analysis Toolkit) and industry collaborations are helpful, as is the concise reporting of large data sets from multiple project partners. There is good progress compared to last year, but data and information need to be pushed out to industries more. As mentioned before, companies and entities might find this useful, but they do not know that it exists or that it could be relevant.
- It appears that 2011 presented projected 2012 "units in operation" but did not have sufficient information about the number of "backup power" units for 2012, which made the total number of projected units slightly off compared with 2012 operational units (in a positive way). Also, because no more Recovery Act funding is used to support 2012 fuel cells (MHE and backup power units), the 2012 projection was not necessary (a mention of the numbers of units purchased by private industry beyond Recovery-Act-funded units suggests a positive effect of the data collection and may strengthen the final NREL report). The number of operational hours/kg for backup power systems (slide 10) has increased significantly (from 6.6 hours/kg in 2011 to 8.4 hours/kg in 2012). It may be worth mentioning or giving an explanation as to why this is the case. Continuous run time for fuel cell backup systems (slide 11) would have been good to see, and compared with previous years. On slide 13, the numbers of MHE units in operation do not add up correctly based on the numbers provided per class; it may also be worth

mentioning the total average operational time per day. This reviewer questioned if NREL looked at the correlation between the number of hours of operation per day and degradation (including the number of start/stops of the fuel cell). This could be a good metric to share with industry regarding preferred operational mode to extend the durability of the fuel cell unit. It would also be useful to see comparable averages for battery MHEs from industry (fuel cell MHE safety report, slides 18–19). This reviewer would also like to know if the fact that $H_2$ compressors have a significant share of total $H_2$ leaks has been shared with others in industry.

### Question 4: Collaboration with other institutions

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

- Collaboration activities, including site visits, are impressive.
- There are high-quality partners on all of the MHE and backup power projects and partners that put in substantial resources, both financial and human, into the projects.
- It looks like the project has good collaboration from all parties. This reviewer suggests providing a slide overview on how the project team obtains good collaboration and data.
- This project is a little different from other projects in regard to collaboration (i.e., it appears that the project team ends up working with most of the various partners in other projects to conduct analysis and products). There is less direct understanding of how and when they interact, but it appears to be succeeding.
- This project has excellent coordination with other contractors and other parts of DOE. There is widespread acknowledgement of NREL, and of this group specifically, as the “place to go” to obtain data and to report successes (and maybe more importantly, problems); this project demonstrates the outstanding and successful effort to collaborate with customers, too.

### Project strengths:

- This project displays good collaboration activities.
- This project shows impressive work, considering that it was funded with $0.00.
- The MHE and backup power projects were well executed with high-quality partners.
- This is a great way to put all the data together and get information back to the industry and potential customers. It needs to continue even when the Recovery Act projects wind down.
- This project produces a lot of valuable data and includes a wide variety of partners and application sites. The secure data center adds value for both partners and strengthens NREL’s position globally.
- This research takes complex projects, conducts detailed analysis, and produces easily digestible products. It is by far one of the most exciting things within the Program, as it allows stakeholders, industry, and the public to understand what is happening across various projects. This work successfully protects private and confidential information, yet it provides significant value to industry. It highlights where future focus and challenges need to be addressed as well as progress and success.

### Project weaknesses:

- This work could be used in more applications as a template for review in other areas.
- This project’s weakness is its lack of comparable data for the incumbent technologies.
- The project’s weakness is its future funding. The project could add more safety panel site visits to strengthen the Safety Report.
- This project includes a lot of data, both raw and “reduced,” but it would be useful to see a bit more in the way of analysis. Theis reviewer would like to know what this means for the future and the competitiveness of $H_2$ power.
- Another reviewer would like to know where all these data are reported, so people can use the data as a reference in planning and developing new products and markets.

### Specific recommendations:

- Keep this work going!
- This project really needs a cost-of-ownership analysis for backup power.

- Continue this work and look into expanding this type of review, analysis, and summarization products to other industries and areas.
- The project did not address the Recovery Act goals related to job creation or saving existing ones.
- NREL should consider developing a measurement tool to show the benefit of their data collection work and analysis to industry and on job creation. DOE should continue the data collection for the benefit of industry and build on Recovery Act project data. The reviewer suggests collecting data on internal combustion engine and battery applications, both for backup power and MHE.
- A relative risk assessment delineating the safety of $H_2$-powered versus battery-powered forklifts would be very useful. This should also be put into perspective by comparing it to the relative risk of all power-related incidents with risks (e.g., “crashes” or load collapse).

**Attendee List: 2012 Hydrogen and Fuel Cells Program and Vehicle Technologies Program**

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# Sub-Program Comments Provided by Reviewers

## Hydrogen Production and Delivery Sub-Program Comments

### Hydrogen Production

**1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)**

- Yes, the sub-program was well presented.
- The sub-program area was adequately covered, along with important issues and challenges. Progress was clearly presented in comparison to the previous year.
- The sub-program was clearly covered, and in great depth.
- The sub-program area was well covered. Issues and challenges were well identified. Given the number of technologies in the sub-program, presenting these might have been a challenge, but slide 5 provided a concise description for each technology and related the challenges across technologies—well done. Progress during the current year was well presented, but progress during the previous year was not as apparent, so a comparison between the two could not be performed.
- Yes, the sub-program was adequately covered.
- The sub-program was more than adequately covered; the important issues and challenges were identified; and there was good evidence of progress.
- The presentation summarized the sub-program well. The sub-program's 2012 accomplishments were clearly identified.
- The Hydrogen Production sub-program was well covered and clearly articulated. The goals, accomplishments, and plans were well presented and on target.
- The sub-program was adequately covered. All of the current pathways were presented, and their barriers were identified. Progress was clearly presented in comparison with the previous year.
- Very broadly speaking, yes. There was much greater focus on accomplishments than on challenges and issues, but the latter are very hard to cover in a talk of this length.
- Yes. The "develop distributed and central technologies to produce hydrogen ($H_2$) from clean domestic resources within DOE Hydrogen Production sub-program" debrief did not cover the 2011 achievements in detail; however, the sub-program mainly focused on 2012 program goals. The sub-program's near-term and long-term challenges with respect to distributed production and centralized and semi-centralized $H_2$ production were identified. The U.S. Department of Energy (DOE) prioritization of research and development (R&D) needs in $H_2$ production clearly identified a technologies development road map through 2030.
- The Hydrogen Production sub-program review was excellent. The presenter provided the right level of detail to highlight the accomplishments and challenges of the program. In particular, it was nice to see that several areas, such as the TDA Research Inc. $H_2$ cleanup system (which has broader commercial potential than use just for biogas organic sulfur removal), demonstrated significant progress. Also, the progress underway with high-pressure electrolysis from Giner and Proton presents a nice cliff-hanger for next year's review in terms of associated costs and performance.
- Excellent program oversight. One need is to indicate which fiscal year dollar amounts are expressed in (i.e., are the funds defined in fiscal year [FY] 2002 dollars or FY 2012 dollars).
- There is a need to consider whether the short-term portfolio is sufficient with regards to well-to-wheel emissions. The sub-program should very much focus on cost!
- The sub-program area was adequately covered. However, capital cost and process efficiency challenges for thermochemical production were not addressed in the presentation.
- Significant progress is being made in those research areas targeted for continued funding in FY 2013. The elimination of FY 2013 funding in selective research areas is a reflection of the ability of these technologies to show progress or a path forward if a technical barrier has been identified.

- The sub-program seems to be spending a lot of effort on exotic pathways of $H_2$ production. However, it seems to put very little emphasis on a very promising technology—solid oxide electrolysis. In the eyes of this reviewer, this technology is the most promising for renewable $H_2$ production. This reviewer is not a developer of the technology and does not stand to gain from its development. The reviewer is very informed about all pathways from a technical and economic point of view. Solid oxide electrolysis cell (SOEC) technology should be placed under a microscope, and its economics should be outlined and encouraged according to its merit. This technology has the potential to consistently produce $H_2$ at over 80% efficiency (lower heating value), with zero apparent degradation (due to less than therma-neutral operation). SOEC can also have an extremely low capital cost, because the material operates at very high current densities. Solid oxide ceramics are also very dense, thus producing $H_2$ of very high purity due to the low diffusion of impurities into the final product. Recent information on performance shows that this technology can respond rapidly to transients, which makes it feasible for integration with variable renewables. It would be prudent to spend some effort analyzing this technology's merit, and place it in the spotlight to stimulate industrial development for nearer term, affordable, renewable $H_2$ production.
- No. This activity has been funded for many years, especially during the previous administration. It does not appear to have had much advance in the state of technology. The only discussion was on the use and improvement of an advanced modeling tool, the Hydrogen Analysis (H2A) model. It was unclear if progress was delayed due to funding limitations or greater challenges that could be overcome, or whether funds were given to model development over R&D. This should have been discussed more thoroughly.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- A detailed plan for addressing issues and challenges was identified. There are no gaps in the project portfolio.
- There are no apparent gaps in the project portfolio.
- The Hydrogen Production sub-program plans capture main challenges associated with fuel production. The technologies road map reported the successful conclusion of both natural gas (NG) reforming and biomass gasification R&D efforts.
- Gaps are well defined and in line with the Office of Energy Efficiency and Renewable Energy (EERE) mission.
- There are identified plans and challenges. There are gaps, but these are probably the result of the availability of funds. The sub-program is doing a good job, given the funding.
- Plans were well called out in the presentation. The sub-program portfolio covered the area very well, given program funding levels and priorities.
- The plans of this sub-program will adequately address the issues and challenges, provided there is sufficient funding.
- Plans were presented for addressing barriers/issues. The portfolio encompasses many different technologies, and all require significant experimental work to address the very challenging barriers to obtain DOE's targets for efficiency and $H_2$ cost.
- There are clearly laid out plans to address gaps. One remaining question is why there is so little emphasis on improving bio-derived liquids. Also, more analysis related to the costs of biomass feedstocks should be undertaken prior to diminishing the budget in this area. For instance, now that excess NG is available in the United States for power production (i.e., renewable portfolio standards), there should be more biomass available at a reasonable cost for $H_2$ production. The increase in funding for photoelectrochemical (PEC)-related projects was unclear and it was unclear if this technology will meet the production cost targets at a commercial scale.
- This overview spent little or no time discussing plans. There is one (very well used) slide discussing challenges at a very high level. There are few gaps in the portfolio, although it was sometimes hard to tell if individual programs were properly addressing the most important gaps.
- There is a big gap on large central production technologies and small-scale renewable systems. The Office of Fossil Energy and the Biomass program have funded the construction of a number of demonstration projects, but the data from these projects has not been modeled and used to judge state of the technology.
- The main gap is trying to separate the different program components (production, delivery, and storage) without looking for synergies between them. The reviewer asked if, for example, the liquefaction process can be (partly) run with waste heat from the production process. The ultimate goal is not improving $H_2$ production, but rather enabling $H_2$-based transportation.
- The program should be looking at large-scale electrolysis for central $H_2$ production.

- There should be some focus on rural America and other isolated sites (i.e., off grid and off road). Fuel generated at remote localities could make a huge difference in how things are done “out in the boonies.” Technology that enables relatively small and rugged units that can be used in remote areas is a great idea for the H-prize. For example, units comprising solar-powered water electrolyzers and compressors and accompanying $H_2$ storage systems could provide power on demand at remote locations where liquid fuels are difficult to obtain. The technology could also serve safety, disaster response, combat, humanitarian, and third-world needs. In the latter category, there are regions in India where electricity costs 60 times the rate that New Yorkers pay. These regions are ripe for “leapfrog” technologies because the cost could be recovered quickly by a family, provided that appropriately sized units could be made available by advances in the technology.
- A well-to-wheel analysis was not covered. The sub-program needs to very much focus on cost.
- The elimination of funding for bio-derived liquids for distributed production seems appropriate, given that none of the technologies being pursued have a clear path forward for meeting cost targets. Furthermore, these technologies are rather complex, and it is hard to image them being operated in a distributed fashion.
- Plans for active engagement of the Concentrating Solar Power (CSP) Program were not addressed. Solar collection and concentration reflects major capital cost components for solar thermochemical production, and coordination/collaboration with CSP is essential to this area of $H_2$ production.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program’s needs?

- Yes.
- The sub-program appears to be focused, well managed, and effective. This program is more demanding than some of the other sub-programs, because much of the work is long term in nature.
- The sub-program is well managed and even includes collaborations between other DOE offices as well as other agencies.
- Yes. The sub-program is looking at how to reorganize aspects and options to meet future projected cost targets.
- The program is well managed, but its focus and effectiveness are hampered by inadequate levels of funding to achieve sufficient progress that would retain the interest of stakeholders and researchers.
- The sub-program area does focus on main technologies and does address the DOE Hydrogen and Fuel Cells Program’s (the Program’s) needs. It is important to recognize that some R&D technologies may offer some technical merits while not being practical. Sub-program management needs to weigh each technology’s practicality in many areas (e.g., mass productions/fabrications and end-to-end system practicality).
- The sub-program appears to be well managed, but it seems too broad in scope.
- Better facilities are needed for environmental testing, specifically for compressor materials at elevated temperature in $H_2$.
- Yes, in general. But the portfolio needs the uniform application of techno-economic analysis to identify the specific targets that must be achieved to reach $H_2$ production goals. It would probably be advantageous to focus more effort on approaches that have a higher probability of success.
- While the sub-program appears to be focused and well managed, this reviewer suggests that the sub-program keep abreast of progress being made in the DOE Office of Science’s Office of Basic Energy Science (BES) Energy Frontier Research Centers and hub programs for a number of the technologies being funded in centralized production to leverage those R&D investments and to make sure that effort is not duplicated.
- It is not clear if the biological production approach being taken will ever yield cost-effective $H_2$ production. Perhaps this approach should be focused on fundamental work only, funded and directed by BES.
- The project was focused on developing a new tool, but it was unclear if the tool was validated by industry and through independent analysis. Although the current portfolio was well managed, the presentation failed to adequately give the audience an understanding of the real technology development issues, especially if there was limited funding.

### 4. Other Comments:

- It is a good and carefully planned sub-program.
- This is a very well run team; emphasizing longer term research that is appropriate given that steam methane reforming (SMR) will dominate this area for decades to come. The challenges are substantial, but not impossible.
- Large-scale production is needed.

- Progress in key cost factors are explained by using a model that was recently updated.
- The feedstock cost is a significant portion of the cost of $H_2$ in many of the technologies being developed within this sub-program. It is extremely difficult to project the cost of the feedstock in the future; the sub-program's cost estimates have increased significantly over the last year due to factors such as increased demand, which again is very difficult to project forward. As such, it is very difficult to judge to what extent improvements in technologies being developed reduce the cost of $H_2$ independent of the feedstock cost. Compared to the cost of gasoline, which is the basis for the DOE cost target, the majority of the cost is feedstock. Although the H2A is needed, the principal investigators (PIs) should be instructed to clearly show how their technology reduces the capital and operating costs independent of the cost of the feedstock. This way, reviewers will have a better way of judging the impact of the R&D effort on improving the technology.
- Funding levels for the Hydrogen Production and Delivery sub-program have fallen to subcritical levels. The most effective response is to significantly reduce the number of options being pursued in order to ensure effective progress in a smaller number of active investigations. It is interesting to note that two long-term technologies with much higher 2015 and 2020 production cost targets (biological and PEC) receive about five times the funding allocated to a third long-term technology, solar thermochemical hydrogen production (STCH), with lower 2015 and 2020 production costs, while at the same time, the STCH target embraces much higher central production capacity than PEC or biological.
- The presentation tried to give the audience the feeling that technology was moving forward to achieve the key cost drivers. However, it looked like it was using the Hydrogen and Fuel Cells Technical Advisory Committee as an independent panel to support the portfolio, rather than actually evaluating the work and the funding allocation to achieve the production targets. The presenter should more clearly describe the state of technology and the barriers to getting large-scale commercial systems into the market.

## Hydrogen Delivery

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- Yes.
- The sub-program was covered adequately. Important issues and challenges were identified, and progress during the past year was presented.
- The sub-program area was adequately covered, with all important issues and challenges identified. Good progress has been made compared to the previous year in terms of improving efficiency and cost reduction.
- Yes, the sub-program was thoroughly covered. The presenter provided a balanced view on technologies and commercialization challenges.
- The presentation on the Delivery sub-program was excellent—thorough and encompassing. Goals and challenges were well articulated. Progress was clear and impressive.
- Yes. All of the delivery elements were identified, and their 2011 status reported. Cost reduction goals were specified. The overview presentation was concise and relatively easy to understand.
- Yes. The speaker did a good job of describing the issues/barriers and then presenting how the accomplishments address the issues.
- The program was well described. Economic and technical challenges associated with delivery were explained.
- The sub-program area was well covered. Important issues and challenges were well identified. Progress was well identified. The presentation used a very nice format that described status and progress by technology area: the slide banner named the challenge and progress, and the body of the slide noted the past year's accomplishments, recent accomplishments, and future work.
- This was a nice, careful presentation that touched on all critical issues, progress, and changes from last year. It clearly and succinctly showed the organization of the sub-program, goals, accomplishments, the status of work, and future plans.
- Yes, the Hydrogen Delivery sub-program covered both the 2011 achievements and future work/goals. Major challenges with respect to $H_2$ delivery technical changes, transportation, liquefaction, and delivery were identified. 2011 milestones and progress were clearly presented.

- The sub-program was adequately covered, important issues were covered, and progress was delineated within budget constraints. The presentation should have mentioned the team members.
- This was an excellent overview of the program and its progress. Important issues and challenges were identified, with the exception of funding.
- Given the limited amount of funding in these programs, it appears that significant progress is being made and that the advancements from last year were clearly apparent.
- Yes. The reviewer would like to learn about any collaboration with other federal (e.g., U.S. Department of Defense, U.S. Department of Transportation, and U.S. Department of the Interior) and state programs, as well as other countries (e.g., India and Germany).
- The presenter covered the sub-program very well.
- The Hydrogen Delivery sub-program area is well addressed by the range of projects. High station costs are identified as a primary concern, and there is a reasonable focus on projects exploring compressor and storage costs. The range of projects is well justified and focused on areas where delivery cost reductions are possible and needed.
- The presentation adequately covered the barriers and challenges to reducing the cost of delivered $H_2$. Progress could have been presented in a more direct fashion; specifically, why the approach has changed from the higher risk of developing adsorbents versus overwrapped conventional equipment. Similarly, the reviewer asked whether analysis or the lack of R&D results led to the change in direction.
- The content of the sub-program review was excellent in laying out the near- and long-term market scenario for $H_2$ delivery. Also, the new cost targets for $H_2$ production, delivery, and dispensing were introduced. Clear examples were given of how the delivery targets can be reached in the near term through the use of high-pressure tube trailers, which will allow minimization of high-cost compression at the forecourt and/or the use of fiber-reinforced pipeline systems. Also, challenges such as the regulatory hurdles for high-pressure tube trailers were highlighted. The sub-program accomplishments served as proof points that the delivery cost objectives can be met. For instance, the Lincoln Titan carries five large-cylinder glass-fiber-wrapped vessels with increased carrying capacity of 18% over the four-cylinder module that meets the 2015 target.
- Delivery is an essential component in the realization of the fuel cell electric vehicle (FCEV) market. The Delivery sub-program addresses long-term to early-market scenarios and involves technical pathway cost analysis. Each solution pathway (e.g., tube trailer transport, pipeline transmission, and so forth) has been systematically investigated from both an engineering and cost analysis perspective. Substantive collaborations among national laboratories and industry are strengths of the sub-program.
- The presentation needed additional focus on early market barriers (e.g., a pipeline seems to be a very long-term path).
- More work needs to be done to improve component reliability in delivery and dispensing systems. As the sub-program recently was reminded, the failure of components can be catastrophic to the industry. More testing of various valves, sensors, and electronic controllers needs to be performed in a controlled environment before systems are fielded (e.g., testing 1,000 valves of type A, type B etc.). This needs to be done so that reliability can grow without jeopardizing actual installations. The reviewer has heard from California installations that valves and fittings have been the weakest link for years, and that is something that DOE can address for the industry at large.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Yes. The most important bases were covered.
- The plans presented appear to be adequate and realistic. No gaps were noted in the project portfolio.
- A detailed plan for addressing issues and challenges was identified. There are no gaps in the project portfolio.
- Concise plans were presented for addressing barriers.
- The gaps have been well identified. With the exception of applying more budget to get it done faster, the sub-program is doing a good job.
- The program has done a good job with limited funds to identify and address areas where DOE investment can facilitate and enable delivery. Gaps do exist, but they cannot be addressed with current program funding levels.
- Plans are well called out for addressing issues and challenges. For the given funding, the portfolio covers priority issues very well.
- There were no major gaps.

- All critical components were covered.
- Plans for addressing issues and challenges have been identified. Any gaps are attributable to the availability of funds.
- Plans are discussed. Long-range goals and objectives may need to consider materials science barriers.
- The Hydrogen Delivery sub-program plans capture main issues and challenges associated with fuel delivery. However, $H_2$ fueling stations, locations, fuel storage, and consumers' easy access were not addressed.
- Future plans are all well thought out. Given the significance, magnitude, and variability of the compressor cost, the two projects involved in the program need to be given priority and carefully administered and reviewed.
- The overall plan is excellent. The addition of some focus on early markets is very appropriate. A few useful additions to the plan could include clearer effort on the pros and cons of liquid vs. gaseous $H_2$ delivery, some clearer focus on cold gas delivery, and potential utilization of the Lincoln Composite Titan 4 ISO unit for storage at stations and terminals to reduce these storage costs.
- Plans were identified. Some strategic consideration needs to be given to setting priorities for delivery technologies and costs for near-term, intermediate-term, and long-term options. This strategic assessment should be executed in light of estimated $H_2$ consumption levels in the near term, intermediate term, and long term. Driving costs down through investments for options that might never be employed, or that might be employed for relatively brief periods, could supplant R&D investments for much lower cost and more extensively deployed delivery options.
- The plans presented address the key issues, but more information should have been presented on the economic characterization of the complete system to achieve the $4 per gallon of gasoline equivalent (gge) target. It was not clear why there hasn't been more demonstrated success, especially given that this work has been in progress for a number of years.
- Key roadblocks of retail site costs for compression and bulk transportation were identified.
- A number of the technological and regulatory challenges were addressed. The portfolio seems to be addressing all of the key areas. This reviewer suggests obtaining data on the Lawrence Livermore cryo-compression unit with vehicle dispensing, because this seems like a viable approach for addressing delivery costs.
- The problem of $H_2$ delivery cannot be treated as a separate problem from the problem of storage. If the program focuses on minimizing $H_2$ delivery cost, the result may be a technology that increases the cost of vehicle storage, increasing the total cost of ownership. The program needs to look for synergies between production, delivery, forecourt, and vehicle storage, and concentrate on supporting those.
- There do not seem to be any real gaps in the portfolio of projects.
- There should be some focus on rural America and other isolated sites (i.e., off grid and off road). Fuel generated at remote localities could make a huge difference in how things are done "out in the boonies." Technology that enables relatively small and rugged units that can be used in remote areas is a great idea for the H-prize. For example, units comprising solar-powered water electrolyzers and compressors and accompanying $H_2$ storage systems could provide power on demand at remote locations where liquid fuels are difficult to obtain. The technology could also serve safety, disaster response, combat, humanitarian, and Third World needs. In the latter category, there are regions in India where electricity costs sixty times the rate that New Yorkers pay. These regions are ripe for "leapfrog" technologies because the cost could be recovered quickly by a family, provided that appropriately sized units could be made available by advances in the technology.
- The regulatory and public acceptance issues related to $H_2$ pipelines are likely to be a significant non-technical barrier.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's needs?

- Yes.
- The sub-program looks to be in very capable hands.
- Yes, the sub-program is well managed. There is a focus on cost reductions, and the vendors are well on their way to meeting the goals. This is a mark of an effective program.
- This sub-program appears to be exceptionally well organized and managed considering that it appears to be relatively underfunded, considering the magnitude of the issues and potential showstopper capabilities of these issues.
- The program seems focused and well managed. Breaking the effort into near-term and long-term market scenarios is a valuable way for accelerating the development and deployment of the technologies being pursued.

- Yes, the prioritized path is well planned. The analysis conducted over the years has forced or tracked progress toward the goals.
- The sub-program administration and technical team are strong. Individual projects are steered toward DOE targets. The program has been very well managed. Research and technology priorities have always been identified and pursued systematically.
- The sub-program has been well managed. Outstanding analysis efforts have guided research efforts to ensure that dollars are spent in areas that matter. Despite limited funding, several projects have advanced to demonstration or commercialization, including trailers, centrifugal compression, and magnetic liquefaction.
- The sub-program appears to address major issues to reach the defined goals. It is good to see both liquefaction and compression solutions in the mix.
- The focus of the sub-program is good. The execution of roadblock items has been delayed due to funding. But there is good progress on compression, lightweight transport cylinders, and reduced-cost liquefaction.
- The sub-program managers have managed the program very effectively.
- The area is focused, but management appears to be more laboratory-directed than headquarters-directed. It is unclear whether the approvals for performance standards, for example, are leading the technology or lagging it. It is also unclear how much of a role each technology plays in achieving the final targets, and whether they are interrelated so that all must be achieved, or whether there are other sensitivities.
- The Hydrogen Delivery sub-program is focused only on the delivery of fuel and not on fueling stations and fuel storage.
- Greater priority is needed on the fiber-reinforced polymer pipeline technology, which could be a real winner.
- Better facilities for environmental testing are needed, specifically for compressor materials at elevated temperature in $H_2$.

## 4. Other Comments:

- It is a good and well planned program.
- This was a very nice, tight presentation.
- The sub-program has worked in areas where market pull from applications such as compressed NG has helped enable the early commercialization of products.
- The presentation should have included results from the California Fuel Cell Partnership refueling station designs and operating costs to give the audience an understanding of how much of an improvement this technology approach will make in reducing the cost. Currently, there are more than 5 years of actual operating data with incremental improvements in the technology. The reviewer asked how these results compare, and whether the development cycle takes as long to enter the market.
- Given the importance of $H_2$ infrastructure that supports a variety of applications (truly the most important crosscutting area), public investment should be prioritized on infrastructure challenges. The major automobile manufacturers will solve the design and production of vehicles, but they need infrastructure so that the public can use their vehicles. At least two major auto companies, General Motors and Toyota, plan to roll out their fleets in 2015. The major challenge they are facing is that the fueling infrastructure to support these fleets is not there yet. It is unrealistic to think that the car companies will set up the infrastructure. That should be a federal function, just like the interstate highway system. The reviewer noted that this program solves many of the technical problems, and asked whether it could do more with more funding. Dropping an already small budget from $5.2 million (2012) to $2.9 million (2013) is not going to get us there fast enough. This sub-program should be made a much higher priority.
- Some strategic consideration needs to be given to setting priorities for delivery technologies and costs for near-term, intermediate-term, and long-term options. This strategic assessment should be executed in scenarios of estimated $H_2$ consumption levels in the near term, intermediate term, and long term. Driving costs down through investments for options that might never be employed, or that might be employed for relatively brief periods, could supplant R&D investments for much lower cost and more extensively deployed delivery options. Whereas the targeted delivery costs are chosen to provide incentive for early deployment and commercialization, such costs should be targeted in concert with technology deployment in the areas of production and consumption. Cheaper is not necessarily better in the context of parallel capability development.
- It is unclear whether the cost targets are expressed in FY 2012 dollars or FY 2002 dollars. Pathway challenges are clear indicators of the direction forward, with the cross-modal common challenge.
- Model development of HDSAM has been and remains key to understanding the cost limitations of the scope of this sub-program.

## Hydrogen Storage Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary presentation of the sub-program if appropriate.)

- Yes.
- The sub-program area was well covered and the challenges and progress highlights were presented clearly.
- The presentation summarized the program well. The 2012 accomplishments were clearly identified.
- Yes. Issues and challenges were clearly delineated for all types of $H_2$ storage. Progress was also clearly shown with examples.
- The sub-program area was well covered with a clear view of progress made in the current year. The presenter showed a clear understanding of the technical and programmatic details of the sub-program projects.
- All aspects were covered completely along with valuable information and examples.
- The presentation was clear and progress indicators were provided. A specific comparison with last year was not given, nor was it really needed given the way that the presentation was delivered.
- This was an excellent overview of sub-program activities and priorities. There was a good discussion of important issues and challenges. New R&D thrusts for 2011 and 2012 were introduced, and the impact of continuing projects on the overall program was highlighted. Overall, it was a very clear and illuminating presentation.
- Yes, definitely. The presenter did an excellent job covering the different ongoing activities and addressing the important issues and challenges of $H_2$ storage in both the long and short terms. The inclusion of applications beyond vehicular $H_2$ storage was welcome. The technical progress was clearly presented, especially in system modeling and system design.
- The presenter did a good job covering the status of the sub-program. The talk was verbally clear, but as usual, the slides were flooded with too much information to take in with a quick glance.
- The sub-program area was adequately covered. $H_2$ storage materials and the associated engineering are major issues, but the cost reduction of high-pressure cylinders is also an important issue to be covered in this sub-program. Some parts of the $H_2$ storage field are conducted with close coordination to related projects. International collaboration could not be found in the presentation.
- The goal seems to be to develop and demonstrate viable $H_2$ storage technologies for transportation, stationary, material handling, and portable power applications. There are four key elements of the storage sub-program: advanced tanks, materials development, engineering, and testing/analysis. Targets for each application area are pretty well addressed, but the vast majority of the work is focused on transportation and materials for transportation, with little work on portable power and stationary applications. This reviewer does not understand the value of close coordination with the DOE Office of Science's Office of Basic Energy Science (BES), because BES researchers make it clear that their work is focused on basic science and that they are not focused on practical application. Progress toward goals was highlighted well; however, progress of 2012 vs. 2011 was not highlighted. The sub-program areas were adequately covered and important issues and challenges were identified well.
- The cross section of storage activities was adequately represented. Progress in some areas was more apparent than progress in others. Some areas, such as spillover and improving the binding of $H_2$ in metal-substituted metal-organic frameworks, need more clear guidance as to what success looks like. These areas seem to be in the incremental phase, where little progress is being made, and are both far from attaining DOE targets.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- There is no gap in the project portfolio. The plan is well considered, considering the limited budget.
- The plans are focused on the DOE targets.
- Plans were identified for addressing issues.
- Clear plans and pathways for addressing the important issues and challenges in the different areas were presented. In addition, improvements over the current baseline materials-based systems were identified. Given the obvious limited funding, the project's portfolio is well balanced. The addition of performance targets for projects in areas other than vehicular applications closed a preexisting gap.

- Issues and challenges were adequately addressed. There are still some gaps in terms of meeting targets (e.g., DOE $H_2$-storage system targets).
- Plans for reducing the costs of tanks appear to be well planned and executed. Boron-nitrogen-based organic storage materials appear promising. Two major gaps in this technology are off-board regeneration issues and the potential toxicity of the materials. Either of these has the potential to be a showstopper.
- Yes. A thorough description of projects and the issues the projects are addressing was provided. Also, a road map for the remainder of FY 2012, FY 2013, and FY 2014 was given. There are significant challenges with all chemical storage options. The costs for all storage systems will be very difficult to reduce. It seems unlikely, with a nearly 20% cut in funding for FY 2013, that any new work on the discovery of better storage materials can occur. It is also unclear whether other organizations such as BES, the National Science Foundation (NSF), and others will adequately support the critically needed fundamental research necessary for $H_2$ storage materials.
- Future plans were clearly identified. Due to funding constraints, the project portfolio is limited and has a focus on systems engineering, which may be premature. Current materials do not meet DOE goals. The program's level of effort on the discovery, development, and improvement of new materials does not match the needs of the program and country. Industry is capable and well positioned to address all engineering challenges if given viable storage materials. However, industry is not in the position to provide the resources, expertise, or risk associated with the development of a new and difficult technology based on materials discovery. This is a gap that should be filled by government-funded R&D and should be the focus of this sub-program.
- There are well-poised plans to address issues. There are gaps largely due to the ever-dwindling budget. The major requirements are still materials, especially now that the engineering center has made meaningful progress on system designs. Plans to address compressed tank costs are desperately needed. The economic success or failure of FCEVs will hinge on rapid cost reduction in fuel cells and (more relevant to this group) compressed gas tanks. Currently all of the material-based storage media fall short of the 2017 DOE targets and there is very scant resource available to create new materials. The plan to use NSF/BES and others to partly stop this gap is innovative, but insufficient. EERE is the only organization that drives to performance goals. As a result, it is the only one likely to deliver the needed storage materials.
- In general, plans to address technical obstacles were effectively summarized. However, in this reviewer's opinion, there is one important component that is missing from the overall portfolio. Nonreversible chemical hydrides (especially ammonia borane and alane) have emerged as important materials systems in the engineering development projects in the sub-program. However, very little effort is being directed toward the daunting challenge of efficient and cost-effective regeneration of the spent fuel. Development of an efficient onboard $H_2$ delivery process complete with contaminant mitigation is of paramount importance in the near term. However, finding a cost-effective process for regeneration could, at best, limit the timely introduction of a working system, or at worst, it could be a showstopper altogether. DOE management should take a close look at the regeneration issue and consider how the sub-program might be able to accommodate a more robust activity in that area.
- There does not seem to be an adequate plan for addressing the fact that there are not adequate $H_2$ storage material candidates for which a system can be engineered. The needs of portable power systems and stationary systems are not well addressed.
- A stage-gate review of spillover efforts and the sorption work needs to be considered very soon as those areas seem to have "hit their asymptote."

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's needs?

- Yes.
- Yes, with the current scenario of decreasing budgets.
- This is a well-managed sub-program.
- This sub-program is well thought out and properly focused on addressing the major issues and technical challenges of $H_2$ storage. The strong technical background and the experience of the sub-program manager in the field of $H_2$ storage are having a very positive impact on the quality and directions of the sub-program. The sub-program appears to be very well managed and rigorously coordinated and geared toward satisfying the DOE Hydrogen and Fuel Cells Program's needs.
- The sub-program is very well managed. The DOE Technology Development Managers are well respected by the project PIs and other participants, and they are doing a top-notch job of coordinating the efforts, anticipating program needs, and organizing new technology funding thrusts to support emerging areas. This reviewer

strongly supports their decision to maintain a high-risk/high-payoff component in the sub-program in addition to the engineering system development efforts. The sub-program management has done an excellent job of making a compelling case for continued support of the overall program in the face of difficult funding challenges.

- Yes. It is focused and well managed, and the managers are knowledgeable about the history and content of their program. It is focused on meeting the DOE targets in general, but there are a few projects out there that will be unable to achieve the targets and those projects should be reevaluated.
- The sub-program is both focused and well managed. They do a very good job with the funds allocated, but it is not likely that they will meet their goals at this level of funding. If funding cannot be expanded, the sub-program might need to focus further on compressed gas and one other approach (based on what was presented, adsorbent-based materials would be the most likely candidate). While the broad approach makes sense at the current level of progress, the budget is not sufficient to make real progress in all areas, and thus focusing on a couple of leading routes would be required for serious progress.
- The sub-program is focused and very well managed. However, budget reductions have left the critical area of materials-based $H_2$ storage development essentially unsupported. Because of this, a vital element of the Program's needs is not being sufficiently addressed. This is a gap in a long-term plan that significantly diminishes U.S. technology leadership. Hopefully, funding can be found to support a condensed and cooperative group of experts to make substantial progress on this challenging issue.
- The sub-program is well focused as far as the efforts of the Hydrogen Storage Engineering Center of Excellence (HSECoE) are concerned. Some of the peripheral projects, particularly ones at universities, could probably be dropped, because they are not addressing systems/approaches that have a chance of helping the HSECoE meet its 2014 objectives or the 2017 DOE $H_2$ storage system level targets.
- The sub-program area appears to be focused a little bit tightly. The materials for validation downselected by the engineering center of excellence are adsorbents and liquid-state chemical hydrides. Because there are no longer any materials centers of excellence, the only independent research is under EERE and fundamental research is under BES (which conducts advanced material research). More materials work may be needed to explore alternatives for downselected materials. Management of the sub-program is appropriate and the sub-program is effective in addressing the Program's needs.
- The major shortcoming of the storage program is that it is currently operated in isolation and focused almost entirely on the onboard issues associated with storage. By looking at onboard issues only, the sub-program has expended resources on suboptimal systems that appear promising on board, but have off-board issues that will preclude their adoption. A systems approach would have benefitted the program significantly and led to more efficient utilization of limited resources. Expansion to forklifts and portable power are good additions to the program.
- Some of the BES researchers need better focus. In more than one talk, the presenter clearly had not done proper prior literature review and was essentially repeating work already done very well in the materials centers of excellence, which ended in 2010. It remains a fact that "an [hour] in the library will save you a [month] in the lab." The engineering work going on in the HSECoE looks quite solid, with good decisions being made on go/no-go decisions. The research at the University of Oregon looks fabulous. There should be more projects like these.

## 4. Other Comments:

- This sub-program is in many ways a "poster child" for how DOE programs should run. There is a proper balance between the needs of several industries with involved and interested industrial advice, world-class research projects, and active and effective program management. All the storage program needs is proper funding.
- The organization, planning, and execution of the HSECoE are exceptional. Savannah River National Laboratory is doing a great job running the center and the partners are making significant contributions. Thought should be given to extending the project through 2017 to give the center a better chance of meeting DOE's 2017 performance targets.
- Despite the continually decreasing funding, this sub-program remains at a high quality, is effective, on track, and continually focused on the real issues in $H_2$ storage. However, it is severely underfunded at this stage and it would really need more resources to get a real chance of advancing the storage technologies toward meeting the technical targets.
- This program should stay the course.

- The energy efficiency of the various storage options should probably be addressed more directly. Application-specific targets for storage systems should have been established. This reviewer fears that some materials that could be good for applications other than transportation could be discarded.
- The high cost and low efficiencies for alane and ammonia borane will prevent these technologies from ever being implemented. If the sub-program wants to continue work on these systems, it should reallocate resources to see if it is possible to improve these, rather than continuing work on onboard properties.
- The program seems to have a particular focus on cryosorbant material research (i.e., metal-organic framework). Because design ideas have resulted in several new materials, establishing $H_2$ storage trends as a function of material properties is recommended, as it aids in establishing the viability of overcoming challenges related to their intrinsic properties such as density, heats of $H_2$ adsorption, and so on. This may allow the program to focus more on the most promising materials.
- There is no comment on international collaboration in the presentation. Specifically, this sub-program's known current collaboration under the umbrellas of the International Partnership for Hydrogen and Fuel Cells in the Economy and the International Energy Agency Hydrogen Implementing Agreement were not adequately addressed. The budget for this subprogram has been reduced year by year; therefore, international collaboration becomes even more important to share the recent achievements conducted by other countries.
- The slides are sometimes a bit busy and hard to read from the back of the room.

## Fuel Cells Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- Yes.
- Yes. The introduction was perfect.
- Yes. This presentation provided a broad, but brief, overview that highlighted both the outstanding challenges and some key results.
- Yes, clear and exciting progress from last year was clearly shown.
- The fuel cells area was clearly and comprehensively covered. Successful examples were described, and the remaining challenges were discussed.
- Yes. Given the short time for presenting such a large body of work/progress, the presentation seemed to summarize the program well.
- Yes. The Fuel Cells sub-program area was adequately covered in the presentation. The progress as compared to the previous year was clearly presented.
- The area was well covered, and the cost drivers were clearly articulated that inform the use of the available resources. Progress was demonstrated from prior years, and the steady improvements were documented.
- The Fuel Cells sub-program was well covered. The objectives, strategy, and challenges were highlighted, along with recent progress. Automotive, stationary, and portable applications were all well covered. These are all important markets for fuel cells.
- The description was good. Annual progress was presented, and at least some of it was described (there was a lot of progress to review, and not much time).
- Yes. The sub-program area was adequately covered, important issues and challenges were identified, and progress was clearly presented. The issues of cost, durability, and performance were highlighted, and progress was presented, covering a range of fuel cell types, including polymer electrolyte membrane (PEM) fuel cells, portable fuel cells, and solid oxide fuel cells (SOFCs).
- The sub-program area has been well covered, and the most important issues and challenges have been well identified. The project "A Total Cost of Ownership Model for Design and Manufacturing Optimization of Fuel Cells in Stationary and Emerging Market Applications" might have also been presented with the two other cost analyses, as the question of total cost of ownership is critical for fuel cell commercialization. The main progress since last year has been clearly presented.
- Yes, although it would be good to see overall goal charts and progress broken out by components.
- Yes. The sub-program usually calls out highlights, which is expected, but it would be interesting to see a table of all of the different catalysts studied (by catalyst type and project) and show side-by-side where they are at for various activity and durability metrics. The same could be done for membrane conductivity, water transport model fits (at various current densities, or under a specified transient condition), and so on.
- Highlights of the sub-program were adequately covered. Considering the length of the time for the talk, a comprehensive review is not possible. Some of the more important issues were covered—cost is always the most important. The need for fuel processor cost reduction was highlighted, as was the need for fuel cell durability to be enhanced. Progress on catalysts, portable power, and humidification (balance of plant) were covered.
- This was a very effective overview. One additional area that might be mentioned is the complementarity of fuel cells and batteries for transportation: batteries make more efficient use of renewable electricity but are limited in range and rate of recharging; fuel cells provide full-function vehicles with good range and rapid refueling but have lower overall efficiencies for utilization of the currently most practical forms of renewable energy. Fuel cells also require more fueling infrastructure than do (slowly recharged) batteries. Both still face cost challenges.
- The sub-program was adequately covered, and the primary challenge of cost for automotive and stationary systems was identified. The challenge with increasing durability was not addressed. Progress was clearly presented, with highlights demonstrating increased catalyst durability and increased direct methanol fuel cell and direct dimethyl ether fuel cell catalyst activity.
- The discussion on projected transportation fuel cell system costs, on slide 6, has many assumptions in it. It would be valuable to state some of the most major assumptions, such as the cost/performance being based on nanostructured thin film (NSTF) technology. Similarly, it would be nice to see this same discussion based on the

standard technologies that developers are actually using (e.g., dispersed platinum (Pt) on carbon supports) because to date, there is not an indication that developers have been successful in using the NSTF. Knowing where the program stands in terms of cost analysis with standard materials versus non-standard materials would be valuable.

- For the objectives, all areas were well covered. For micro-combined heat and power (CHP), only electrical efficiency was listed for 5 kW systems, while total efficiency >90% was listed for >100 kW CHP systems. It was unclear why total efficiency for micro-CHP/5 kW was not included. There are examples of 5 kW micro-CHP systems with >85% total efficiency, operating on NG. There were good examples of progress presented, such as 3M's work and GM's work on de-alloyed catalysts, meeting or exceeding DOE goals.
- The sub-program was not covered adequately. Select projects were highlighted, but it is not clear that the sub-program has a clear plan for how it will bridge the gap from its current status to ultimate targets. All of the important challenges were listed, but the budget breakdown suggests that only a few of these challenges are prioritized. The only quantifiable progress highlighted was a minimal reduction in the projected high-volume cost.

## 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Yes.
- Yes. There are no obvious gaps in the project portfolio.
- There is no gap in the project portfolio. All of the issues and technical challenges have been identified, and the strategies to address those issues and challenges have been planned.
- All plans address the issues. There are no apparent gaps.
- Yes. Cost and durability issues were emphasized, and the projects being funded are (mostly) aimed toward these key barriers.
- Plans have been made for addressing the remaining issues and challenges. The plans are a good use of the amount of funds allocated.
- The plan forward seems to be very adequate.
- Issues and challenges were well defined and identified.
- Some guidelines to address the challenges were discussed.
- Issues and challenges were identified as "targets." The plan for addressing the targets was clear.
- In addition to laying out component or modeling status versus target metrics, there could also be a very cursory description for each project of what gaps need to be addressed.
- Yes. With more resources, more catalysis, membrane, and alternative fuel cell research (e.g., for alkaline, reversible, and toward reduction-oxidation flow batteries) should be included.
- Good plans are in place for fuel cell systems.
- Plans are in place for most stack aspects, but balance of plant (BOP) is not sufficiently covered to make important progress. While not the biggest cost factor, it is an important one that can contribute to the eventual success of the FCEV economically. It is encouraging to see increasing manufacturing aspects.
- Plans were identified, which were largely more of the same for this past year. A major gap in the portfolio is in the membrane development area, particularly membranes that can handle liquid fuels. The use of liquid fuels as a long-term goal was also underrepresented.
- The membrane activities seem to be deficient. In addition, the new funding opportunity announcement (FOA) is vague—it should at least point out the major topics and emphasis.
- This program has many projects ending and almost all are ending in FY 2013. When those projects end, there are tremendous gaps, even if the DOE alternate projects are awarded, and certain areas were not included in the last solicitation. The major example is that there will be no durability projects after FY 2013, as the current projects will all end, and the subject was not addressed in the latest solicitation. Other areas that are not well covered include advanced membrane development (i.e., hydrocarbon and high-temperature) and mass transport, which was also not covered in the last solicitation.
- There is no funding for many of the focus areas listed in the presentation. This is a major concern going forward. Areas with little or no funding include membranes, electrolytes, gas diffusion layer (GDL), plates, seals, and interfaces. Meanwhile, there are numerous projects that seem to overlap in their focus (i.e., accelerated stress test development, nanowire catalyst, and transport modeling). Much better distribution of resources is required. Also, the strategic analysis can be misleading. For example, the analysis shows that stack power density is the single

biggest factor affecting fuel cell system cost. However, to enable higher power densities while simultaneously meeting thermal heat rejection targets, higher temperature operation will be required. Currently, there is no focus on higher temperature automotive systems or components.

- The importance of contaminants' effect on fuel cell performance and durability can be highlighted more. The plans for addressing issues and challenges are vague. More funding on lowering fuel cell cost and increasing durability and performance seem to be a more appropriate use of DOE funding, rather than developing low-cost BOP components.
- Plans for cost analysis were identified, and several future milestones relating to costs were presented. There are currently gaps in the portfolio, and the situation appears to be worse in the near future. One current gaps are high-temperature PEM and polybenzimidazole-phosphoric acid systems, both of which are receiving a lot of attention in Europe but are not represented in the current portfolio. Phosphoric acid and molten carbonate research is also a large gap in the portfolio. Another gap is PEM membrane research, where the current projects are ending but no new membrane projects are scheduled to start. Fuel processor work is currently underfunded. With a decreasing budget the last few years and the recent expanding scope to include an "all of the above" strategy encompassing all types of fuel cells, there is simply no way to avoid large gaps in the portfolio. Increased funding is needed to cover the expanded portfolio. With the majority of projects culminating in the next two years and no new FOA planned, the situation will worsen.It appears there will be gaps in crucial areas such as PEM catalyst work.
- Plans have been identified to address the issues and the remaining challenges. Bipolar plate developments are apparently not covered anymore, even if durability and cost issues remain. As it is now quite well accepted that coatings will be needed, investigations on it may already be useful, in particular for roll-to-roll and low-cost coatings. Investigations on fuel cell components and systems operating at medium temperatures (95°C–120°C) may be useful for automotive original equipment manufacturers.
- The plans on slide 12 address some, but not all, of the challenges faced by the industry. Key challenges remain on cost and high-temperature performance/durability, which will be the main impediments to widespread commercial scale-up. Additional funding should be allocated to a variety of approaches to increasing power density and materials that can support high-temperature operation and enable vehicle/application cooling systems to be successfully implemented. On the BOP side, additional funding for air compressor and humidifier development is important for the industry as a whole.
- The strategy slide lists a "technology neutral" approach, which this reviewer endorses. However, for areas such as stationary power that may span several different fuel cell technologies (e.g., phosphoric acid fuel cell [PAFC], SOFC, and high-temperature PEM), there may be specific challenges that do not cross all types. For example, the temperature ranges of high-temperature PEM, PAFC, and SOFC are roughly 160°C, 190°C, and 700°C, respectively. This span dictates different material challenges with regards to seals and BOP design/subcomponents. While cost is mentioned on this slide, there is no citation for manufacturing R&D. Manufacturing R&D is different than materials R&D. It addresses key needs to get to low-cost manufacturing. This appears to be a gap in the general fuel cell strategy, although it is currently addressed in the active portfolio.
- New awards were announced that appear to partially address the areas with issues. Some of progress reported also works towards addressing issues.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's R&D needs?

- Yes.
- The program is intensively and well-managed.
- Yes. The sub-program is extremely well managed and clearly focused.
- This sub-program is well-managed, with the projects that it currently has. It has funding limitations that are responsible for any gaps.
- The Fuel Cells sub-program is well focused and is effectively using the allocated resources to solve the most pressing program needs.
- The sub-program has a much improved focus.
- Overall, the portfolio is focused and well-balanced to address needs.
- The sub-program is well focused and well managed. It is especially effective in addressing membrane electron assembly related issues, but it is soft on BOP aspects.

- Yes, although it is not clear what the relative emphasis is on reducing the overall installed costs of fuel cell systems for power.
- Yes, except for what appears to be a funding cliff. Many projects are ending, but new ones are not being funded. This reviewer asks whether all momentum will be lost.
- The sub-program is well managed and effective. In a time of diminishing funds, the sub-program appears to have been directed to broaden its scope to include all types of fuel cell systems, rather than maintain the focus it had on PEM systems. The strategy to increase the scope of the sub-program when funds are decreasing does not seem to be well thought out.
- Yes. For the future, a go/no-go decision and its stage gate should be more clear, and a basic and common philosophy of how to determine these needs to be defined.
- Yes. The sub-program has done a lot of work over the years to eliminate projects that did not show promise and reward those that have. Funding has swung dynamically from membranes to catalysts as the membrane suppliers began addressing performance and durability, but the catalysts remained a problem affecting both cost and durability.
- If based on the examples of success shown, the sub-program area is focused towards catalysts, with the exception of a humidifier. A chart showing simple progress toward goals in some of the other focus areas would help (stack components, system and BOP, and so on).
- The program seems to be taking a simplified approach to resource allocation. Cost and durability have appropriately been recognized as the biggest gaps to enabling fuel cell commercialization. The program puts most of its resources on non-Pt and low-Pt group metal catalysts (for cost reduction) and durability studies. The interdependencies of all components in the stack do not seem to be appreciated, and neither the cost nor durability targets can be met without advancement in the other subcomponents and understanding the interactions between these components. Also, with minimal funding available, it is puzzling that a large fraction of projects awarded this year are for cost analysis rather than technical development.
- The outcome of awards from the last FOA was a major disappointment across the industry. This FOA started with the best intentions to adhere to a tight schedule for submission, evaluation, and award. After a very long delay with no information on status, only a handful of awards were made, with little–to-no explanation of why. This reflects poorly on DOE and is disruptive to commercial organizations who must justify to shareholders substantial time spent preparing proposals with no visibility on timeline or status of decisions. Many in the industry are hopeful that the approach advertised by the recent Advanced Research Projects Agency-Energy FOA (i.e., a lightweight concept paper first, full proposal only after some indication of probability) will be more effective.
- Tornado plots seem to be a good mechanism for focusing research efforts where they would have the most impact on fuel cell costs. The sub-program area seems to generally be using them well. It is always difficult to get the right weighting between the areas that could produce the greatest cost benefits and the areas where major improvements are possible (e.g., whether much can be expected in improvements in compressors). Overall, management of the sub-program appears to be thoughtful, informed, and effective.

### 4. Other Comments:

- The team can be congratulated on their work and encouraged to continue to final success.
- Like the past reviews, this meeting remains the most important one in order to have an exact and updated state-of-the-art picture of fuel cell technologies. It also allows reviewers to have many exchanges with the different researchers and DOE people.
- Considering the unusual funding demands from Congress, DOE has done an admirable job keeping its focus and moving programs forward.
- The broad focus is appropriate for a national program and allows for some early success. That is probably a good strategy politically, too. That said, with the lower budget, it is critical that there are enough resources applied to the harder automotive program to ensure continued progress and keep the critical mass of researchers in the field.
- This was supposed to be a review, not a technical meeting. This reviewer was a little bit disappointed that most of the questions were about detailed technical issues, not direction and other macroscopic management-related issues.
- It was hard to read the slides. There were too many concerns on individual slides. Presenters should use big text (the rooms are huge, and there is just one screen). Also, the sound system was far from stellar. This reviewer had difficulty at times hearing what was said. It might make sense to school the speakers in public speaking.

- It is always nice to have presentations that are energetic and engaging. Most presentations at the Annual Merit Review are either monotone/boring or difficult to understand, mainly due to a lack of clarity. Perhaps presentation clarity can be part of the evaluation.
- Some of the durability data seems to have been generated under very controlled laboratory test conditions. These data should be validated under real-life fuel cell testing conditions to ensure their application in practical fuel cell operational conditions.
- As fuel cell cars are coming closer to deployment, infrastructure should be addressed more. No $H_2$ vehicle will be deployed without $H_2$ on the road, and few will be deployed if $H_2$ is just available for captive fleets. Other than hybrid cars, FCEVs depend on $H_2$ not only when deployed first. An $H_2$ infrastructure for captive fleets will allow for the first user to operate it, but no used car market can develop. That was one of the reasons why methanol never could make a breakthrough in California, when it was deployed in captive fleets. A transition to $H_2$ should involve at least the technical readiness of mass market infrastructure based on pipelines and road transport. This seems underrepresented in the existing infrastructure efforts. They look like they might allow for niche markets, but not for mass markets, at a time when vehicle development is geared toward mass markets.
- It was not specified whether the automotive stack is 80 kW net or gross.

## Manufacturing R&D Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- This is a well-managed and well-executed sub-program.
- The overall goals and objectives of the sub-program were clearly defined, and the projects in the sub-program were highly relevant to these goals and objectives. The important issues regarding manufacturing were clearly highlighted for this sub-program. The accomplishments of some key projects in the sub-program were highlighted, as were the results from a manufacturing workshop held last year.
- The manufacturing sub-program projects are rather mature, well conceived, and executed, and they are delivering quality results. The body of projects is broad (as it should be), encompasses the major challenges in this area, and is interrelated in many cases.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- There are plans in place for addressing key issues, including defining the current state-of-the-art and potential advancements that could be made with further investment. There are no significant gaps in the project portfolio, but additional projects could greatly expedite the advancement of these manufacturing technologies.
- The only gaps are due to the availability of funds.
- For this sub-program, the issues and challenges that remain have been clearly identified, and a plan exists, with the only limitation being the availability of funding. This program has a clear fit and path for expansion into other fuel cell types and applications if the Program had the funding to ramp up its focus in the area of manufacturing.
- There are no projects for low-cost, high-volume manufacturing of GDLs. There are no projects for high-volume assembly of stacks. This program concentrates only on component developments.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's R&D needs?

- Yes.
- This program seems well managed.
- The sub-program is clearly well managed, and the accomplishments from all of the programs have been remarkable. Many of the programs are seeing reductions in cost of more than 50% for the components, while others are demonstrating novel technologies that are directly applicable to fuel cell manufacturing. These advancements are necessary if the fuel cell industry is to grow domestically.

### 4. Other Comments:

- The DOE Hydrogen and Fuel Cells Program would greatly benefit from more projects like the ones found in this sub-program. Providing the funding necessary to drive the manufacturing technologies will be far more useful at this stage of fuel cell development than continuing to fund projects focused on fundamental understanding. The technology is adequate to penetrate the market today, but costs remain prohibitive to compete with technologies such as lithium-ion batteries and diesel engines. If the goal is really to drive the market adoption of fuel cells and reduce the impact to the environment, then the main focus has to be on reducing the overall system cost and pushing quality fuel cell products to the market.
- A few of the projects seem to have tasks that migrate away from manufacturing into areas such as membrane or electrode design, or durability studies. It seems like this sub-program has a disproportionately high number of projects that are beneficial only to the prime contractor.

## Technology Validation Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- Yes.
- Yes, but there was no comparison with the previous year (one was not needed).
- Projects included in the sub-program were effectively summarized and highlights were presented. Slides 6 through 9 provide an overview of technology validation projects, with appropriate emphasis on those accounting for the bulk of resources expended since the sub-program was created, particularly the national $H_2$ FCEV and infrastructure learning demonstration, fuel cell bus demonstrations, and validation of an integrated energy station. There was no specific mention of issues and challenges. A “forward funding” approach is being adopted, but the implications are not clear to this reviewer based on information provided in the presentation. Time was not sufficient to follow up on that topic.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Yes, there are plans for addressing issues and challenges.
- The presenter noted that the Technology Validation sub-program is in transition. The fuel cell applications being addressed by the sub-program are expanding. Important elements of the sub-program’s future portfolio will be determined in large measure by awards resulting from current DOE FOAs. The FOAs focus on light-duty FCEV validation and $H_2$ refueling station performance. They were discussed. Milestones relevant to the sub-program’s future are included in slide 10.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program’s R&D needs?

- Yes.
- Yes, fairly well. Management has recently changed.
- Yes. The data from this sub-program are extremely valuable to demonstrate the readiness of $H_2$ and fuel cells.
- The positioning, purpose, and goals of the sub-program were clearly and succinctly articulated in slides 2 and 3 of the presentation. Some projects within the sub-program are outstanding, having been refined over a period of years and benefitting from superb management. An example is the National Renewable Energy Laboratory’s (NREL’s) data collection, analysis, and reporting in connection with the FCEV and infrastructure learning demonstration project. The primary focus of a few of the projects seems to be on work not consistent with the goals and described boundaries of the sub-program. Examples are NREL’s renewable electrolysis system development and testing (wind-to-$H_2$) project, the Florida Hydrogen Initiative, and the Hawaii Hydrogen Power Park.

### 4. Other Comments:

- The activities within some projects are outside the positioning of the sub-program as indicated on slide 2. For example, NREL’s wind-to-$H_2$ project has elements of technology development, a test facility, a user test facility, and a technology development laboratory. Another example is the Florida Hydrogen Initiative, within which there are multiple types of activities, from basic materials research to $H_2$ education. Responsibility for such projects could logically be placed within each of multiple sub-programs. A challenge to be addressed by overall Program management is ensuring that there is effective communication about project plans, oversight, and results across relevant sub-programs. Expansion of the sub-program’s scope to fuel cell material handling equipment (MHE), stationary fuel cell installations, and fuel cell back-up power seems to be moving along well. DOE is encouraged to focus this sub-program on independent, objective analysis of data received from both DOE-sponsored and non-DOE projects. Conversely, activities not consistent with the positioning and description of the sub-program, such as technology development, testing, and education, should be minimized in planning the future sub-program portfolio.

## Safety, Codes and Standards Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- Yes, the program is sufficiently covered, issues/challenges are identified, and progress is presented.
- Yes, the coverage of the program was adequate, and it demonstrated progress from the previous year.
- The sub-program was adequately covered, and important issues were identified. The projected reduction in funding is significant, and the across-the-board reductions in sub-program elements are proposed.
- This presentation described the program, its mission, direction, and key issues. A nice description of accomplishments from the program showed good relevance to the acceleration of deployment of these technologies, by providing solid, relevant information that goes directly to developing defensible codes and standards and by providing critical information that will enable fault-tolerant design.
- This sub-program area was adequately covered by the presenter. This portion of the DOE Hydrogen and Fuel Cell Program's budget has been [reduced] for a few years, but the sub-program team has done an exemplary job of getting critical issues addressed with the funding that they do have. The issues and challenges in getting the $H_2$ economy fully commercialized are well known to this sub-program team. They have forged productive alliances with industry members to accomplish common goals.
- The sub-program area was adequately covered, and the challenges were identified. Safety issues are of critical importance for public acceptance of $H_2$ and fuel cell techGnologies. It is absolutely needed to show that the $H_2$ and fuel cell industry is safe—even safer than other energy-related industries. Images like the Hindenburg accident and/or the nuclear bomb must be replaced in the public by safe and "green" applications.
- The work and leadership from the sub-program team have been critical to support standards/code development. This is greatly needed in new and emerging technologies. New technology areas benefit from coordination of the high-level industry, similar to the national template created in the early stages of the emergence of $H_2$ as a fuel alternative. While in traditional, established markets, standards maintenance could be considered routine work for a standards developer, the scenario is different in new and emerging technologies. Traditional standards products generate support through support services to established manufacturers. In new technologies, standards developers do not have any mechanism to generate financial support—they sink their money into research and product development. Safety standards are needed to help the industry evolve and demonstrate safety, but these evaluations cannot be done without a standardized method. The standards development organizations (SDOs) invest significantly in development of requirements to support developing industries. A mechanism through DOE needs to be identified to continue supporting the development of the safety standards until the industry is "launched."
- Yes. However, this reviewer wishes that there was more funding made available to support the activities that were started but yet to be completed. Progress was clearly presented.
- By attending this session, the reviewer got tangible evidence of progress in addressing the sub-program objectives identified on slide 2, with the exception of the first bullet (safety in DOE-funded projects). It was easier for this reviewer to identify progress compared to the previous year from the information contained in the overview presentation than from the individual presentations in the session. To assess progress, it would be helpful to indicate the degree of completion/achievement of the "past" milestones identified on slide 17, as well as the current amount of progress towards reaching the future ones.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Gaps are being addressed in the portfolio.
- Plans are identified for continuing with existing efforts.
- There are no obvious gaps in the project portfolio. Plans for addressing issues and challenges are identified.
- The challenges are clearly addressed. Safety is a crosscutting activity for which regulation, codes, and standards will help to maintain a good image in the public. Nevertheless, the building of standard answers, should a safety problem occur, is missing in the portfolio.

- Yes. The sub-program has a large task, and there is good “top-down” alignment of project goals to objectives, objectives to challenges, and challenges to current emphasis. The sub-program holds to two goals: safety implementation and applied R&D and leadership in standards development. The sub-program lacks cohesion between those two stated goals. The approach seems directed at the critical R&D pathway and does not suggest a collaborative effort with the development and implementation of safety practices and procedures. Standardization should ensure safety, but those standards, and the R&D activities that support their development, must begin and end with the end users, industry, code enforcement officials, and others. The reviewer asked whether there are opportunities to encourage sub-program participants to collaborate and leverage decreasing funding to advance both goals.
- Yes. There are still significant gaps in the programs. The level of detail in articulating the plans forward, given the changes in funding, has not been made clear. As a result, it seems like a “hurry up and wait” situation. The stakeholder voices have not changed in their expectations, but it seems as if DOE is moving the decision point out five additional years, from 2015 to 2020.
- Yes. The description of the international round robin is a good example of an activity targeted to work harmoniously with the international community to harmonize the test method protocol in measuring the physics necessary as dictated by SAE 2579, GTR, EHIP rev 12b, etc. The forklift tank cycling campaign is also an example of how the sub-program was able to rally resources to rapidly address a critical safety issue, which answered the original question (“Are these tanks being used safely in the current use domain?”), and the sub-program was able to get some good science out of the program. The sub-program is positioned to continue to advance the needs of the regulation, codes, and standards community and to respond to unanticipated, high-priority needs.
- As far as this reviewer understands, there is no major future change foreseen in the methodology that is followed in the sub-program. The “cases” covered are identified on the basis of need, such as “all the work related to the safe in-door deployment of forklifts.” As such, this reviewer cannot identify any major gaps in the overall scope of activities. On an individual project basis, the material coverage in the materials and components compatibility project seems too narrow, considering the fact that $H_2$-compatible materials will have to be used and deployed worldwide. The sub-program should interact with and learn from other countries on their experience with steels of different composition from those used in the United States and with fiber-reinforced composites.
- The major gap continues in the funding of national standards efforts. DOE support has been critical and much appreciated by industry. The problem is the view that the industry has arrived, and there is no longer a need for this support. Standards are critical for existing and emerging industries. Industries are safe because of the checks and balances in the system. Safety codes and standards have been identified as a critical element for helping industries transition to commercialization. The approach with $H_2$ to address the standards early in the market development was revolutionary for reducing time to market. This also enabled the United States to be a leader in the standards area. It would be beneficial for all aspects of industry for DOE to investigate through stakeholder engagement and reconsider the need for supporting national standards development activities for areas critical to the DOE agenda and the national agenda.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program’s R&D needs?

- Yes.
- Yes. The sub-program’s communication with industry is excellent.
- This area is very well managed and has produced results that are useful.
- The sub-program area appears to be focused. International cooperation is absolutely needed in this field in order to achieve standards that will be adopted worldwide and that will address the Hydrogen and Fuel Cell Program's needs.
- Yes, the sub-program appears well focused and targets near-, mid-, and long-term objectives as well as coordinating the R&D activities with the standards development. The near-term needs are supported through the focus on related niche markets, such as forklifts through steel tank cycling and indoor fueling risk assessment. The mid-term needs for vehicle deployments are addressed by activities such as fuel quality, international harmonization, fast fill, tunnel safety, and first responder safety. The long-term needs are supported through fundamental applied science, such as sensor technology development, materials compatibility, and safety incident database development. The sub-program appropriately balances these R&D activities with the changing

landscape of market needs, immediate issues, standards document development schedules, and international harmonization.

- As indicated in slide 5, the sub-program will likely be affected by a non-negligible budget cut. All elements of the sub-program identified in slide 5 are presently endowed with sufficient resources to enable them to meet the majority of their objectives. However, the anticipated budget reduction casts this in doubt for the future, particularly in view of the facts that (1) sub-program learning from technology validation programs will have to be speeded up and (2) large-scale deployment of $H_2$ and fuel cell technologies is coming ever nearer.
- The sub-program seems to need to be redeployed to meet the new finding framework. There is clear competition among all of the alternative energy fuels. As a result, there seems to be a bit of a free for all. As a result, funding seems to be moving out of the R&D part and instead focusing on industries that are focused on near-commercial opportunities. There is not a clear plan, nor is there proper coverage for, stationary and portable fuel cells. They seem to be shadowed by the automotive industry. Also, with SDOs losing their funding for standards development, it will greatly reduce the pace at which new technology standards will be written. SDOs will work hard to finish what is already in play, but the new standards will be hard to get started.
- It is not clear how projects are selected. Unlike other sub-programs, this sub-program does not compete its portfolio. Competitive solicitations for all parts of this sub-program are recommended to ensure that the most qualified teams are performing the work. The use of consultants is extensive, and it may not be the best use of limited program funds.

## 4. Other Comments:

- The importance of this program remains high, as $H_2$ and fuel cells (all applications) progress into commercialization.
- The DOE Hydrogen and Fuel Cells Program has been working hard to increase the exposure and acceptance of fuel cells and $H_2$. This support is critical to the United States and to industry success.
- The leadership, R&D activities, and outreach parts of the sub-program are all performing excellently in spite of shrinking budgets. It is unclear how well these parts are coordinated. A feedback mechanism is implied in the approach overview; yet it is unclear how well that is working to improve the efficiency and guide the sub-program approach. It is also unclear whether the outreach programs are gaining "champions" from their audiences, and whether the industry is helping lead the identification of R&D priorities, gaps in codes and standards, or gaps between safety "best practices" and codes and standards.
- Regulations, codes, and standards (RCS) is a critical need area, not just to ensure the safe deployment of $H_2$ technologies (which is a must), but also to ensure that relevant national and internationally harmonized RCS are in place to accelerate the deployment and to ensure global commercialization of $H_2$ technologies. With the imminent rollout of $H_2$ fueling stations globally (in Germany, Japan, Korea, and the United States [particularly New York and California]), relevant RCS must be in place so as to not hinder the rollout and to ensure that $H_2$ technologies are deployed safely. The global leadership of this program in the international and domestic RCS community is clearly recognized. This is a critical and arguably a rate-limiting component to the deployment of $H_2$ technologies, and it should be funded at a much more robust level than the $5 million U.S. dollars proposed in the 2013 request, particularly since the development of RCS is a critical path element to the rollout of $H_2$ fueling stations and the deployment of fuel cell technologies in the transportation sector. Relevant RCS work to ensure the safe deployment of these technologies is not just a market deployment issue, but one of safety. The original equipment manufacturers are on a path to deploy hundreds of thousands of vehicles in a very near time frame (2014–2017); commensurate with this is the planned infrastructure roll out (i.e., hundreds of refueling stations worldwide). Relevant RCS must be in place to ensure the harmonization and safe development of this infrastructure. This sub-program is not only on the critical path for deployment; it is also on the critical path for the safe deployment of these technologies. This sub-program should be given a much higher priority in funding allocation closer to its previous funding levels of $15 million.
- Some attention must be devoted in order to avoid duplication of tasks between some projects. The role of DOE, in order to oblige fuel cell and $H_2$ projects to consider safety as an important issue from the start until their final implementation, should be considered not only for projects supported by DOE but also for projects that are privately supported. This will help to develop, from the start of the deployment of these technologies, a safe industry with an excellent image in the population.
- There is a visible trend toward funding national laboratories at the expense of support for SDOs. There are experienced individuals who make things happen within those organizations that may indeed not be funded in

the future. It would be a mistake to allow experience to go down the drain for the sake of consolidating reporting structures.

- Suggestions for slide 4: replace or otherwise correct the term “foundational” $H_2$ behavior (it is unclear whether the presenter meant “fundamental”). In addition, next to international harmonization (bottom box), there is also a need for collaboration between stakeholders (going beyond mere consultation) and for pooling of brain, infrastructural, and financial resources. This reviewer also has suggestions for the organization of the SCS session: effort should be paid to improve the sequence of the presentations in the session (e.g., for ease of comprehension, SCS-011 should have been presented before SCS-010, which should have immediately followed it).

## Education Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- Yes, the sub-program was well outlined and adequately covered. All significant issues and challenges were addressed. Annual progress was clearly identified and highlights were presented. Work and progress were clearly consistent with overall DOE goals and objectives.
- Yes, to all questions. The education element of the overall program is critically important to the successful adoption of these technologies. This is particularly relevant now, as infrastructure rollout is starting in preparation for the early stages of vehicle commercialization. A significant challenge facing this sub-program is funding. Interestingly enough, funding in this critical area goes a long way. The deployment of hydrogen technologies will only accelerate and benefit from a more robust funding of the Education sub-program.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Plans and methods to address challenges were identified, but future work remains uncertain due to the end of the state and regional program(s).

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's R&D needs?

- Yes, this sub-program has made an important impact on the knowledge and comfort levels that the general public, educators, decision makers, first responders, and others have with these technologies. However, as the first deployments of hydrogen technology is upon us, improvement is required. Several current examples exist where, although responsible entities worked hard to educate the public and authorities having jurisdiction, events occurred where the impact could have been minimized with further education.
- Yes, the sub-program appears focused, well managed, and effective, from both a state and a regional level, and appears to increase exposure and impact.

### 4. Other Comments:

- Funding for this sub-program should be increased.
- The presenter has remained a steady and sound supporter of the sub-program, and has provided excellent managerial support. The sub-program should be funded in the future to include state and regional education efforts, partnership building, policy formation, and information management. Without these types of projects, deployment will advance at a much slower rate and there will be potential for missed opportunities.

## Market Transformation Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- The 2011 activities were clearly presented. Challenges and important issues have not been explained very clearly. In addition, the link between the different projects is not very well established. However, the sub-program is very important for the success of $H_2$ implementation.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Issues and challenges, and plans to address them should be presented clearly. The project portfolio is quite complete, but the overall structure could be better. The final objective, and how the projects fit into it, is unclear.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's R&D needs?

- There are a lot of different projects in the sub-program, but not all of them fit into a future perspective of a sustainable $H_2$-based energy system. The financial resources are limited, so it seems even more important to focus on projects that fit into the future perspectives. Direct methanol fuel cells might not be part of this.

### 4. Other Comments:

- This is an important sub-program that has a lot of very interesting programs, but its overall structure could be improved.

## Systems Analysis Sub-Program Comments

### 1. Was the sub-program area adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations of the sub-program if appropriate.)

- Yes, the sub-program area was adequately covered, issues/challenges were identified, and highlights of key progress were given.
- Yes, there was clear progress shown in every presentation.
- It is worthwhile to conduct studies on FCEV light-duty cars and trucks. However, these studies need to recognize the importance of the automakers' need to comply with a complex state and federal regulatory structure involving corporate average fuel economy, mobile source emissions, and zero emission vehicle mandates, which is not easy.
- This reviewer should have emphasized the results from the Technology Validation sub-program (183 FCEVs included in the test, 3.5 million miles, 500,000 trips, 25 $H_2$ stations, 35,700 refuelings, and so on) to emphasize that FCEVs are real and have been thoroughly tested (as opposed to battery electric vehicles [BEVs] and plug-in hybrid electric vehicles [PHEVs], which are just entering service and have not been similarly tested). Progress was not identified compared to previous years.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- Plans were given, and the sub-program is on the right track. Analyses, which have become more granular and specific (the reviewer especially liked the trend toward regional analyses), should continue to be so, and interaction between models and analyses should be integrated and increased.
- There are gaps in the portfolio, such as no analysis of what would happen if 120°C PEM fuel cells became reality or a really good, critical look at what it would take to do carbon capture and storage in the context of the rapid developments in the oil and gas industry.
- Researchers should conduct pathway modeling for the $H_2$ infrastructure cost upon reaching the dollars per gge target, which would be similar to the pathway established for PEM fuel cell stacks to hit the $30/kW target.
- One gap is the lack of a dynamic analysis of intermittent renewable electricity (primarily wind, but also solar) compared to the dynamic electricity load in a given region, and quantifying the benefits of $H_2$ storage over battery storage and compressed air energy storage as a function of the number of days of storage provided. The analysis would include both the direct benefits of load management (storing off-peak electricity for use on-peak) as well as the benefits to the grid, such as frequency stabilization. The study would also compare the environmental benefits of $H_2$ storage to back up wind farms as opposed to using NG combined cycle (CC) plants for wind firming (one paper claims that using NG turbines to back up wind actually increases greenhouse gas emissions [GHGs]), because ramping up an NG turbine reduces efficiency, such that running the NG turbine 100% of the time produces less GHGs than the wind/NG CC turbine combination. This paper also claims that the wind/NG turbine system generated more nitrogen oxides (NOx), because ramping the turbine increases NOx emissions. $H_2$ storage would eliminate this conundrum and make wind a true zero-emission baseload electricity source.

### 3. Does the sub-program area appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's R&D needs?

- Yes.
- The sub-program is extremely well managed and focuses very well, considering its limited resources.
- The sub-program is focused and well managed, but this reviewer would encourage the modeling on $H_2$ infrastructure to be crisper. This reviewer applauds the sub-program for its nimbleness in addressing the implications of the substantive drop in the NG price as a result of hydro-fracturing. This reviewer also applauds the sub-program for addressing FCEV light-duty vehicles.

## 4. Other Comments:

- In upcoming annual merit reviews, it might be helpful to add another slide at the beginning that PIs can use (if the project is not new) that summarizes the development of the project since it first started. For example, show the major additions/enhancements made to models, the types of issues that were covered, how the effort integrates with other Systems Analysis sub-program efforts, and where the project is going from this point on. This type of a summary could be provided at each project level and/or just the sub-program level. It is unclear what the role and outlook of the Systems Analysis sub-program is beyond 2015/2020 as commercialization nears, models are well-established, and a whole host of analyses have been conducted. Moving forward, it is unclear if the sub-program will provide more validation as more data is received from deployments or if it will conduct other types of analyses. This is something to consider, and perhaps give reviewers an outlook. It might be useful to consider the following together as a whole: typical $H_2$ refueling station development versus unconventional $H_2$ resources (e.g., biogas at wastewater treatment plants) and non-vehicle $H_2$ demand centers. Considering these together could provide insights about how they may help each other or how efforts could be leveraged.
- Slide 10 is misleading because it came from a third party that averaged data from various types of $H_2$ stations. As a result, the $H_2$ cost shown on slide 10 cannot be associated with any particular station type, but rather is an average of mobile refuelers, trucked-in gaseous and liquid $H_2$, and a few on-site electrolyzers or SMR. In addition, because the station type is not identified, this graph does not include variable costs, because it is not known whether to add NG costs (for an SMR station) or electricity costs (for an electrolyzer). Thus, the capital costs are compromised by the inclusion of lower cost station options, and the variable costs are not even included. Slide 11 is also worrisome because it seems to imply that $H_2$ infrastructure costs per vehicle are similar to or even higher than BEV costs, while other credible studies indicate the opposite. For example, the McKinsey & Company report on alternative vehicles in the European Union estimated that BEV and PHEV electrical infrastructure costs would be five times higher than $H_2$ infrastructure costs. On slide 13, the presenter should include the range for the FCEV (e.g., FCEV-350) to contrast it with the BEV-100, with only a 100-mile range.

## Comments on American Recovery and Reinvestment Act Activities

### 1. Were Recovery Act activities adequately covered? Were important issues and challenges identified? Was progress clearly presented in comparison to the previous year? (Include information presented in the plenary and/or session overview presentations if appropriate.)

- This has been a well-executed sub-program. The commercial sales validate the worthiness of DOE funding.
- Recovery Act activities were well covered. Important issues and challenges were well identified. (Lessons learned were highlighted, which is especially commendable.) Progress versus the prior year was not apparent, but this did not harm the quality of the presentation.
- The overall status of all of the Recovery Act projects was provided, with a quantification of deployments, jobs, funds spent, and safety. Siting and permitting were identified as some of the major challenges.

### 2. Are plans identified for addressing issues and challenges? Are there gaps in the project portfolio?

- The only weakness was the lack of a backup plan to replace the few projects that could not be started because of changes subsequent to award.
- Future activities and plans were not apparent. With only 14% of program funding remaining, perhaps they were not worth discussion. There was a good representation of follow-on (non-DOE funded) purchases of fuel cell material handling equipment (MHE); this is a great indicator of the program fulfilling Recovery Act goals.
- The project alluded to issues with the extended run-time project and stated that back-up strategies should be arranged in advance. In reality, this is very difficult to do, because these are, by definition, unanticipated events. However, structural mechanisms for dealing with this in the future can certainly be implemented.

### 3. Do these activities appear to be focused, well managed, and effective in addressing the DOE Hydrogen and Fuel Cells Program's needs?

- Yes.
- The projects appear to be well managed, and they have successfully leveraged DOE's investments to result in many more private-party purchases of fuel cell systems for field deployment. This was the ultimate goal of the Recovery Act, so the efforts have been successful at addressing DOE's objectives in this area.
- It was a bit hard to agree to this, as all the projects were funded under the Recovery Act sources. It would be better that they not be so identified. Certainly they were good projects.

### 4. Other Comments:

- The reviewer would like Composite Data Products for back-up power to be comparable to those for MHE.
- The Recovery Act appears to be successful in what it set out to do for fuel cells and $H_2$ technologies.

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## General Project Evaluation Form

This evaluation form was used for the following sub-program panels: Hydrogen Production and Delivery; Hydrogen Storage; Fuel Cells; Manufacturing R&D; Safety, Codes and Standards; Education; Market Transformation; Technology Validation; and Systems Analysis.

### PeerNet Evaluation Criteria: General Evaluation Form

*Provide specific, concise comments to support your evaluation. Please write clearly.*

**1. Relevance** to overall DOE objectives—the degree to which the project supports the Hydrogen and Fuel Cells Program and the goals and objectives in the *Multi-Year Research, Development, and Demonstration* (RD&D) *Plan*. (Weight = 20%)

**4 - Outstanding.** Project is critical to the Hydrogen and Fuel Cells Program and fully supports DOE RD&D objectives.
**3 - Good.** Most project aspects align with the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
**2 - Fair.** Project partially supports the Hydrogen and Fuel Cells Program and DOE RD&D objectives.
**1 - Poor.** Project provides little support to the Hydrogen and Fuel Cells Program and DOE RD&D objectives.

☐ 4 - Outstanding

☐ 3 - Good

☐ 2 - Fair

☐ 1 - Poor

**Comments on relevance to overall DOE objectives:**

**2. 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 - Outstanding.** Sharply focused on critical barriers; difficult to improve approach significantly.
**3 - Good.** Generally effective but could be improved; contributes to overcoming some barriers.
**2 - Fair.** Has significant weaknesses; may have some impact on overcoming barriers.
**1 - Poor.** Not responsive to project objectives; unlikely to contribute to overcoming the barriers.

☐ 4 - Outstanding

☐ 3 - Good

☐ 2 - Fair

☐ 1 - Poor

**Comments on approach to performing the work:**

**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 = 40%)

**4 - Outstanding.** Excellent progress toward objectives; suggests that barrier(s) will be overcome.
**3 - Good.** Significant progress toward objectives and overcoming one or more barriers.
**2 - Fair.** Modest progress in overcoming barriers; rate of progress has been slow.
**1 - Poor.** Little or no demonstrated progress toward objectives or any barriers.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**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 - Outstanding.** Close, appropriate collaboration with other institutions; partners are full participants and well coordinated.
**3 - Good.** Some collaboration exists; partners are fairly well coordinated.
**2 - Fair.** A little collaboration exists; coordination between partners could be significantly improved.
**1 - Poor.** Most work is done at the sponsoring organization with little outside collaboration; little or no apparent coordination with partners.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**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. (Weight = 10%) *[NOTE: if a project has ended, please leave this section blank.]*

**4 - Outstanding.** Plans clearly build on past progress and are sharply focused on barriers.
**3 - Good.** Plans build on past progress and generally address overcoming barriers.
**2 - Fair.** Plans may lead to improvements, but need better focus on overcoming barriers.
**1 - Poor.** Plans have little relevance toward eliminating barriers or advancing the Program.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**Comments on proposed future work:**

**Project strengths:**

**Project weaknesses:**

**Recommendations for additions/deletions to project scope:**

## American Recovery and Reinvestment Act Project Evaluation Form

This evaluation form was used for the American Recovery and Reinvestment Act panel.

### PeerNet Evaluation Criteria: Recovery Act

*Provide specific, concise comments to support your evaluation. Please write clearly.*

#### 1a. Relevance

Is the project effort relevant to the American Recovery and Reinvestment Act of 2009 (Recovery Act) goals of creating new jobs as well as saving existing ones, spurring economic activity, and investing in long-term economic growth? (Weight = 10%)

**4 - Outstanding.** Project is very relevant and will make substantial contributions to the Recovery Act goals.
**3 - Good.** Project is relevant and will make moderate but significant contributions to the Recovery Act goals.
**2 - Fair.** Project is somewhat relevant and will make some contribution to the Recovery Act goals.
**1 - Poor.** Project is not relevant and is unlikely to contribute to the Recovery Act goals.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**Comments on relevance of the project to the Recovery Act—create new jobs as well as save existing ones, spur economic activity, and invest in long-term economic growth:**

#### 1b. Relevance

Does the project's technology development plan and/or deployment plan address the DOE Fuel Cell Technologies (FCT) Program's Recovery Act project goals of accelerating the commercialization and deployment of fuel cells and fuel cell manufacturing, installation, maintenance, and support services? (Weight = 10%)

**4 - Outstanding.** Project is very relevant and will make substantial contributions to FCT Recovery Act project goals.
**3 - Good.** Project is relevant and will make moderate but significant contributions to FCT Recovery Act project goals.
**2 - Fair.** Project is somewhat relevant and will make some contributions to FCT Recovery Act project goals.
**1 - Poor.** Project is not relevant and is unlikely to contribute to the FCT Recovery Act project goals.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**Comments on relevance—does the project's technology development plan and/or deployment plan address the FCT Recovery Act project goals of accelerating the commercialization and deployment of fuel cells and fuel cell manufacturing, installation, maintenance, and support services?**

## 2. Development/deployment approach

Are the project's technical and deployment milestones and schedule clearly identified, appropriate, and feasible, and are technical and commercial barriers and risks adequately addressed? (Weight = 30%)

**4 - Outstanding.** Project team sharply focused on achieving milestones, overcoming barriers, and managing risks; difficult to improve approach significantly.
**3 - Good.** Appropriate milestones and schedule identified, and barriers and risks addressed. Effort likely to achieve project goals, but approach could be improved.
**2 - Fair.** Approach has significant weaknesses, but may contribute toward achieving most project goals.
**1 - Poor.** Unlikely to make progress toward project goals and/or barriers; risks are not adequately addressed.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**Comments on development/deployment approach:**

## 3. Technical accomplishments and progress

What is the overall progress toward project's objectives and milestones? Is progress adequately reported and quantified (e.g., number of jobs, installations, etc.) as required by the Recovery Act? (Weight = 40%)

**4 - Outstanding.** Excellent progress toward the objectives and milestones; barrier(s) likely to be overcome.
**3 - Good.** Significant progress toward objectives and overcoming one or more barriers.
**2 - Fair.** Rate of technical progress is slow; some progress made in overcoming barriers.
**1 - Poor.** Little or no demonstrated progress toward objectives, or toward overcoming barriers.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**Comments on technical approach and progress:**

### 4. Collaborations

Does the project team effectively use collaborations between partners and with other industrial, commercial, university, or research organizations to achieve its objectives? (Weight = 10%)

**4 - Outstanding.** Effective collaboration between partners and with other institutions enhances probability of success of effort.
**3 - Good.** Some collaboration exists; partners are fairly well coordinated.
**2 - Fair.** Minimal collaboration exists; coordination between partners could be improved.
**1 - Poor.** There is little coordination between partners or collaboration with other organizations.

- ☐ 4 - Outstanding
- ☐ 3 - Good
- ☐ 2 - Fair
- ☐ 1 - Poor

**Comments on collaborations:**

### Project strengths:

### Project weaknesses:

### Specific recommendations:

## List of Projects Not Reviewed

| Project ID | Project Title | Principal Investigator Name | Organization |
|---|---|---|---|
| BES-001 | Overview of the BES Hydrogen Storage Activities | John Vetrano | U.S. Department of Energy, Office of Basic Energy Sciences |
| BES-002 | From Fundamental Understanding to Predicting New Nanomaterials for High-Capacity Hydrogen Storage | Taner Yildrim | National Institute of Standards and Technology |
| BES-003 | Novel Theoretical and Experimental Approaches for Understanding and Optimizing Hydrogen-Sorbent Interactions in Metal Organic Framework Materials | Yves Chabal | University of Texas at Dallas |
| BES-004 | Design and Synthesis of Chemically and Electronically Tunable Nanoporous Organic Polymers for Use in Hydrogen Storage Applications | Hani El-Kaderi | Virginia Commonwealth University |
| BES-005 | Atomistic Mechanisms of Metal-Assisted Hydrogen Storage in Nanostructured Carbons | Nidia Gallego | Oak Ridge National Laboratory |
| BES-006 | Elucidation of Hydrogen Interaction Mechanisms with Metal-Doped Carbon Nanostructures | Ragaiy Zidan | Savannah River National Laboratory |
| BES-007 | Synthetic Design of New Metal-Organic Framework Materials for Hydrogen Storage | Pingyun Feng | University of California, Riverside |
| BES-008 | New Pathways and Metrics for Enhanced, Reversible Hydrogen Storage in Boron-Doped Carbon Nanospaces | Peter Pfeifer | University of Missouri-Columbia |
| BES-009 | Novel Molecular Materials for Hydrogen Storage Applications | Maddury Somayazulu | Carnegie Institute of Washington |
| BES-010 | Energy Storage in Clathrate Hydrogen Material | Carolyn Koh | Colorado School of Mines |
| BES-011 | Hydrogen Caged in Carbon—Exploration of Novel Carbon-Hydrogen Interactions | Angela Lueking | Pennsylvania State University |
| BES-012 | Complex Hydrides - A New Frontier for Future Energy Applications | Vitailij Pecharsky | Ames Laboratory |
| BES-013 | Atomistic Transport Mechanisms in Aluminum-Based Hydrides | Jason Graetz | Brookhaven National Laboratory |

| Project ID | Project Title | Principal Investigator Name | Organization |
|---|---|---|---|
| BES-014 | Kinetics and Thermodynamics of Metal and Complex Hydride Nanoparticles | Chris Wolverton | Northwestern University |
| BES-015 | Computational Studies of Hydrogen Interactions with Storage Materials | Chris Van de Walle | University of California, Santa Barbara |
| BES-016 | Discovery of a New Species in the Hydrogen Chemistry of $NaAlH_4$ in *In Situ* NMR | Mark Conradi | Washington University in St. Louis |
| BES-019 | Activation of Hydrogen with Bi-Functional Ambiphillic Catalyst Complexes | Tom Autrey | Pacific Northwest National Laboratory |
| BES-020 | Heavy Cycloadditions: Reactions of Digallene with Cyclic Polyolefins | Philip Power | University of California, Davis |
| BES-021 | Ammonia-Borane: A Promising Material for Hydrogen Storage | Larry Sneddon | University of Pennsylvania |
| BES-023 | Ammonia-Borane under High Pressure | Jiuhua Chen | Florida International University |
| ST-018 | Improving Porosity and H2-Affinity of Porous Framework Materials | Joe Zhou | Texas A&M University |
| ST-031 | Advanced, High-Capacity Reversible Metal Hydrides | Craig Jensen | University of Hawaii |
| ST-034 | Aluminum Hydride | Jim Wegrzyn | Brookhaven National Laboratory |
| ST-038 | Hydrogen Storage by Novel CBN Heterocycle Materials | Shih-Yuan Liu | University of Oregon |
| ST-052 | Best Practices for Characterizing Engineering Properties of Hydrogen Storage Materials | Karl Gross | H2 Technology Consulting LLC |
| ST-063 | Electrochemical Reversible Formation of Alane | Ragaiy Zidan | Savannah River National Laboratory |
| ST-085 | Glasses and Nanocomposites for Hydrogen Storage | Kristina Lipinska-Kalita | University of Nevada, Las Vegas |

| Project ID | Project Title | Principal Investigator Name | Organization |
|---|---|---|---|
| ST-100 | Hydrogen Storage Cost Analysis, Preliminary Results | Brian James | Strategic Analysis, Inc. |
| ST-101 | Enhanced Materials and Design Parameters for Reducing the Cost of Hydrogen Storage Tanks | Kevin Simmons | Pacific Northwest National Laboratory |
| ST-102 | Room Temperature Hydrogen Storage in Nano-Confined Liquids | John Vajo | HRL Laboratories, LLC |
| ST-103 | Hydrogen Storage in Metal-Organic Frameworks | Jeffrey Long | Lawrence Berkeley National Laboratory |
| ST-104 | Novel Carbon (C)-Boron (B)-Nitrogen (N)-Containing H2 Storage Materials | Shih-Yuan Liu | University of Oregon |
| ST-105 | Ultra Lightweight High Pressure Hydrogen Fuel Tanks Reinforced with Carbon Nanotubes | Dongsheng Mao | Applied Nanotech, Inc. |
| ST-106 | Alternative Fiber Evaluation and Optimization of Filament Winding | Mark Leavitt | Quantum Fuel Systems Technologies Worldwide |
| ST-107 | The Quantum Effects of Pore Structure on Hydrogen Adsorption | Raina Olsen | Oak Ridge National Laboratory |
| PD-020 | Inexpensive Delivery of Cold Hydrogen in Glass Fiber Composite Pressure Vessels | Andrew Weisberg | Lawrence Livermore National Laboratory |
| PD-051 | Surface Validation: Physical and Electronic Characterization of Materials for Photoelectrochemical Hydrogen Production | Clemens Heske | University of Nevada, Las Vegas |
| PD-052 | PEC Materials: Theory and Modeling | Muhammad Huda | University of Texas at Arlington |
| PD-056 | Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen | Liwei Xu | Midwest Optoelectronics, LLC |
| PD-058 | Characterization and Optimization of Photoelectrode Surfaces for Solar-to-Chemical Fuel Conversion | Tadashi Ogitsu | Lawrence Livermore National Laboratory/National Renewable Energy Laboratory |
| PD-076 | Photoelectrochemical Generation of Hydrogen from Water Using Nanotube-Based Semiconductor Systems for Improved Visible Light Activity | Mano Misra | University of Nevada, Reno |
| PD-077 | Solar Energy Utilization | Ravi Subramanian | University of Nevada, Reno |

| Project ID | Project Title | Principal Investigator Name | Organization |
|---|---|---|---|
| PD-082 | Process Intensification of Hydrogen Unit Operations Using an Electrochemical Device | Glenn Eisman | H2 Pump LLC |
| PD-085 | Hour-by-Hour Cost Modeling of Optimized Central Wind-Based Water Electrolysis Production | Chris Ainscough | National Renewable Energy Laboratory |
| PD-089 | H2A Hydrogen Production Analysis Model Version 3 | Darlene Steward | National Renewable Energy Laboratory |
| PD-090 | Low Cost Large Scale PEM Electrolysis for Renewable Energy Storage | Katherine Ayers | Proton OnSite |
| FC-001 | Advanced Cathode Catalysts and Supports for PEM Fuel Cells | Mark Debe | 3M |
| FC-002 | Highly Dispersed Alloy Catalyst for Durability | Lesia Protsailo | UTC Power |
| FC-011 | Molecular-scale, Three-dimensional Non-Platinum Group Metal Electrodes for Catalysis of Fuel Cell Reactions | John Kerr | Lawrence Berkeley National Laboratory |
| FC-015 | Improved Accelerated Stress Tests Based on FCV Data | Timothy Patterson | UTC Power |
| FC-025 | Air Cooled Stack Freeze Tolerance | Dave Hancock | Plug Power, Inc. |
| FC-027 | Development and Validation of a Two-phase, Three-dimensional Model for PEM Fuel Cells | Ken Chen | Sandia National Laboratories |
| FC-030 | Water Transport in PEM Fuel Cells: Advanced Modeling, Material Selection, Testing, and Design Optimization | Vernon Cole | CFD Research Corp. |
| FC-031 | Development and Demonstration of a New Generation High Efficiency 10kW Stationary PEM Fuel Cell System | Durai Swamy | Intelligent Energy |
| FC-035 | Lead Research and Development Activity for DOE's High Temperature, Low Relative Humidity Membrane Program | James Fenton | University of Central Florida |
| FC-038 | NanoCapillary Network Proton Conducting Membranes for High Temperature Hydrogen/Air Fuel Cells | Peter Pintauro | Vanderbilt University |
| FC-039 | Novel Approaches to Immobilized Heteropoly Acid (HPA) Systems for High Temperature, Low Relative Humidity Polymer-Type Membranes | Andrew Herring | Colorado School of Mines |

| Project ID | Project Title | Principal Investigator Name | Organization |
|---|---|---|---|
| FC-040 | High Temperature Membrane with Humidification-Independent Cluster Structure | Ludwig Lipp | FuelCell Energy, Inc. |
| FC-041 | Novel Approach to Advanced Direct Methanol Fuel Cell Anode Catalysts | Huyen Dinh | National Renewable Energy Laboratory |
| FC-042 | Advanced Materials for RSOFC Dual Operation with Low Degradation | Randy Petri | Versa Power |
| FC-043 | Resonance-Stabilized Anion Exchange Polymer Electrolytes | Yu Seung Kim | Los Alamos National Laboratory |
| FC-051 | The Fuel Cell Testing at the Argonne Fuel Cell Test Facility | Ira Bloom | Argonne National Laboratory |
| FC-075 | Fuel Cell Balance of Plant Reliability Testbed | Susan Shearer | Stark State College |
| FC-097 | Stationery and Emerging Market Fuel Cell System Cost Analysis | Kathya Mahadevan | Battelle |
| 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 |
| FC-100 | High Aspect Ratio Nano-Structured Pt-based PEM Fuel Cell Catalysts | Brian Larsen | National Renewable Energy Laboratory |
| MN-011 | Cause and Effect: Flow Field Plate Manufacturing Variability and its Impact on Performance | Eric Stanfield | National Institute of Standards and Technology |
| TV-006 | Validation of an Integrated Hydrogen Energy Station | Ed Heydorn | Air Products |
| TV-007 | California Hydrogen Infrastructure Project | Ed Heydorn | Air Products |
| TV-009 | Hawaii Hydrogen Power Park | Richard Rocheleau | Hawaii Natural Energy Institute |
| TV-014 | Sustainable Hydrogen Fueling Station, California State University, Los Angeles | David Blekhman | Cal State LA University Auxiliary Services, Inc. |
| TV-016 | Stationary Fuel Cell Evaluation | Jennifer Kurtz | National Renewable Energy Laboratory |

| Project ID | Project Title | Principal Investigator Name | Organization |
|---|---|---|---|
| TV-017 | Next Generation H2 Station Analysis | Sam Sprik | National Renewable Energy Laboratory |
| SCS-013 | International Energy Agency Hydrogen Implementing Agreement Task 31 Hydrogen Safety | William Hoagland | Element One, Inc. |
| ED-012 | State and Local Government Partnership | Joel Rinebold | Connecticut Center for Advanced Technology, Inc. |
| ED-014 | H2L3: Hydrogen Learning for Local Leaders | Patrick Serfass | Technology Transition Corporation |
| ED-017 | H2 Educate! Hydrogen Education for Middle Schools | Mary Spruill | National Energy Education Development Project |
| AN-028 | Evaluation of U.S. DOE Energy Recovery Act Fuel Cell (Technologies Program) Initiative (ARRA-FCI) | Toni Marechaux | Strategic Analysis, Inc. |
| H2RA-004 | Advanced Direct Methanol Fuel Cell for Mobile Computing | Jim Fletcher | University of North Florida |
| H2RA-006 | PEM Fuel Cell Systems Providing Backup Power to Commercial Cellular Towers and an Electric Utility Communications Network | Mike Maxwell | ReliOn Inc. |
| H2RA-009 | Fuel Cell-Powered Lift Truck FedEx Freight Fleet Deployment | John King | FedEx Freight |
| H2RA-010 | Fuel Cell-Powered Lift Truck Sysco Houston Fleet Deployment | Scott Kliever | Sysco of Houston |
| H2RA-011 | GENCO Fuel Cell Powered Lift Truck Fleet Deployment | Jim Klingler | GENCO |

## 2012 AMR Survey Questionnaire Results

### 1. All Respondents

#### 1.1. What is your affiliation?

| | Number of Responses | Response Ratio |
|---|---|---|
| Government agency directly sponsoring the program under review | 10 | 4.4% |
| National/government lab, private-sector or university researcher whose project is under review | 54 | 24.1% |
| Non-government institution that received funding from the program(s) under review | 49 | 21.8% |
| Non-government institution that does not receive funding from the program(s) under review | 37 | 16.5% |
| Government agency with interest in the work | 13 | 5.8% |
| National/government lab, private-sector or university researcher not being reviewed | 33 | 14.7% |
| Other | 25 | 11.1% |
| No Response | 3 | 1.3% |
| **Total** | 224 | 100% |

**"Other" Responses**

- Contractor at government agency sponsoring program under review
- Community college adjunct professor
- Private sector business that has received other government funding
- Consultant
- Private company
- Non-profit
- Reviewer from Germany
- Non-U.S. government organization
- Consultant
- State government whose project was under review
- Commercial business
- Oak Ridge Institute for Science and Education (ORISE)
- Reviewer
- American Association for the Advancement of Science Policy Fellow working with Fuel Cells Technologies Program
- Foreign institution
- Industry
- Hydrogen technology consultant
- International Partnership for Hydrogen and Fuel Cells in the Economy member
- European Commission
- Federal peer reviewer
- Fuel cell and hydrogen consultant for Germany; www.efceco.com
- University of South Carolina
- Private company
- Consulting group
- Renewable energy developer

### 1.2. Purpose and scope of the Annual Merit Review (AMR) were well defined by the Joint Plenary Session (answer only if you attended the Joint Plenary on Monday).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 4 | 0 | 15 | 54 | 46 |
| 3% | 0% | 13% | 45% | 39% |

*20 Comment(s)*

- The purpose and scope of the Annual Merit Review (AMR) was well explained during the Joint Plenary Session.
- The purpose was very well promoted and built on the success of previous years.
- The purpose was not clearly described.
- The quality of the overview presentations made by the U.S. Department of Energy (DOE) staff varied.
- The purpose, to review DOE Vehicle Technologies Program and Hydrogen and Cells Program research, was defined appropriately.
- The Joint Plenary Session is very important to all of the attendees because it explains the purpose and scope of the DOE Hydrogen and Fuel Cells and Vehicle Technologies Programs (the Programs).
- The Joint Plenary Session was very useful since it is impossible to attend all of the parallel sessions (and thus the overviews at the beginning of each). This session provided an opportunity to obtain the full scope of the DOE Programs.
- The concept (that this was a review rather than a conference) was not very well defined in the plenary session (not that it had to be).
- The presentations were well done, but perhaps forced. The motive of "getting value for the money" was clearly emphasized.
- The first presentation was somewhat hypothetical with a high-level focus and no direct connection to the two DOE Programs.
- The quality of the plenary speakers' presentations was high; however, the print on the lunch session slides was very difficult to read. The reviewer requested copies of the plenary speakers' slides.
- The main objectives of the Programs and the link to the individual projects could have been presented more clearly.
- The plenary presentation was OK.
- The plenary session described the Programs and highlights, but little was said about the review process.
- The presentations could have been more big picture, high level, and inspirational, rather than detailed. The details were covered in the sub-program overviews in the afternoon.
- The review process is not very effective—there was not enough detail provided and the rating scale is too broad.
- The overviews were good, but it is difficult to absorb so much information in such a short time.
- The plenary talks are in general very uninspiring. The presentations could at least be delivered with some enthusiasm, even if feigned. The vision and the new achievements are far more interesting than the budget. While the budget is obviously important, it does not belong in a plenary talk to technical people.
- Present slides that are easier to digest—less bits and pieces and more clear statements and information; this applies to many presentations throughout the meeting. Prescribe a leaner and clearer template for the slides.
- Increase the focus on the objectives and criteria for the reviews. No one explained how DOE would use the reviews. Too much focus was on PeerNet and not enough on what DOE wanted from the reviewers.
- The Hydrogen and Fuel Cells Program talk was outstanding. The keynote speaker's was OK. Possible future speakers could include: Nate Lewis (California Institute of Technology), Dan Nocera (Massachusetts Institute of Technology), Pete Diamandis (author, *Abundance: the Future is Better than You Think*), or Elon Musk (CEO, Tesla Motors). It should be noted that Diamandis and Musk need to be educated on hydrogen.

### 1.3. The two plenary sessions after the Joint Plenary Session were helpful to understanding the direction of the Hydrogen and Fuel Cells and Vehicle Technologies Programs (answer only if you attended either the Hydrogen and Fuel Cells or Vehicle Technologies plenary sessions on Monday).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 3 | 1 | 13 | 63 | 43 |
| 2% | 1% | 11% | 51% | 35% |

*11 Comment(s)*

- The Hydrogen and Fuel Cells Program Plenary Session was outstanding.
- The Vehicle Technologies Program Plenary Session was helpful.
- The long-term perspective of both Programs would be interesting.
- There was a lot of repetition between the joint plenary and the individual plenary sessions.
- This session seemed to be a summary of progress of various projects; it did not seem to convey any sort of overall Program direction.
- Other than some indications about progress toward specific goals, there was no overall mention about the direction of the Programs.
- Separate plenary sessions allowed for more detail on the individual sub-programs, helping to provide additional context for the research to be reviewed.
- Generally, the sessions were helpful, but the sound quality in the Hydrogen and Fuel Cells Program session was poor. The slides that are prepared for these presentations are great documentation for the vastness of this research, but they can be overwhelming to even those familiar with the Program. More emphasis should be placed on simpler slides with key takeaway messages.
- The relationship/correlation between the different activities within each program was very clearly laid out through the series of talks during this session.
- The following sessions need to be longer: thermoelectric material application and waste heat recovery system application. There was too much information on the slides.
- It is unclear whether the DOE Fuel Cell Technologies Program is phasing out, going up, or remaining flat in the years to come.

### 1.4. Sub-program overviews were helpful to understanding the research objectives. (Answer only if you attended one or more sub-program overviews.)

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 7 | 2 | 12 | 88 | 70 |
| 4% | 1% | 7% | 49% | 39% |

*15 Comment(s)*

- The details were good and, mostly, presented very well.
- The sub-program overviews/presentations needed to be longer.
- The overviews were quick summaries of the projects; they did not provide insight into the overall sub-program direction.
- The overviews provided additional details for each sub-program and its motivations, which was helpful information.
- Because the sub-programs are so well integrated, there was some duplication between the upper level and the sub-program.

- The overviews are helpful because the reviewers can understand more recent progress and discuss with the presenter about the research fields. Peer reviewers will give their judgements for the projects.
- The main objectives of the sub-programs and the link to the individual projects could be presented more clearly.
- The sub-program managers spent very little time discussing progress between the previous year and the current year, even though DOE AMR reviewers were asked to use this as a criterion for evaluation.
- Poor sound quality got in the way. Some reference to the Hydrogen Safety Panel should have been included in the sub-program overview.
- The Lightweight Materials overview provided great information on future trends.
- These overviews provided more detailed information about the sub-programs, which would be difficult to ascertain without those briefs.
- Sometimes the manager tried to present too much material. If the presenter talks too fast, the audience cannot keep up.
- Some of these presentations often included busy and complex slides from which it was difficult to extract and assimilate the necessary and useful information.
- Good overview, although there was too much focus on the money and not enough on the mission.
- The individual sub-program managers say similar things, but the actions/directions of their sub-programs often do not reflect what they say.

### 1.5. What was your role in the AMR? 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 | 97 | 43.3% |
| Presenter of a project | 72 | 32.1% |
| Peer Reviewer | 54 | 24.1% |
| No Response | 1 | <1% |
| **Total** | 224 | 100% |

## 2. Responses from “Attendee, neither Reviewer nor Presenter"

### 2.1. The quality, breadth, and depth of the following were sufficient to contribute to a comprehensive review:

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| | Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|---|
| Presentations | 0 | 1 | 7 | 57 | 26 |
| | 0% | 1% | 8% | 63% | 29% |
| Question and answer periods | 0 | 4 | 14 | 50 | 24 |
| | 0% | 4% | 15% | 54% | 26% |
| Answers provided to programmatic questions | 0 | 2 | 22 | 48 | 18 |
| | 0% | 2% | 24% | 53% | 20% |
| Answers provided to technical questions | 0 | 1 | 12 | 54 | 25 |
| | 0% | 1% | 13% | 59% | 27% |

*7 Comment(s)*

- This attendee did not hear any programmatic questions.
- More time was needed for questions and answers (Q&A).
- Many of the presenters did not leave time for questions. Interesting discussions had to be cut off because of time constraints.

- It is difficult to provide a comprehensive review with so many projects in only a few days; but, overall, it is a very good review of the DOE Programs. For a more detailed review, perhaps based on feedback from a preliminary review like this, there may need to be more detailed site reviews.
- In many presentations, either more presentation time was needed to better cover the material, or more time for questions should have been provided.
- The format was good and provided adequate time for the presenters to describe their work in good detail and for Q&A periods.
- It would be nice to have more time to interact and ask questions of presenters.

### 2.2. Enough time was allocated for presentations.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 5 | 6 | 59 | 21 |
| 1% | 5% | 7% | 64% | 23% |

*9 Comment(s)*

- Enough time was allocated; however, some presenters are not skilled public speakers.
- Forty-five minutes would be better.
- Five more minutes for Q&A would have been useful, even if it meant shortening the presentations.
- It might be nice to have time for one more technical slide.
- DOE needs to do something to the presenters that go over the time limit year after year. The moderator should have the ability to "disconnect" the presentation somehow. It is unclear how it is possible to fairly judge those who work within the rules when others do not abide by them and are unfairly allowed to present more information.
- The technical details provided by the presenters varied significantly. Some went into every detail, whereas others presented a cursory overview. Consistency might improve the review process.
- There is never enough time, but this attendee does not see an alternative.
- Some used the time well for program specifics; some used it well for technical specifics of the project. The attendee was looking for the project detail information.
- Large projects (i.e., multi-million dollars, multi-year) are hard-pressed to provide a comprehensive review of information in 20 minutes, resulting in presentations with too many slides crammed with too much data/information as presenters try to justify the work. Presenters should be required to focus. The attendee suggests having a sliding scale for presentation length depending on project size, duration (just starting versus four or five years old), funding, etc.

### 2.3. The questions asked by reviewers were sufficiently rigorous and detailed.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 0 | 9 | 31 | 41 | 12 |
| 0% | 10% | 33% | 44% | 13% |

*11 Comment(s)*

- The quality of the questions depended on the person.
- The quality of the questions varied quite a bit from sub-program to sub-program.
- The reviewers were not very engaged; they had few questions.
- One of the reviewers was reviewing a program that he owns.
- The questions could be slightly tougher.
- The review process could be more thorough.

- It is hard to expect reviewers to have enough presence of mind on the projects when the review interval is one year. This is too long for an independent reviewer to get up to speed on a short notice.
- The questions were fairly routine and mostly addressed “unlabeled” information; questions, for example, asked the presenter about specific test conditions represented on the graph. Thoughtful, in-depth questions were rarely asked. When they were, the presenters side stepped them as much as possible.
- In some presentations and Q&A sessions, company presenters were overly secretive and did not disclose sufficient information to make it interesting.
- The reviewers did not ask questions in many of the presentations. The few that were asked were sufficiently rigorous.
- Reviewer questions, which were usually excellent, were many times outside the scope of the project being presented.

### 2.4. The frequency (once per year) of this formal review process for this Program is:

| | Number of Responses | Response Ratio |
|---|---|---|
| About right | 85 | 37.9% |
| Too frequent | 1 | <1% |
| Not frequent enough | 4 | 1.7% |
| No opinion | 3 | 1.3% |
| No Response | 131 | 58.4% |
| **Total** | 224 | 100% |

*6 Comment(s)*

- Two or three reviews per year are optimal.
- More oral presentations would be preferred. The poster session started late after a very full day. The presenters were busy and it was difficult to have a meaningful discussion there; however, it was good for networking.
- One year is the correct time period. More than once is a waste of taxpayers’ money because there will not be a lot of progress. Two years seems too long.
- The review should be twice a year, with more emphasis on actual progress. Some of the poster sessions would have been better as presentations.
- For a formal review, one year is about right. But more frequent meetings, perhaps quarterly, to show results would be useful.
- Given the nature of the technical work (research and development), yearly formal review is appropriate to enable sufficient time for significant progress on each project.

### 2.5. Logistics, facilities, and amenities were satisfactory.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 0 | 3 | 6 | 35 | 49 |
| 0% | 3% | 6% | 38% | 53% |

*20 Comment(s)*

- The location was nice.
- There was no plug for a laptop.
- A fee should be charged to cover the excellent food and coffee services.
- The facilities were uncomfortably crowded during breaks.
- A less expensive hotel would be good.
- The location near a Washington Metro stop was a big plus.
- The space outside the presentation rooms could be larger. It got crowded very quickly.

- Walking between buildings made it impossible to attend all the desired presentations.
- The meeting rooms were comfortable and the organization of the meeting was excellent.
- Find sponsorship for better food/drinks, rather than having a cash bar.
- This meeting had too many people; the gathering was too big. There is no benefit in mixing storage and fuel cells together in one meeting. Also, Washington, D.C., is too expensive.
- Easier access to wireless in the review rooms without paying additional hotel charges would be desirable.
- Wireless connection in the conference room is requested.
- The respondent appreciated the discounted rate at hotels. It would be nice to have a discounted airfare as well. A roundtrip from Detroit to D.C. was $900, even three weeks in advance, forcing the respondent to fly to Baltimore Washington International and spend over an hour getting to the hotel.
- Reviewers who have to attend reviews that are only in one hotel should be allowed to register and park in that hotel without having to go to the main hotel first. The Wardman Park location is superior and preferred over Crystal City.
- Logistics, facilities, and amenities were excellent. Providing table space for attendees to place their laptops/notebooks while taking notes would be more convenient. This may not be possible for the first day of overviews because of high attendance.
- There needs to be wireless access in the rooms for everyone, not just reviewers.
- Ideally, the venue would be large enough to have all the presentations at one location. At times, the reviewer had to hurry to move from one hotel to the other to see presentations.
- One elevator was not working and the other two seemed to be synchronized, so two were doing the work of one. The Internet connection in the rooms was poor.
- The Marriott was an excellent facility for the review. The staff was excellent, the food was great, and the accommodations were adequate.

### 2.6. The visual quality of the presentations was adequate. I was able to see all of the presentations I attended.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 0 | 5 | 8 | 47 | 32 |
| 0% | 5% | 9% | 51% | 35% |

*13 Comment(s)*

- Visual consistency and set up between each room was excellent.
- Some rooms are too big compared with the screen size.
- Some rooms were overcrowded, which only illustrates a lot of interest.
- Many presentations used graphs and plots with a font that was too small to see in the larger rooms.
- Many presenters had tiny font sizes that could not be read—even from the front few rows.
- Some of the screen information displayed was a little blurry and was hard to view.
- Some presenters use fonts that are too small and illegible figures, making it difficult to follow. Obviously this depends on the presenter.
- The audio/visual facilities and the technicians provided by the hotel were good. Some presenters had too much data per slide, resulting in small fonts.
- There needs to be a visual standard for all presentations. Some had a very small font that could not be seen in the back. Also, the color choices were poor on some presentations, causing confusion.
- Guidelines regarding minimum font size and the amount of information allowed on a slide would help. If the table or graph is not readable, the presenter should summarize the data differently to communicate the message.
- The bottom third of the screen is typically masked by the people in the front. It seems so easy to raise the screen (and perhaps use larger screens).
- Very small chairs led to crowding in some rooms; it was impossible to see around the people sitting in front to see the projected slides. This often led to a significant blockage of the view.

- The podium in some rooms was so close to the screen that the presenter could not read the slide. The mixing of the presentation formats (Adobe/pdf and Powerpoint/ppt) did not work well. Speakers would go up to the computer and try to access their slides and mess things up. After a while, the audio/visual person had to come up to the computer to bring up the next presentation. Another room had no pdf/ppt scheme—just a folder with all the presentations listed sequentially—that worked perfectly every time.

### 2.7. The audio quality of the presentations was adequate. I was able to hear all the presentations I attended.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 7 | 8 | 46 | 31 |
| 1% | 8% | 9% | 49% | 33% |

*12 Comment(s)*

- The audio quality was mostly adequate.
- There was poor audio quality in some of the rooms.
- The reviewer found it difficult to hear sometimes.
- Lunch sessions were very difficult to hear over the diners.
- Some presenters needed better microphone training. The available sound equipment was excellent.
- Audio quality was poor in some rooms. It was too loud and had feedback at times; at other times it was too quiet.
- The salons at the Marriott have terrible acoustics. The sounds were so distorted in Salon 3 that they were barely recognizable.
- There were several issues in Salon IV with audio. Because there was not a speaker in the back of the room, it made it difficult to hear.
- Only one microphone was available for Q&A, which wasted precious time and could not cover two parties during discussions.
- The audio equipment in the smaller sessions was poor. The sessions that were earlier or later were disturbed by the people outside.
- The attendee spent time in Salon III at the Gateway and suggested that the audio/visual crew put speakers on both sides of the audience. They were only located on the right side and made it hard to hear on the left side of the room.
- The audio/visual technician was sometimes a little late turning up the volume of a microphone; but after that, the volume was fine. There was too much noise in the snack area during the sessions. From the back of the room, it was difficult to hear the speaker because of all the noise outside the doors.

### 2.8. The meeting hotel accommodations (sleeping rooms) were satisfactory.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 1 | 21 | 39 | 22 |
| 1% | 1% | 25% | 46% | 26% |

*4 Comment(s)*

- The meeting hotel accommodations were too expensive. (3 responses)
- While D.C. is known to be pricey, better accommodations were expected.

### 2.9. The information about the Review and the hotel accommodations sent to me prior to the Review was adequate.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 0 | 0 | 6 | 46 | 40 |
| 0% | 0% | 7% | 50% | 43% |

*1 Comment(s)*

- The price is high and the number of rooms is limited.

### 2.10. What was the most useful part of the review process?

*60 Response(s)*

- The technical presentations were the most useful part of the review. (12 responses)
- The Q&A sessions were the most useful part of the review process. (5 responses)
- Interfacing/networking with other professionals was the most useful part of the review. (6 responses)
- The poster sessions and conversations during poster session times were the most useful parts of the review process. (3 responses)
- The CD containing all the presentations and posters, which was provided on Monday, was extremely useful as the presentations could be studied. (2 responses)
- The plenary sessions during the first day and the poster sessions were the most useful parts of the review.
- The electrochemical storage sessions and the good networking opportunity were both useful parts of the review. The AMR provides the opportunity to organize separate discussions/meetings around the presentations taking advantage of the presence of scientists otherwise hard to gather in one room.
- The most useful part was the Mitigation of Vehicle Fast Charge Grid Impacts with Renewables and Energy Storage.
- The overviews of the programs on the recent accomplishments, future challenges, and funding opportunities were most useful and the principal investigators' (PIs') presentations were very impressive.
- The most useful part of the process was the way the topics were grouped in sequence.
- The presentations and the reviewers' feedback are the most useful parts of the review process.
- Getting overviews of multiple sub-program areas were the most useful parts of the review process.
- The most useful part of the review was learning about the state-of-the-art research.
- The presentations and Q&A were good, especially when followed by breaks where follow-up questions and networking could be undertaken.
- The presentations and the discussions during the coffee breaks were the most useful parts of the review.
- The cost studies and the overview for alternative powertrains were the most useful parts of the review.
- Learning about the level and readiness of the new technologies sponsored by DOE was the most useful part of the review process.
- Meeting people in person, networking, and listening to presentations on similar topics were the most useful parts of the review.
- The AMR gave an excellent perspective of what was being done at the national level. Also, it provided opportunities for people to network with 30-minute breaks and poster sessions.
- The AMR provides an opportunity to review and discuss the progress of key programs.
- The most useful part of the process was learning about the progress of the development of technology.
- The most useful parts of the process were learning the project details and interacting with other related projects. Networking was also very important.
- The information obtained was the most useful part of the process.
- The AMR provided updates on all of DOE's programs in these areas in a single week and enabled reviewers to directly interact with both the DOE Program Managers and the performers.

- The respondent attended as a means to become more familiar with the various programs and found the experience very useful.
- The presentations and the discussions and meeting the people performing the best research worldwide in the topic were the most useful parts of the process.
- Overall, progress on the projects plus technical details and interacting with everyone were the most useful parts of the process.
- The first day gave a comprehensive overview of the work achieved.
- For the purpose of the reviewer's work, the Program and sub-program overviews were very useful.
- The technical presentations were most valuable, enabling the reviewer to catch up with the state-of-the-art of various technologies.
- The American Recovery and Reinvestment Act (Recovery Act) project reviews, especially on fuel cell forklifts, were the most useful parts of the process.
- Learning about the information available that was pertinent was the most useful part of the process.
- This first-time attendee felt that everything was useful.
- The review meeting (presentations and posters) was very helpful in understanding the research needs of the industry.
- This attendee was able to gain a good understanding of how the Energy Storage Program is managed and executed, and came home with a good understanding of the objectives of the program. It was good to meet those who are working in the program.
- The most useful part of the process was meeting with government researchers and being updated by them on where research is taking place.
- The AMR gave the reviewer the most comprehensive and unbiased overview of research in the lithium battery space, which was highly useful to the reviewer's program.

### 2.11. What could have been done better?

*33 Response(s)*

- The meeting was excellent. (2 responses)
- Everything was well organized. (2 responses)
- The audio and visual tools could have been better.
- Split the Hydrogen and Fuel Cells Program from the Vehicle Technologies Program so we can focus on the topic.
- This attendee would prefer getting the talks on a USB drive instead of a CD because most laptops no longer have CD drives.
- Shorten the Plenary session.
- There should be more time for questions from the audience.
- There could have been more time for presentations and questions.
- It would be better if a small summary of each topic is available at the website and in the handout so people can decide beforehand which presentation they will attend. (2 responses)
- There were too many sessions in parallel. The attendee wanted to attend both batteries and fuel cells and could not and suggests having them in separate times.
- There needed to be more access to DOE leadership. With such a high-profile event, a talk by the Secretary of Energy would have been appropriate.
- It was difficult to go back and forth between two different hotels. It would have been easier to have registration set up in both.
- The AMR should be held at a less expensive hotel with an improved sound system. There were relatively few questions, even from the reviewers. In some cases, the reviewers did not have a single question.
- It would have been better to invite more international peer reviewers to attend the meeting.
- Some presenters took too long to present and the time for questions was not enough. The presenters need to practice well in advance and leave more time for questions.
- The lighting was too dark in a certain poster session.

- There should be more discussion/presentations at the plenary about the overall place of the Hydrogen and Fuel Cells Program in the national energy policy.
- The presenters could have given an estimated readiness level to allow for a review of technologies closer to completion.
- Many of the presenters did not give very much background, which made following their talk difficult. Although there is a limited amount of time, a better background/introduction section is encouraged. Often, the "problem statement" and "approach" served this purpose well; however, it was often glossed over.
- This attendee would have preferred to be in a single hotel/building rather than two. The government-rate rooms sold out much too quickly.
- It would have been better to have ice cream bars at every break!
- It would have been better to have a larger facility—spilled coffee is painful.
- This is the best meeting in the field of the upcoming hydrogen-based economy!
- It would have been better to arrange for more time to interact with presenters and DOE staff.
- It would be useful to have the plenary session presentations in the CD provided during the AMR.
- This attendee noticed many of the presentations had no one sitting at the review desks and questioned if these were being reviewed or if the reviewers sat elsewhere.
- Scheduling could have been better by trying to condense the time spent in each sub-program, such as fuel cells, production, etc., by running parallel sessions. Also, moving more projects into the poster sessions would have been better.
- The food at the poster session receptions could be more suitable for a broad audience.
- It would have been better to make the plenary presentations available sooner.
- The refreshment breaks were chaotic and disturbed the technical session rooms for at least 15 minutes into the presentation following the 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 percentage is the percent of the total respondents selecting the option.

| Highly Satisfied | Dissatisfied | Neutral | Satisfied | Highly Satisfied |
|---|---|---|---|---|
| 0 | 1 | 4 | 55 | 31 |
| 0% | 1% | 4% | 60% | 34% |

*3 Comment(s)*

- It is a good venue with good facilities and easy to get on the Metro. There was a ton of information and the presenters are very accessible.
- Overall, it is an excellent meeting and the food is great.
- The evaluation methodology being used by the reviewers was unclear and the reviewers had fewer verbal questions and comments than at other reviews.

### 2.13. Would you recommend this review process to others, and should it be applied to other DOE programs?

| • | Number of Responses | Response Ratio |
|---|---|---|
| Yes | 91 | 40.6% |
| No | 2 | <1% |
| No Response | 131 | 58.4% |
| **Total** | 224 | 100% |

*8 Comment(s)*

- Yes, if there existed a sufficient critical mass.
- Yes, except make certain that all reviewers do not have a conflict of interest.

- In principle, yes, but the respondent can say nothing about other DOE programs.
- It is very important to monitor the progress of funded research to decide whether to discontinue or to suggest interventions to get the research back on track.
- This attendee has recommended this review process to others.
- This was one of the best merit reviews the respondent has attended.
- This would be very useful for the Sunshot Program.

### 2.14. Please provide comments and recommendations on the overall review process.

*20 Response(s)*

- The review was very good. (5 responses)
- The information provided is very useful. (3 responses)
- There were too many interesting presentations simultaneously.
- The meeting was very informative and well organized.
- The meeting was well run by DOE personnel.
- The attendee was satisfied with the overall review process and thanks the organizers for all their hard work!
- The management of the meetings and the catering was perfect.
- This first-time AMR attendee was interested in topics presented during poster sessions and would have liked to see oral presentations on some of them. The crowding in the venue was overwhelming at times, with little room to move to the side for more in-depth, or private conversations.
- The presenters should not go into detail about funding and not use light green as a color for text. It is too faint and therefore unreadable.
- The presenters should be objective and disclose the composition (or some basic description of the composition) of the sample that was shown in the presentation. The attendee understands the proprietary aspect, but felt that some generic description can be provided to understand the reasons for the differences in performance.
- There ought to be some discussion of the previous year's review and its effect on the project.
- The presenters could have given an estimated readiness level to allow for a review of the technologies that are closer to completion.
- This attendee would recommend the AMR to others seeking an overview of the fields; however, the very public nature of the event diminishes the frank technical exchange of a private review. The presenters should weigh in on this in terms of application to other DOE programs.
- If the review is part of the grant, then the PI should be required to submit their presentations on time or be scored to reflect that they were late. Knowing how taxpayer dollars are being spent and that they are well spent is important. If the PI cannot meet this deadline, the attendee questions what else the PI may be overlooking.

## 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 percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 2 | 1 | 25 | 24 |
| 2% | 4% | 2% | 47% | 45% |

*12 Comment(s)*

- This was very well done.
- It was helpful to have the prior-year presentation to measure the progress.
- The presentations were available in plenty of time to allow the reviewer to read over them and begin thinking about review inputs.
- A week earlier would have been nice.

- One of the reviews was late getting the Fiscal Year 2012 graphics posted. However, that did not cause any difficulty.
- The final information was not received for a couple of projects on the Friday before the AMR.
- Having the presentations ahead of time helped the reviewer to prepare questions for the PIs.
- The information was provided in a timely manner, but the level of detail is totally insufficient to do anything like a thorough review.
- This reviewer got more information for the 2012 review from the 2011 text report. The 2012 presentations do not provide enough detail in general.
- This reviewer felt hounded with administrative emails, but received almost nothing helpful about the review itself.
- Another reviewer got nothing but titles prior to the AMR. Ideally, this would have helped, although it is not clear that reviewers would really have had the time (or inclination) to review them before the meeting.
- It was unclear whether the reviewers needed to consider job creation in the reviews.

### 3.2. Review instructions were provided in a timely manner.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 1 | 1 | 20 | 30 |
| 2% | 2% | 2% | 38% | 57% |

*7 Comment(s)*

- Timely information was provided by the ORISE team. (2 responses)
- The webinar presentation was a good review.
- It was unclear how the presentation/review procedures worked together, but that may have been just because this was the reviewer’s first time as a reviewer/attendee.
- The PeerNet login information was difficult to find in the email.
- The review instructions were only fair. The webinar was OK. It would have been nice to have the technology development manager provide some context about what the reviewers were being asked to do.
- PeerNet worked very well for the reviewer this time. The professionalism and support of the ORISE team and webcast training sessions were very helpful. They do a really good job of handling the mechanics of the review and should be commended. The reviewer may have them point out that some of the projects are at very different stages on their funding timeline and that results have to be weighed against how much time they have spent on a project (some had barely started, for example).

### 3.3. The information provided in the presentations was adequate for a meaningful review of the projects.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 4 | 11 | 27 | 9 |
| 2% | 8% | 21% | 52% | 17% |

*23 Comment(s)*

- In general, the presenters provided enough information to evaluate the work, given the limitations of proprietary data and time constraints. (4 responses)
- It seems out of proportion to give each project the same amount of time; for example, some projects were given 20 minutes plus 10 minutes for questions, while some projects receive five to ten times more funding and are doing five to ten times more work.
- It is unclear how it is possible to review an $80 million project thoroughly in half an hour.

- There were some programs that were funded at $200,000, while others were as large as $40 million. However, both were allocated the same amount of time, i.e., 20 minutes for presentation plus 10 minutes for Q&A. This resulted in a thorough review of the smaller projects and a cursory one for the larger projects. Some of the activities in the larger projects were mere line items. If the intention of the AMR is to provide a check and a balance, the larger programs should be reviewed for one and a half hours at least.
- For very large programs, the time was inadequate; for small programs, it was sometimes more than adequate. The $100,000 projects and the $30 million projects get the same time. Some presenters avoided presenting any real data, making it hard to evaluate their work.
- It is apparent that the presentations generally are just the good news, and there is little mention of things that did not work out well. Many of the presentations were broad, with many players.
- It would be helpful to require clearer presentation of original goals and objectives of the project, to get a clearer understanding of what has not been met and what has changed.
- The standardized structure of the presentation helps with the review process; however, in some cases, diagrams were not properly explained or were difficult to understand.
- Not all of the evaluation criteria were addressed in all the presentations. (2 responses)
- Some of the presentations did provide adequate information, but most did not. It might be better if people spoke directly to the questions the reviewers has to answer. In some cases, people did this, but often they did not. Also, some presenters used up so much time on introductory material that the reviewers did not have time to hear about the important things that would tell them whether a project was good or on track, etc.
- The definition of “barriers” is interpreted differently by DOE, PIs, and reviewers. This reviewer suggests including a standardized definition or delete and replace with “objectives” of the project.
- With the exception of the Mississippi State project, the presentations were adequate.
- Powerpoint forces the presenters to select limited data and results.
- The presentations could have been a bit more specific. The time was too short with Q&A.
- The fuel cell session presentations contained too much information on each slide. It was difficult to determine the actual data and meaningful results. Provide presentation guidance, including font size. Also, few presentations provided error bars/analyses on their graphs.
- Reviewing the industry projects is always challenging, as they are reluctant to reveal sufficient technical detail to properly evaluate their progress towards the project goals.
- A meaningful review requires examining the 2011 presentation and/or report, reviewers’ comments, and the 2012 presentation. Unless you are an expert in the field, the information provided in the presentations is not sufficient.
- Time is simply too short to grasp anything but the highlights. Most things must be taken at face value. The current process is not fair to either the PIs or the reviewers. The previous ATD/BATT review system was far better and allowed time for real discussions. The current process may be more convenient, but it is simply too much at one time to work. It seems to be done mainly for show, ticking off someone’s box.
- The reviewers were given only the most superficial budget information on rather big programs; yet, they were still asked whether the budgets were sufficient or not. The reviewers should learn more about the specifics of how the money was spent or that question should be deleted.

### 3.4. The evaluation criteria upon which the review was organized (see below) were clearly defined.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| | Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|---|
| Relevance | 3 | 2 | 2 | 21 | 23 |
| | 6% | 4% | 4% | 41% | 45% |
| Approach | 2 | 1 | 2 | 26 | 21 |
| | 4% | 2% | 4% | 50% | 40% |
| Technical Accomplishments and Progress | 1 | 1 | 1 | 29 | 20 |
| | 2% | 2% | 2% | 56% | 38% |
| Technology Transfer and Collaboration | 1 | 3 | 10 | 21 | 17 |
| | 2% | 6% | 19% | 40% | 33% |
| Proposed Future Research | 2 | 2 | 4 | 29 | 15 |
| | 4% | 4% | 8% | 56% | 29% |

*14 Comment(s)*

- The evaluation criteria were not provided in advance.
- The evaluation criteria were clear and appropriate.
- The evaluation criteria have not changed greatly in the past few years, so most reviewers (including this reviewer) are generally clear on the criteria and the type of answers desired.
- The Relevance criterion is overrated. Either the work is relevant or it is not. If they are not doing work that is relevant, they should not be at the AMR. While the review focuses on prior accomplishments, the real question should be: is the project proposing proper future work? This gets only half the credit and is reduced to one or two bullet summary slides.
- All the projects should by definition be “relevant.” (Those that are not should not be funded.) Programs near termination really do not need to discuss “future work.” Many of the collaborations are more like customer-vendor relationships.
- The problem with the Proposed Future Research criterion is that some presenters see future work as new topics to be addressed after the project is completed; other presenters see it as work to be done from the current status to project completion.
- Relevance to DOE targets is not always easy to assess. It should be addressed in the AMR Plenary session.
- The Approach is extremely important because if the reviewer does not agree with the approach, everything else matters less. It was difficult to evaluate Technical Accomplishments and Progress without requiring the PI to also give a schedule of milestones. For example, spending 50% of the funds does not necessarily equate to meeting 50% of the objectives or 50% progress.
- The Collaboration criterion was explained well, but Technology Transfer was explained to a lesser extent. It was not completely clear how Proposed Future Research was defined.
- The Relevance question is just irrelevant and should be removed from the criteria. It is unclear why the research would get funded if it was not relevant. DOE knows why they funded the work. Regarding the collaboration criterion, some projects need collaboration much more than others. Moreover, collaboration is a means to an end, not an end in itself. Thus, this reviewer would not rank collaboration numerically. Future work should be ranked more highly since past work has already been paid for.
- Relevance and Approach are generally skimmed over by the presenters, as they feel that the Technical Accomplishments are the most important. The weights given to the Relevance and Approach should be significantly lower, since Relevance should be a given (the reviewer questioned if DOE would fund irrelevant projects) and Approach is often sort of obvious (experiments get results). Technology Transfer and Collaboration is not relevant for every project, and should not be presented on the same slide with the Budget (redundant).
- Some of these criteria were skewed for projects that were mainly aimed at educational and outreach missions.
- The criteria were pretty well explained, but it was hard to know how heavily the reviewers should weight collaboration at different levels. Some projects naturally involved collaboration; some did not. Some included

collaboration within the state or with certain types of institutions, but did not go further than that. It was unclear how much reviewers should penalize projects for this.

- Include “sufficient resources” criteria for evaluation in this list. In addition, provide more of an explanation on what resources to evaluate.

### 3.5. The evaluation criteria were adequately addressed in the presentations.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| | Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|---|
| Relevance | 1 | 3 | 3 | 30 | 14 |
| | 2% | 6% | 6% | 59% | 27% |
| Approach | 1 | 4 | 4 | 28 | 14 |
| | 2% | 8% | 8% | 55% | 27% |
| Technical Accomplishments and Progress | 0 | 1 | 8 | 22 | 19 |
| | 0% | 2% | 16% | 44% | 38% |
| Technology Transfer and Collaboration | 1 | 3 | 13 | 24 | 9 |
| | 2% | 6% | 26% | 48% | 18% |
| Proposed Future Research | 1 | 2 | 7 | 33 | 7 |
| | 2% | 4% | 14% | 66% | 14% |

*16 Comment(s)*

- Each presentation included each of these issues.
- For most presentations this was true; however, not for all. (3 responses)
- Achievements could have been better presented in several presentations.
- There was a lot less focus on collaborations and industry relevance.
- Proposed future research plans were not presented in much depth.
- Some presenters did not directly answer these questions (or at least emphasize their importance) or adequately address these in their presentations.
- In general, presenters addressed the criteria well, with some exceptions—some did not have enough time to devote to explaining all the criteria.
- There is something about the criteria that does not really work, but it is hard to define.
- For Technology Transfer and Collaboration, the presenters often resorted to name dropping to cater to this requirement. It is better to drop this item altogether in the future.
- Some presenters seem to hide the real data to an extent that makes it hard to judge the technical progress. Some presenters list supplier interactions as collaboration, others do not (but must have suppliers) and this is not always clear.
- Collaborations were highlighted and teams were well defined, but the transition aspects of the projects were not so well defined.
- Relevance related to the Recovery Act’s impact on jobs, activity, and growth were generally not addressed adequately in any of the Recovery Act projects reviewed. Proposed future research beyond the project was not addressed either.
- There was not enough information presented on Technology Transfer and Collaboration. Proposed Future Research was fine, but not relevant to the reviewer’s projects, which are ending.
- There was not enough information on Proposed Future Research. Also, there was not enough on what was learned versus results (due to lack of time). The slides on Collaboration were just a listing of who they are “teaming with,” which does not say much—anyone can have a list of associates (like friends in Facebook). A few PIs posted a one liner by each saying what the partner actually contributed, which was far more useful in evaluating collaborations.

### 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 percentage is the percent of the total respondents selecting the option.

| | Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|---|
| Relevance | 3 | 2 | 5 | 25 | 15 |
| | 6% | 4% | 10% | 50% | 30% |
| Approach | 1 | 2 | 4 | 28 | 15 |
| | 2% | 4% | 8% | 56% | 30% |
| Technical Accomplishments and Progress | 0 | 1 | 3 | 30 | 16 |
| | 0% | 2% | 6% | 60% | 32% |
| Technology Transfer and Collaboration | 2 | 1 | 5 | 29 | 13 |
| | 4% | 2% | 10% | 58% | 26% |
| Proposed Future Research | 2 | 2 | 5 | 28 | 13 |
| | 4% | 4% | 10% | 56% | 26% |

*8 Comment(s)*

- The criteria and weightings were OK.
- The criteria have not changed much, and have always been appropriate.
- Relevance should be 25%; Approach should be 40%; Technical Accomplishments and Progress should be 20%; Technology Transfer and Collaboration should be 10%; and Future Research should be 5%.
- They are all so inter-related, that it is hard to figure out the "right" weighting. Also, the levels are such that all projects are rated "very good" (which may be good for Congress), but DOE then cannot sort out any meaningful spread among project merits.
- For Technology Transfer and Collaboration, the presenters often resorted to name dropping to cater to this requirement. It is better to drop this item altogether in the future.
- Increase emphasis on the Technical Accomplishments and reduce weightings on Future Research and Approach.
- Omit Relevance and emphasize Proposed Future Research ("Future Plans"), and deemphasize Collaborations. Likely due to time constraints, there was not enough time devoted to explaining the rationale behind the approach.
- Relevance should be weighted less since it should be assumed that a project is relevant (otherwise it should not have been funded to begin with) and/or the reviewers should evaluate the relevance of a project as part of evaluating some of the other criteria.

### 3.7. During the AMR, reviewers had adequate access to the Principal Investigators.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 1 | 7 | 24 | 19 |
| 2% | 2% | 13% | 46% | 37% |

*6 Comment(s)*

- The discussion Q&A time and the breaks were sufficient for the reviewer to speak with the PIs.
- Many of the presentations were made by people other than the PIs. Quite often, the presenters did not have sufficient knowledge about the material that was being presented.
- There were no formal interactions outside the presentations, but there were good networking opportunities.
- It was not clear if the reviewers are allowed to access the PIs, besides at the time of the presentations.
- The reviewers had access, but because of the technical nature of the material, the reviewers needed more time to digest the information and then come back with questions. It is hard to do this in real time with no advanced review of the presentations. The posters allowed for more interaction, but it is hard to read the slides on a poster.

Doing the talk first and then the poster session, like the BATT program did, is the way to go to get a real review with reviewers contributing to discussions and developing ideas.

- These reviews cannot last forever, so the time has to be limited somehow. On the other hand, given how some presenters did, the reviewers did not always have enough time to get all the information needed to do a really good job of evaluating the projects. Perhaps there could be a dedicated, but informal, "networking" time for reviewers and presenters so there can be more discussion and exploration of topics that were not sufficiently covered.

### 3.8. Information on the location and timing of the projects was adequate and easy to find.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 0 | 3 | 1 | 30 | 18 |
| 0% | 6% | 2% | 58% | 35% |

*7 Comment(s)*

- The staff did a commendable job keeping the speakers on schedule.
- The information on the location and timing of the projects was easy to find, but not adequate. It was better when there were fewer projects reviewed at a time and there was much more detail provided.
- This reviewer wanted to be in two rooms at the same time.
- This reviewer had to move between hotels a lot. The elevator was busy all the time, so the reviewer had to use the emergency stairs.
- It would be better if all sessions were located in one hotel.
- The reviewer does not remembering seeing information on the location and timing of the projects, other than what states they were in.
- This reviewer was not sure what this questions means and questioned if this is referring to the time and place of the talks or of the work being reviewed. It was easy to find the talks; very good information provided. As far as the project timing, the standard slide worked well. While some of the PIs did not like the few "standard slides" they had to present, these were actually good to have and really essential to compare projects.

### 3.9. The number of projects I was expected to review was:

| | Number of Responses | Response Ratio |
|---|---|---|
| Too many | 2 | <1% |
| Too few | 9 | 4.0% |
| About right | 41 | 18.3% |
| No Response | 172 | 76.7% |
| **Total** | 224 | 100% |

*7 Comment(s)*

- The reviews were cursory.
- This reviewer only reviewed two projects, which was completely reasonable.
- It was unclear to the reviewer why they had to also review the DOE presenter.
- Overall, there were fewer projects to review this year; but it is not unreasonable to ask a reviewer to review five or six projects.
- All of this reviewer's 14 projects were bunched in the last one and a half days; this reviewer could have done a few earlier in the week as well.
- This reviewer had a light load of three projects, of which one ended due to a business decision. Attending all week could be hard to justify if there were not so many networking opportunities and opportunities to hear updates about other funded projects.

- This reviewer's work load (27 reviews) was really too high to do as good a job as they would have liked, especially since the reviewer is not a consultant and is squeezing this into a day job. The reviewer did like that they had a balance of posters and oral presentations and suggests relaxing the conflict of interest criteria to make it much less imposing so more reviewers can participate.

### 3.10. Altogether, the preparatory materials, presentations, and the question and answer period provided sufficient depth for a meaningful review.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 2 | 3 | 3 | 30 | 14 |
| 4% | 6% | 6% | 58% | 27% |

*7 Comment(s)*

- It is unreasonable to think that the reviewer can really understand how the larger projects accomplished their goals and are set on the right path for future work in 20 minutes. The reviewer would rather review half of the project every other year and give them twice as much time to present and answer questions.
- The fact that universities, national labs, and industries were being reviewed in a one-week-long review session does not lead to a meaningful review. It is better to split these three reviews so that knowledgeable people are assigned as reviewers, and so that there are enough experts in the audience as well.
- For the larger projects, the review felt quite rushed. However, given the large number of projects overall, it is hard to see how much more time could have been given.
- DOE announced that the meeting was not a technical meeting but a review. However, Q&A was almost related to the detailed technical issues, not to the evaluation criteria.
- It would have been nice to have more preparatory materials, at least within the context of the review. The reviewer only had access to the Powerpoint presentations.
- The whole review process is simply far too short and does not give the PIs enough time to explain and justify their work. The previous ATD/BATT reviews were vastly better at getting real evaluations. At the ATD/BATT reviews, the reviewers had genuine discussions and were in a far better position to fairly judge the work and also to offer advice on next steps. The current process is simply too rushed and superficial.
- The process seemed chaotic, especially for this first-time reviewer.

### 3.11. Please provide additional comments.

*15 Response(s)*

- The review was very well organized.
- This reviewer felt this was the best review process they have ever experienced.
- A very informative and helpful overview of the Programs' work overall. This was a very good use of time.
- There needs to be more private space for the reviewers. It was too packed.
- A minimum font size needs to be defined.
- It would help if the reviewers also had access to the summary project reports (text should not be more than 5–10 pages).
- The final scoring on the projects seems to have nothing to do with the final funding on each project. The PI should be accountable for poor scoring or performance.
- A good number of the slides were impossible to follow. Even sitting in the front row, the micro text is hard to discern. The most abusive of the presenters tended to put a micro text in one corner, and then continued adding additional boxes. The presenters need clear specifications on the font size. There also should be a clear message on each slide.
- DOE handled the AMR well. It is a model for other scientific, engineering, and technology programs. The one drawback was not having a level playing field for reviewers. The only non-DOE reviewers that paid for their travel expenses are the national laboratories; they should be doing this out of their overhead funds.

- The main problem with the meeting is that it is way too big. There are too many people to talk to and the noise level is almost unbearable at breaks and meals. Also, there are too many talks going on at once that the reviewer wanted to see.
- Many of the presenters would show a team effort and a variety of activities to mask the fact that they were not able to make sufficient progress in their effort. It would help if Relevance and Accomplishments are given higher weight in the review.
- The reviewer felt hounded by administrative details (especially only reviewing two projects) and is questioning getting involved in this review again. There was not enough information about the content provided to the reviewers, and the registration did not make sense. It is ridiculous to ask reviewers to go to two hotels for a meeting.
- The draconian language used in the agreement to view pdf files was plainly ridiculous and totally unacceptable. Furthermore, every presentation has a disclaimer that there is nothing confidential and they are posted on the Internet. It is unclear why any of this should have any confidentiality associated with it.
- This reviewer has reviewed for many years and has found that the AMR has progressively improved. The biggest issue is absorbing the large volume of material presented by DOE at the plenary session, sub-program overviews, and session introductions.
- It is not possible to have a perfect review system, but the reviewer wanted to provide the following feedback. This was the reviewer's first time doing this, so perhaps it gets easier/smoother after some practice. Reviewers should not be typing reviews into computers during the presentations since they cannot help but be distracted.

## 4. Responses from Presenters

### 4.1. The request to provide a presentation for the AMR was provided sufficiently prior to the deadline for submission.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 2 | 1 | 3 | 26 | 38 |
| 3% | 1% | 4% | 37% | 54% |

*7 Comment(s)*

- The presentation was requested too early. It is rather atypical to have presentations ready so far in advance. Also, the presenter would have liked to change a few things on their presentation after they have submitted it, but that did not seem possible.
- Given the early deadline and the flexibility in accepting presentations after the deadline, it all worked out fine.
- There was ample notice and reminders of what was needed and when.
- The request and due date were too early, because by the time the AMR occurred the presentations were two months outdated.
- The presentations were requested a little too early.
- There was more than enough time; however, the presenter questioned why it takes two months to burn these CDs.
- Although the deadline was missed due to internal approvals process, the organizers were flexible enough to accommodate the delay and were supportive through the process.

### 4.2. Instructions for preparing the presentation were sufficient.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 2 | 0 | 0 | 28 | 40 |
| 3% | 0% | 0% | 40% | 57% |

*4 Comment(s)*

- The instructions have evolved over time, and have gotten better. However, the supplied examples could be of higher quality (or be updated).
- They were way too detailed; the key information gets lost when there is too much of it. Most people do not have time to go through a 50-slide template for a 20-slide presentation!
- What we present based on the instructions is completely different than what the reviewers are interested in. The reviewers are there to learn something whether that is your intention or not. They do not want a list of highlights, nor your motivation, nor your impact; they want to know what science is behind the highlights.
- The instructions were very clear and good examples of the relevant scoring areas were provided.

### 4.3. The audio and visual equipment worked properly and were adequate.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|
| 1 | 2 | 3 | 25 | 37 |
| 1% | 3% | 4% | 37% | 54% |

*5 Comment(s)*

- The presenter had video and had to use a lapel mic to share audio.
- Sometimes a message would appear on the screen that covered part of the slide for an entire presentation.
- A remote slide-advance button (handheld) would be a fancy, but welcome, addition. Having laser pointers available (or loaned) has been a nice touch. Kudos for employing the speaker "countdown" clock effectively.
- The slide number was available to the reviewer but not to the presenter. During the Q&A, the presenter had difficulty navigating back and forth to the slides, was automatically kicked to next presentation, and could not get back to presentation slides.
- The equipment was fine, but it was extremely difficult to see the screen (especially from the back of the room), which the presenter noticed last year as well. This is due to a light that is directly above the screen that shines onto the screen. Because it is a canister light, you probably cannot turn off the one above the screen without turning them all off, but maybe you can put a piece of cardboard over the light directly over the screen.

### 4.4. The evaluation criteria upon which the Review was organized were clearly defined and used appropriately.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| | Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|---|
| Relevance | 1 | 1 | 8 | 38 | 16 |
| | 2% | 2% | 13% | 59% | 25% |
| Approach | 1 | 1 | 6 | 38 | 17 |
| | 2% | 2% | 10% | 60% | 27% |
| Technical Accomplishments and Progress | 2 | 1 | 5 | 35 | 20 |
| | 3% | 2% | 8% | 56% | 32% |
| Technology Transfer and Collaboration | 3 | 1 | 9 | 35 | 15 |
| | 5% | 2% | 14% | 56% | 24% |
| Proposed Future Research | 0 | 1 | 7 | 35 | 18 |
| | 0% | 2% | 11% | 57% | 30% |

*10 Comment(s)*

- It depends on how the reviewers interpret the instructions.
- Technology Transfer and Collaboration was not clearly defined.
- The presenter was aware of the criteria from previous year reviews. The reviewer comments were very helpful and constructive.
- The Relevance and Approach sections overlap somewhat, at least as defined by the examples given in the instructions. Tasks, Targets, Barriers, and Milestones can all cross these organizational boundaries; if DOE reviewers are flexible, then the intent is fine as given.
- The presenter is not aware of the criteria used by reviewers in any of these categories. It would be good for presenters to learn about the evaluation criteria as well as reviewers.
- The evaluation criteria were basically clear; however, the respondent is not sure if they were clear to the reviewers last year (based on the reviewer comments that the respondent looked at).
- There is no requirement for actual evidence on the Technical Accomplishments and Progress. Presenters can sell snake-oil in the AMR, i.e., claims that never materialize. Remove "Collaboration" from the evaluation criteria as this is not a requirement for the success of a project.
- There should be one slide for Relevance, zero for Approach, 15 for Accomplishments, zero for Technology Transfer, and one for Future Research. The Collaboration slide is often used to list the people that actually did the work and everyone in the organization is listed.
- Some of the advanced concept/research is important to the overarching strategy, but not directly related to established technical targets.
- Since the presenters have not received the results of the Review yet, it is unclear how they can intelligently comment on whether the criteria were "used appropriately."

### 4.5. Explanation of the questions within the criteria was clear and sufficient.

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| | Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|---|
| Relevance | 1 | 1 | 6 | 42 | 13 |
| | 2% | 2% | 10% | 67% | 21% |
| Approach | 1 | 0 | 8 | 38 | 15 |
| | 2% | 0% | 13% | 61% | 24% |
| Technical Accomplishments and Progress | 1 | 0 | 6 | 41 | 14 |
| | 2% | 0% | 10% | 66% | 23% |
| Technology Transfer and Collaboration | 4 | 1 | 5 | 36 | 14 |
| | 7% | 2% | 8% | 60% | 23% |
| Proposed Future Research | 0 | 1 | 7 | 39 | 14 |
| | 0% | 2% | 11% | 64% | 23% |

*5 Comment(s)*

- This presenter mostly relied on the examples for guidance.
- The reviewers often asked questions not specific to the project being reviewed.
- Technology Transfer and Collaboration were not clearly defined.
- This presenter did not understand the questions and asked what is meant by "criteria."
- Collaboration should mean different research groups, not who in your group did the work.

### 4.6. The right criteria and weightings were used to evaluate the project(s)/program(s).

The top number is the count of respondents selecting the option. The bottom percentage is the percent of the total respondents selecting the option.

| | Highly Disagree | Disagree | Neutral | Agree | Highly Agree |
|---|---|---|---|---|---|
| Relevance | 2 | 1 | 12 | 35 | 11 |
| | 3% | 2% | 20% | 57% | 18% |
| Approach | 2 | 1 | 9 | 37 | 11 |
| | 3% | 2% | 15% | 62% | 18% |
| Technical Accomplishments and Progress | 2 | 1 | 8 | 35 | 13 |
| | 3% | 2% | 14% | 59% | 22% |
| Technology Transfer and Collaboration | 4 | 2 | 12 | 34 | 8 |
| | 7% | 3% | 20% | 57% | 13% |
| Proposed Future Research | 1 | 1 | 10 | 38 | 10 |
| | 2% | 2% | 17% | 63% | 17% |

*5 Comment(s)*

- There needs to be evaluation criteria for the clarity of the presentations. Some were very poor, clearly not vetted, and did not do justice to the project.
- Technology Transfer and Collaboration were not clearly defined.
- The weighting should be as follows: Relevance, 15%; Approach, 5%; Technical Accomplishments and Progress, 75%; Technology Transfer and Collaboration, 5%; Proposed Future Research, 5%.
- Collaborations were overemphasized. In many cases, due to competing interests in technology development, collaborations are contrary to the interest of the developing organization.
- Too much attention was given to non-technical slides.

### 4.7. Please provide additional comments:

*12 Response(s)*

- The AMR was well organized and ran smoothly.
- Altogether, the AMR is a very worthwhile event.
- The respondent enjoyed hearing about the other projects, their challenges and successes.
- Given the number of participants at the AMR, it is amazing that everything worked out so smoothly. Congratulations to the organizing team.
- The hotel was very expensive. The next AMR could be held 20 miles outside Washington, D.C., where hotels would be much cheaper.
- The presentation material was requested more than two months in advance and updates just prior to the presentation date were not allowed without special permission. With projects that have data, updates should be permitted at least one week prior to the presentation.
- Washington, D.C., is a really expensive place to have this meeting. DOE is ultimately paying the travel and hotel for most participants (largely through projects), so saving the travel for DOE personnel based in D.C. does not save real money. Having the review in an area that needs stimulus (e.g., Flint, MI) would save money, create good press, etc.
- The AMR location is excellent and easy to get to. The hotel was fine and meeting rooms had plenty of attendee space. Also, a 30-minute time slot for oral was sufficient and allowed time for good discussion. The respondent also presented poster sessions and that worked out nicely; they were not as crowded as they sometimes get.
- Right now the AMR slides are submitted almost three months in advance of actual presentation date. The respondent questioned if it is possible to shorten the time between when the presentation slides are due and the AMR meeting; for example, perhaps the slides can be due 30 days before the actual AMR meeting date.
- The respondent is guessing that the easiest time to get a conference room in D.C. are the weeks where people would not want to travel: Mother's Day and Fourth of July, since every other organization would not consider having their employees travel those days. The reviewer suggests holding the AMR on another week.
- This review meeting has become unwieldy. Handling nearly 1,000 people in one hallway for breaks is not productive or refreshing. Perhaps the review should be broken into Energy Systems one week, and Vehicle Systems the next week.
- The benefit of long lead time in preparing briefs is somewhat offset by real-world events that occur between the time the briefing gets "locked down" and the time the presentation is made. The presenter suggests accepting revisions to the Progress section of the presentations a little closer to the presentation date since the research is being evaluated on that progress.