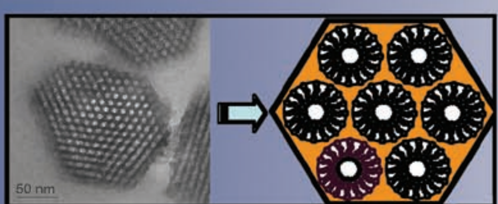
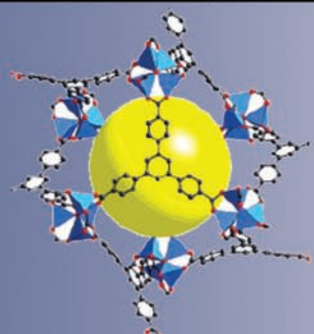
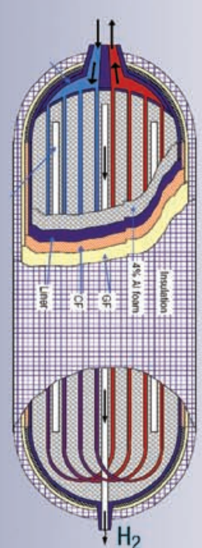
U.S. Department of Energy

DOE Hydrogen Program

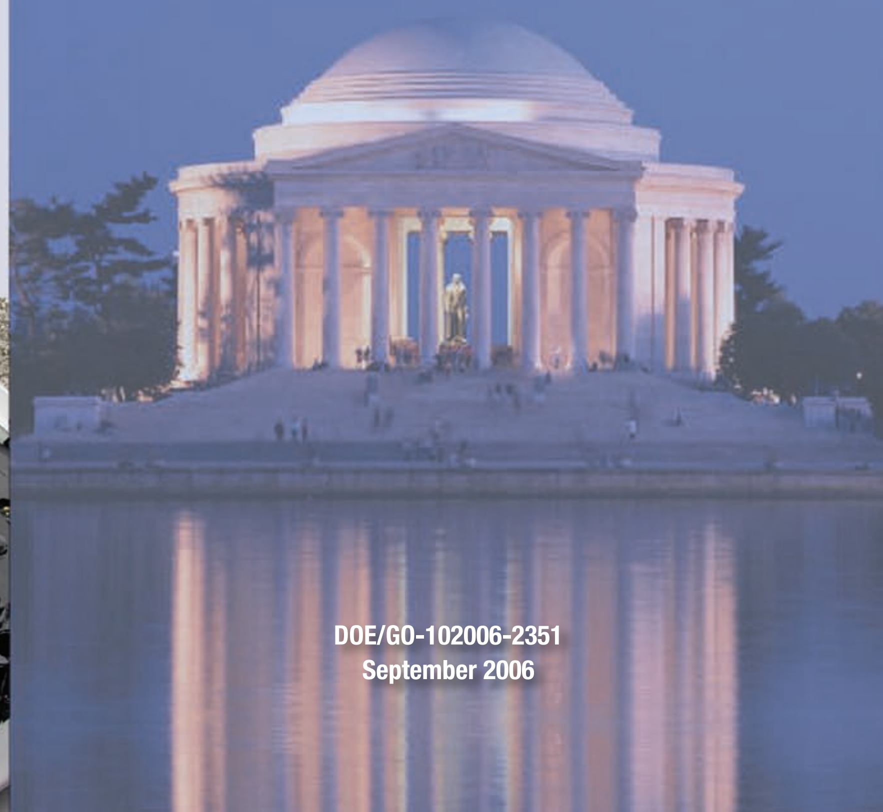






May 16–19, 2006
Arlington, Virginia

2006 Annual Merit Review and Peer Evaluation Report



DOE/GO-102006-2351
September 2006

Available electronically at http://www.osti.gov/bridge

Available for a processing fee to U.S. Department of Energy and its contractors, in paper, from:

U.S. Department of Energy
Office of Scientific and Technical Information
P.O. Box 62
Oak Ridge, TN 37831-0062
phone: 865.576.8401
fax: 865.576.5728
email: mailto:reports@adonis.osti.gov

Available for sale to the public, in paper, from:

U.S. Department of Commerce
National Technical Information Service
5285 Port Royal Road
Springfield, VA 22161
phone: 800.553.6847
fax: 703.605.6900
email: orders@ntis.fedworld.gov
online ordering: http://www.ntis.gov/ordering.htm

Printed on paper containing at least 50% wastepaper, including 20% postconsumer waste

PROLOGUE

Dear Colleague:

This document summarizes the comments provided by the Peer Review Panel at the U.S. Department of Energy Hydrogen Program FY 2006 Annual Merit Review and Peer Evaluation meeting, held on May 16-19, 2006 in the Washington, D.C. area. This was the second annual merit review of the research, development, demonstration, and analysis projects of the entire DOE Hydrogen Program. All four Offices that support the President's Hydrogen Fuel Initiative — Energy Efficiency and Renewable Energy (EERE), Fossil Energy (FE), Nuclear Energy (NE), and Science (SC) — are participants in the DOE Hydrogen Program and support this Review, in order to provide the hydrogen community an overall view of the breadth and depth of DOE's efforts under the Initiative. In addition to the overview presentations given by all four Offices during the opening plenary session, projects from EERE, FE, and NE were presented and peer reviewed, and the hydrogen storage-related projects from SC, awarded in May 2005, were provided as oral or poster presentations but not reviewed by the peer panel.

The recommendations of the Panel have been taken into consideration by DOE Technology Development Managers and Research Managers in the generation of future work plans. The table below lists the projects presented at the Review, evaluation scores, and the major actions to be taken during the upcoming fiscal year (October 1, 2006 to September 30, 2007). The projects have been grouped according to Program Element (production, delivery, storage, fuel cells, etc.), and the weighted scores are based on a 4-point scale involving five criteria. To furnish all principal investigators (PIs) with direct feedback, all evaluations and comments are provided to each presenter; however, the authors of the individual comments remain anonymous. The PI of each project is instructed to fully consider these summary evaluation comments, as appropriate, in their FY 2007 planning.

I would like to express my sincere appreciation to the members of the Peer Review Panel. It is they who make this report possible, and upon whose comments we rely to help make project funding decisions for the new fiscal year. Thank you for participating in the FY 2006 Annual Merit Review and Peer Evaluation meeting.

We look forward to your participation in the FY 2007 Annual Merit Review and Peer Evaluation which is presently scheduled for May 14-18, 2007 at the Marriott Crystal Gateway hotel in Arlington, VA.

JoAnn Milliken
Acting DOE Hydrogen Program Manager
Office of Energy Efficiency and Renewable Energy

MERIT REVIEW AND PEER EVALUATION SUMMARY TABLE

Hydrogen Production and Delivery:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PD-01	*Low-Cost Hydrogen Distributed Production Systems; H2Gen Inno. Inc.; Frank Lomax, Jr.*	3.4		X			Directly addresses Hydrogen Fuel Initiative goal of low cost hydrogen production from natural gas reforming as H2Gen aggressively pursues capital cost and operating cost reductions through improved hardware design and increased thermal efficiency. Delayed aqueous ethanol testing due to insufficient project funding. The technology is technically feasible and has a good chance of success, however need additional production data from hardware tests and to emphasize testing in multiple start - stop cycles to address real world durability issues. Also, more 3rd party work on PSA would assure process credibility.
PD-02	*Integrated Hydrogen Production, Purification & Compression System; BOC Group, Inc.; Satish Tamhankar*	3.4		X			Directly addresses Hydrogen Fuel Initiative goal of low cost hydrogen production from natural gas reforming as BOC approach to capital cost reduction employs several novel components integrating process steps. Integrating hydrogen production and separation eliminates need for water gas shift reactor; hydride compressor eliminates need for costly and high maintenance mechanical compression.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PD-03	*Integrated Short Contact Time Hydrogen Generator; GE Global Res.; Ke Liu*	3.3		X			Directly addresses Hydrogen Fuel Initiative goal of low cost hydrogen production from natural gas reforming by combining contact partial oxidation and steam methane reforming. GE Global Research made significant progress in catalyst screening and selection, process and materials modeling, heat exchanger selection, PSA selection, and sulfur measurements. Excellent collaboration in that all team members are making important and unique contributions. Need to address CPO catalyst manufacturing.
PD-04	*Hydrogen Generation from Biomass-Derived Carbohydrates via the Aqueous-Phase Reforming (APR) Process; Virent Energy Sys.; Randy Cortright*	3.4		X			This project is making sound progress despite very limited funding due to budget constraints. It is focused on the important objective of distributed hydrogen production while utilizing renewable resources with near-zero net greenhouse gas evolution. Focus will be directed towards the use of lowest cost available sugars from lignocellulosic biomass, and improved yields of hydrogen. Good partnership with ADM and the University of Wisconsin. Expanding collaborations should be considered.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PD-05	*Distributed Bio-Oil Reforming; NREL; Bob Evans*	3.5		X			Directly addresses Hydrogen Fuel Initiative goal of producing hydrogen from bio-derived liquids at the point of use (distributed). Good progress in volatilization (obtaining low residues of 6%) and oxidative cracking. Needs work on process economics to identify critically important process variables that will make or break commercial viability. Needs greater emphasis on catalyst development and dealing with undesirable byproducts.
PD-06	*Photoelectrochemical Hydrogen Production; Univ. of Nevada; Bob Perret*	3.3		X			This project is well organized with participation from labs, industry, and universities. This collaboration has and will continue to produce significant developments for this long term technology.
PD-07	*Renewable Electrolysis Integrated System Development and Testing; NREL; Ben Kroposki*	3.0		X			The collaborative approach (including research, analysis, industry, etc.) used by this project is effective in facilitating progress toward commercialization of renewable electrolysis. The focus of the project needs to be sharpened to clearly support commercialization of renewable electrolysis.
PD-08	*Advanced Alkaline Electrolysis; GE Global Res.; Richard Bourgeois*	3.4		X			This project is moving toward an economical, near-term product - further testing is needed for the scaled-up system.
PD-09	*Alkaline, High Pressure Electrolysis; Teledyne; Samir Ibrahim*	2.3		X			The scope of this project will be re-evaluated with the understanding that higher pressure operation is not feasible.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PD-10	*Development of Solar-powered Thermochemical Production of Hydrogen from Water; U of Colorado; Alan Weimer*	3.0		X			This is a large collaborative project well aligned with the goal of hydrogen production with near-zero GHG and other emissions. Good progress is being made although there are clearly significant challenges to achieve the hydrogen cost target. This is a longer term approach for hydrogen production. Efforts should be focused on the cycles selected for R&D, including materials issues, and further down-selection should be made in a timely manner.
PD-11	*Hydrogen Delivery Infrastructure Options Analysis; Nexant Inc.; Bruce Kelly*	3.1		X			This analysis project is vital to properly focus the Delivery Program element R&D. Good progress has been made. The effort needs to focus on hydrogen delivery and eliminate peripheral issues such as distributed reforming which is handled in other analysis projects. Highly qualified organizations are involved and they need to more fully collaborate.
PD-12	*Scale-Up of Microporous Inorganic Hydrogen-Separation Membranes; ORNL; Rod Judkins*	3.0		X			Supports Hydrogen Fuel Initiative in providing a way to recover H_2 from a coal gasification stream as well as pressurized CO_2 for capture and sequestration. Good technology design approaches in constructing the membrane tube, particularly in the fabrication of the microporous separating layer. Not a one-step process for high-purity hydrogen. R&D should focus on improving flux, purity, and selectivity.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PD-13	*Scale-up of Hydrogen Transport Membranes for IGCC and FutureGen Plants; Eltron Research Inc.; Paul Grimmer*	3.0		X			Supports Hydrogen Fuel Initiative by developing a critical element needed for hydrogen purification. Insufficient details are provided to justify the PSA cost reported. Comprehensive study to evaluate interferrants needed to determine their linkage to the membrane degradation. Further, one test is not a durability study. Need to know performance scatter between multiple membranes and if there are mechanical durability issues that may result from time on test or the interferrant in the gas stream. Need to understand why the flux degrades by a factor of 3 over the 11 months test.
PD-14	*Cost-Effective Method for Producing Self-Supporting Pd Alloy Membrane for Use in the Efficient Production of Coal-Derived Hydrogen; SwRI; James Arps*	3.3		X			Supports Hydrogen Fuel Initiative by developing a critical element needed for hydrogen purification. Good use of partner expertise. Focus on sputter deposition methods with careful attention to experimental variables to achieve goals. Need to test membranes in H2S-containing gas to ensure that thin PdCu films behave like the thick versions in terms of S effects on flux and selectivity.
PD-15	*Sulfur-Iodine Thermochemical Cycle; SNL/INL/GA; Paul Pickard*	3.3		X			Directly supports the goals of the Hydrogen Initiative and the Nuclear Hydrogen Program R&D Plan. A lot of progress has been made on this challenging effort with excellent international collaboration. Key issues are well identified and being worked on. Approach and need for catalyst development for the sulfuric acid decomposition step needs to be looked at carefully.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PD-16	*Evaluation of Calcium-Bromine Thermochemical Cycle; ANL; Richard Doctor*	3.1		X			This project will receive some funding in FY07 in order to obtain some additional data. It will then be re-evaluated along with the other non-sulfur based thermochemical cycles for possible future efforts while the NHI program focuses on the more promising sulfur based cycles. Good progress has been on two novel approaches to overcome the basic barriers in this Ca-Br thermochemical cycle. The critical issues for these new approaches (plasma energy use and PEM electrolysis) have been identified and require further research for them to succeed.
PD-17	*Laboratory-Scale High-Temperature Electrolysis System; INL/ANL/Ceramatec; Steve Herring*	3.3		X			Directly addresses long-term future hydrogen generation issues of the Hydrogen Fuel Initiative. Good systematic approach with clear vision of 5MW engineering demonstration by 2015, however additional detail on pilot scale and engineering scale needed. Good overall plan for bench and integrated lab scale programs. Explanation of 25% degradation needed prior to scale up. Good project collaboration. More collaboration with other SOFC technologists and SECA needed.
PD-18	*Nuclear Reactor/Hydrogen Process Interface; INL; Steve Sherman*	3.1		X			This cross-cutting and process interface project is vital to the success of the NHI. Good approach and integration between tasks. Efforts to coordinate this project with the other NHI projects will be improved particularly in the area of materials research.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PDP-02	*Low-Cost, High-Pressure Hydrogen Generator; Giner Electrochemical; Cecelia Cropley*	3.0		X			This project directly addresses DOE stated goals of increasing efficiency and decreasing capital cost through advanced membrane and reduced part counts. The project needs to further explore the trade offs of high-pressure operation vs. system simplification, performance and costs and also firm up demonstration plans.
PDP-06	*Adapting Planar Solid Oxide Fuel Cells for Distributed Power; Ohio University; David Bayless*	2.3				X	This project does not directly support the President's Hydrogen Fuel Initiative. It does support the more efficient use of coal for electricity production, however, by investigating H_2S contamination issues.
PDP-08	*Zeolite Membrane Reactor for Water-Gas-Shift Reaction for Hydrogen Production; U. of Cincinnati; Jerry Y.S. Lin*	3.6		X			Directly addresses Hydrogen Fuel Initiative goal of hydrogen production from natural gas and bio-derived liquids through a strong combination of relevant experimental and modeling capabilities at four universities. Need to show that the membrane reactor system is more cost effective than a multistage reactor system, and the impact of the membrane reactor on overall system cost as a function of transmembrane flux and selectivity and membrane reactor costs. Modeling should include the cost of adding heat transfer functionality to the membrane water gas shift reactor (heat is usually removed between stages of Water gas shift reaction). This analysis would help the researchers set specific flux, selectivity, and cost targets that would define project success.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PDP-10	*Startech Hydrogen Production; Startech Environmental; David Lynch*	2.9				X	Project is a congressionally-directed project from a prior year and is expected to conclude this year.
PDP-12	*Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen; Midwest Optoelectronics; Liwei Xu*	3.1		X			The design and fabrication of PEC cells for production of hydrogen is directly in line with the President's Hydrogen Fuel Initiative. This project has made progress, but low levels of funding has retarded the work and will require the project schedule to be extended. More well defined durability targets and a basis for cost needs will help this project.
PDP-13	*Development of Water Splitting Catalysts Using a Novel Molecular Evolution Approach; Arizona State U.; Neal Woodbury*	3.0		X			This research project supports the long term objectives of DOE. The project is in an early stage of determining equipment design and set up with technical accomplishments forthcoming in the coming years.
PDP-15	*Fundamentals of a Solar-thermal Mn_2O_3/MnO Thermochemical Cycle to Split Water; U of Colorado; Alan Weimer*	3.2		X			Good progress is being made with the limited funding that has been available. The challenges have been well defined and focus has been appropriately put on the key issues. A more detailed plan for the remainder of the project would be very helpful.
PDP-16	*Hydrogen Embrittlement of Pipeline Steels: Causes & Remediation; U. of Illinois; Petros Sofronis*	3.1		X			A systematic approach, both computational and experimental, to studying the fundamentals of hydrogen embrittlement. A very important project to the cost effective and safe transport of hydrogen in pipelines. Very good progress has been despite reduced funding availability. Good collaborations with other researchers in this field.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
PDP-17	*Hydrogen Regional Infrastructure Program in Pennsylvania; Concurrent Tech. Corp; Eileen Schmura*	2.3		X			This project is conducting an in-depth look at hydrogen delivery options on a regional basis. As a congressionally directed project.
PDP-18	*Developing Improved Materials to Support the Hydrogen Economy; Edison Materials Tech Center; Michael Martin*	2.7		X			This project funds competitively awarded projects addressing DOE identified barriers. As a congressionally directed project.
PDP-31	*EVermont Renewable Hydrogen Fueling System; Northern Power Sys.; Tom Maloney*	2.9		X			This project will continue in FY2007 and has been funded.
PDP-33	*A Reversible Planar Solid Oxide Fuel-Fed Electrolysis Cell and Solid Oxide Fuel Cell; Materials and Systems Research; Greg Tao*	3.0		X			This project is not a truly reversible system as the fuel cell and electrolyzer are separate with thermal integration. The project needs to provide efficiency calculations and an economic analysis.
PDP-34	*High Performance Flexible Reversible Solid Oxide Fuel Cell; GE HPGS; Nguyen Minh*	2.9		X			This project is determining and optimizing the best trade-offs in device performance for both modes of operation and has a good mix of modeling and experimental work. Good progress.
PDP-36	*Solid Oxide Fuel Cell Carbon Sequestration; Nisource Energy Tech.; Norm Bessette*	2.3		X			This congressionally-directed project will continue in FY2007, operating on funds provided in FY2006.

Hydrogen Storage:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
ST-01	*LANL work as part of the Chemical Hydrogen COE; LANL; Bill Tumas*	3.5		X			Focus on regeneration, develop go/no-go criteria and down select process, consider additional promising storage materials, and increase coordination on catalyst development for ammonia borane work.
ST-02	*PNNL work as part of the Chemical Hydrogen COE; PNNL; Chris Aardahl*	3.4		X			Focus on regeneration, investigate/evaluate on-board solid handling and thermal management issues for ammonia borane.
ST-03	*Amineborane Hydrogen Storage: New Methods for Promoting Amineborane Dehydrogenation/Regeneration Reactions; U. of Pennsylvania; Larry Sneddon*	3.6		X			Increase focus on regeneration, identify and pursue promising regeneration route while optimizing hydrogen storage and release parameters for the identified material.
ST-04	*Solutions for Chemical Hydrogen Storage: Hydrogenation/ Dehydrogenation of B-N Bonds; U. of Washington; Michael Heinekey*	2.7		X			Address iridium cost issue, and work with CoE partners towards coordination of catalyst work and regeneration of ammonia borane.
ST-05	*Chemical Hydrogen Storage Using Polyhedral Borane Anion Salts; UCLA; Fred Hawthorne*	2.8		X			Address precious metal catalyst cost issue, improve kinetics and capacity, and investigate/develop regeneration requirements.
ST-06	*Novel Approaches to Hydrogen Storage: Conversion of Borates to Boron Hydrides; Rohm and Haas; Suzanne Linehan*	3.1		X			Identify promising routes, demonstrate selected process in the lab and compile data needed for go/no-go decision.
ST-07	*Development of Advanced Chemical Hydrogen Storage and Generation System; Millennium Cell; Ying Wu*	2.9		X			Ensure that modeling work is general and applicable to a wide variety of chemical hydrides.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
ST-08	*Combinatorial Synthesis and High Throughput Screening of Effective Catalysts for Chemical Hydrides ALSO COVERS MH WORK; Intematix; Xiao-Dong Xiang*	3.0		X			Increase collaboration with additional CoE partners where appropriate for increased benefit with this approach within the CoE.
ST-09	*Hydrogen Storage by Reversible Hydrogenation of Liquid-phase Hydrogen Carriers Air Products & Chemicals, Inc.; Alan Cooper*	3.4		X			Pursue options for higher storage capacity materials, address catalyst cost issue and investigate potential hydrogenation issue.
ST-10	*Complex Hydride Compounds with Enhanced Hydrogen Storage Capacity; United Technologies Research Center; Susanne Opalka*	2.9		X			The project is entering its final year. Continue the modeling and experimental approach to emphasize on-board reversibility. Reducing the discharge reaction exotherm is top priority.
ST-11	*Discovery of Novel Complex Metal Hydrides for Hydrogen Storage through Molecular Modeling and Combinatorial Methods; UOP LLC; Greg Lewis*	3.0		X			The project is entering its final year. Leverage high throughput synthesis/testing capability on new materials such as borohydrides. Incorporate a metric of "usable capacity" that takes into account temperature for down-selecting materials.
ST-12	*Hydrogen Fuel Cells and Storage Technology Project at UNLV; UNLV; Clemens Heske*	1.4	X	X			This congressionally directed cross-cutting project (hydrogen storage, fuel cells and ICE combustion) will continue in FY2007, operating on funds provided in FY2006. Work scope is too broad and repeats prior work; redirect to be more in line with the program's goals. Consider outside partners for required expertise.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
ST-13	*Overview of Metal Hydride Center of Excellence and Sandia's Research; Sandia National Laboratory; Lennie Klebanoff*	3.1		X			Generate more "outside-the-box" concepts using a systematic modeling/experimental approach as opposed to focusing on further development of known systems. Incorporate a metric of "usable capacity" that takes into account temperature for down-selecting materials. Increase emphasis on CoE collaborations.
ST-14	*Lightweight Intermetallics for Hydrogen Storage; General Electric; J.C. Zhao*	3.1		X			The emphasis solely on Mg borohydride may be limiting. Continue to emphasize collaborations among the CoE partners. Incorporate a metric of "usable capacity" that takes into account temperature for down-selecting materials.
ST-15	*Synthesis of Aluminum Hydrides for Automotive Applications; BNL; Jason Graetz*	3.1		X			Off-board regeneration of alane is the key technical issue and should receive priority. Collaborations with the Chemical Hydrogen CoE may be helpful in this regard.
ST-16	*Thermodynamically Tuned Nanophase Materials for Reversible Hydrogen Storage; HRL Laboratories; Greg Olson*	3.1		X			A key issue with destabilized hydrides is kinetics. Emphasize studies with scaffolds. Incorporate a metric of "usable capacity" that takes into account temperature for down-selecting materials.
ST-17	*Development and Evaluation of Advanced Hydride Systems for Reversible Hydrogen Storage; Jet Propulsion Laboratory; Bob Bowman*	3.1		X			The NMR analyses are important for the CoE partners developing materials. Increase emphasis on engineering science and analysis in FY07.
ST-18	*Metal Hydride-Based Hydrogen Storage; U. of Illinois; Ian Robertson*	3.1		X			For both the material characterization and theoretical modeling activities, continue to expand communications with the material developers of the CoE.
ST-19	*Hydrogen Storage Systems Analysis; ANL; Rajesh Ahluwalia*	3.4		X			Consider addition of sensitivity analysis tab(s) to tools. Continue to increase coordination with TIAX on storage analyses.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
ST-20	*Analyses of Hydrogen Storage Materials and On-Board Systems; TIAX; Steve Lasher*	3.4		X			Continue focus on cost analysis to minimize overlap with ANL. Continue to increase coordination with ANL on storage analyses.
ST-21	*Carbide-Derived Carbons with Turnable Porosity Optimized for Hydrogen Storage; U of Penna/ Drexel Univ.; Jack Fischer*	2.6		X			Place emphasis on enabling room temperature adsorption and increasing volumetric capacity. Clarify benefits of CDCs over activated carbon.
ST-22	*New concepts for optimized hydrogen storage in metal-organic frameworks (MOFs); UCLA/University of Michigan; Omar Yaghi*	3.0		X			Focus material development to increase both gravimetric and volumetric capacity. Also explore methods to increase hydrogen binding energy.
ST-23	*Carbon Center of Excellence and NREL's Research; NREL; Mike Heben*	3.2		X			Pay equal attention to both gravimetric and volumetric capacity. Verify the modeling predictions with synthesis and material testing. Continue to stress storage approaching room temperature. Down selections and go/no-go decisions should continue and the project should be flexible to move on when required.
ST-24	*Enabling Discovery of Materials With A Higher Heat of H_2 Adsorption; Air Products & Chemicals; Alan Cooper*	3.0		X			Accelerate move to new boron and nitrogen-containing materials building on recent work stressing both volumetric and gravimetric capacity. Intensify partnerships for the material testing capability. Either in-house or through collaborations, test theory predictions with synthesis and testing.
ST-25	*Neutron Scattering Characterization at NIST in support of the Carbon and Metal Hydride Centers of Excellence; NIST; Dan Neumann*	3.5		X			Continue to support and speed the material selection process for both CoEs. Communication with the material developers is critical. Justify the Calphad modeling in the context of other theoretical work being done within the metal hydride CoE.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
ST-26	*Cloning Single Wall Carbon Nanotubes for Hydrogen Storage; Rice U.; James Tour*	3.3		X			Address volumetric capacity as well as hydrogen binding energy and uptake/discharge kinetics.
ST-27	*Advanced Boron and Metal loaded High Porosity Carbons; Penn State U.; Peter Eklund*	3.1		X			Material testing and modeling should include understanding of hydrogen uptake/discharge kinetics as well as material volumetric capacity.
ST-28	*Hydrogen Storage in Graphite Nanofibers and the Spillover Mechanism; U. of Michigan; Ralph Yang*	3.2		X			Independent verification of promising materials is required. Stress understanding and improving the hydrogen uptake/discharge kinetics.
STP-02	*Effects and Mechanisms of Mechanical Activation on Hydrogen Sorption/ Desorption of Nanoscale Lithium Nitrides; U. of Connecticut; Leon Shaw*	2.8		X			Re-align focus on understanding and optimizing the mechanical activation effects (e.g. low temperature). Include limited cycling on reversible capacities. Increase collaborations to include new materials such as borohydrides.
STP-03	*First-Principles Modeling of Hydrogen Storage in Metal Hydride Systems; U. of Pittsburgh; Karl Johnson*	3.1		X			Increase collaborations to integrate theoretical activities with experimental synthesis/testing. Communication across all the theoretical activities within the CoE is also critical.
STP-04	*Hydrogen Storage Research in support of the DOE National Hydrogen Storage Project; Savannah River National Lab; Ted Motyka*	2.8		X			This review covered all storage work occurring at SRNL. Focus on SRNL's unique capabilities to develop materials and subsystems. Quantify the alane regeneration efficiency. Increase coordination with BNL. The microsphere work has limited potential.
STP-05	*Thermodynamically Tuned Nanophase Materials for Reversible Hydrogen Storage: Structure & Kinetics of Nanoparticle and Model System Materials; Stanford U.; Bruce Clemens*	2.9		X			Clarify planned synchrotron studies in FY07. After go/no-go decisions with MgSi, move to more promising materials for in situ studies.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
STP-06	*Fundamental Studies of Advanced High-Capacity Reversible Metal Hydrides; U. of Hawaii; Craig Jensen*	2.9		X			Deemphasize Ti catalyst/alanate work and increase emphasis on catalysis for the destabilized hydrides. Transfer alanate understanding to borohydrides, as applicable. The PI's collaborations are noteworthy.
STP-07	*High Throughput Combinatorial Chemistry Development of Complex Hydrides; Intermatix; Guanghui Zhu*	2.5		X			Increase collaborations with MHCoE partners involved in materials discovery to address the most promising materials. Coordinate theory portion with the existing MHCoE theory groups to reduce overlap.
STP-08	*Synthesis of Nanophase Materials for Thermodynamically Tuned Reversible Hydrogen Storage; California Institute of Tech; Channing Ahn*	2.8		X			Address the fate of the nanoparticles under cycling conditions. Move to materials other than the MgSi.
STP-09	*Effect of Trace Elements on Long-Term Cycling and Aging Properties of Complex Hydrides for Hydrogen Storage; U. of Nevada, Reno; Dhanesh Chandra*	3.0		X			Re-align work to address the most promising materials. Boron based materials are growing in importance. Top priority should be the impurities/cycling work over the characterization work.
STP-10	*Chemical Vapor Synthesis of Nanocrystalline binary and complex Metal Hydrides for Reversible Hydrogen Storage; U. of Utah; Zak Fang*	3.0		X			Re-align work to address the most promising materials. Move away from alanate/amide systems. Address the cyclic stability of nanocrystalline materials.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
STP-12	*ORNL's Hydrogen Storage Research in support of the DOE National Hydrogen Storage Project; Oak Ridge National Lab; Gilbert Brown*	2.7		X			Re-align work on materials that have little potential to meet storage system requirements. Carbon work: Increase collaboration with modeling efforts to estimate feasibility of enhancing hydrogen physisorption. Estimate bulk volumetric capacity. MH Work: Increase collaboration with MHCoE synthesis partners. Emphasize reversibility in new materials discovered.
STP-15	*Conducting Polymer as New Materials for Hydrogen Storage; U. of Pennsylvania; Alan MacDiarmid*	2.9		X			Should it be determined that conducting polymers are not suitable for hydrogen storage, this project should be redirected into a more promising related area of hydrogen storage materials research.
STP-16	*Enhanced Hydrogen Dipole Physisorption; California Institute of Tech; Channing Ahn*	2.7		X			Reiterate the strategy to achieve the optimum pore size quoted in the carbon-based materials. Establish a backup route to achieve pores with optimal surface area to mass ratio. Include volumetric capacity in optimization strategy.
STP-17	*Optimization of SWNT Production and Theoretical Models of H_2-SWNT Systems for Hydrogen Storage; Rice U.; Boris Yakobson*	2.9		X			Address the route to room temperature materials adsorption. Focus on new structures/ geometries that maximize storage potential (gravimetric and volumetric). Continue SWNT production improvements at a low effort level until scale-up is justified.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
STP-18	*Development of Carbon-Based Materials and Characterization of Hydrogen Adsorption by NMR; U. of North Carolina; Yue Wu*	3.2		X			Include a systematic study of the sources of error in the measurement technique and validate measurements before proceeding further to more challenging materials. Emphasize developing a low temperature capability (77K). Clarify which hydrogen storage materials this technique would be suitable. Collaborations are critical.
STP-19	*Growth of Uniform Carbon Nanotubes using Molecular Clusters as Catalysts; Duke U.; Jie Liu*	2.7		X			Stress increasing both gravimetric and volumetric capacity towards room temperature; include hydrogen binding energy and uptake/discharge kinetics with a minimal amount of metal loading. Increase collaborations for material testing.
STP-21	*Carbon-Based Hydrogen Storage; LLNL; Joe Satcher*	2.9		X			Measure relative benefit of a specific dopant in a carbon aerogel host as compared to other carbon hosts. A comparison should be made of room temperature vs. 77K uptake both in their pristine and doped forms. Design of the aerogels should also take into account volumetric capacity.
STP-25	*Electrochemical Hydrogen Storage Systems; Penn State U.; Digby Macdonald*	3.3		X			The electrochemical approach for B-O to B-H reduction through intermediates appears to hold some promise and should be continued along with the direct reduction pathway. Develop a plan that maximizes the information developed in this project that can be applied to the go/no-go decision process for this storage option.
STP-26	*Chemical Hydrogen Storage Using Ultra-High Surface Area Main Group Materials; UC Davis; Philip Power*	2.5		X			Conduct preliminary assessment of hydrogen up-take and release for synthesized materials and focus on the few promising candidates to optimize their storage capacities.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
STP-27	*Main Group Element Chemistry in Service of Hydrogen Storage and Activation; U. of Alabama; Anthony Arduengo*	3.6		X			Continue to pursue and develop higher capacity storage materials with increasing focus on regeneration work.
STP-37	*Clean Energy Research Project: Advanced Metal Hydrides; Univ. of South Carolina; Jim Ritter*	2.6		X			This project is a cross-cutting project covering hydrogen storage and fuel cell technology. The review addressed mainly the storage activities. Continue to stress materials that will meet volumetric and gravimetric capacity as well as transient performance targets.
STP-43	*Hydrogen Research at Univ. of South Florida; Univ. of South Florida; Lee Stefanakos*	2.5		X			This project is a cross-cutting project. Continue to eliminate weak tasks, focus on promising storage materials and conduct testing on materials to determine and verify performance.

Fuel Cells:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FC-01	*High-Temperature Polymer Electrolyte Membranes; ANL; Debbie Myers*	2.5				X	Project successfully completed in FY06. Continuation subject to results of lab call. Measure conductivity of membrane at low RH and >100°C.
FC-02	*Development of Polybenzimidazole-based, High Temperature Membrane and Electrode Assemblies for Stationary and Automotive Applications; Plug Power; Rhonda Staudt*	2.6		X			In FY07, Plug Power will continue long term acid trap testing and demonstrate sealing concept.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FC-03	*Non-Nafion Membrane Electrode Assemblies; LANL; Yu Seung Kim*	3.2				X	Project successfully completed in FY06. Continuation subject to results of lab call. Focus and extend OCV and start/stop tests to interface-optimized MEAs using Nafion in electrodes and BPSH, 6F, 6FCN membranes.
FC-04	*Advanced Fuel Cell Membranes Based on Heteropolyacids; NREL; John Turner*	3.0				X	Project successfully completed in FY06. Continuation subject to results of lab call. Focus on understanding the binding mechanism of HPAs and the conductivity mechanism in the membrane.
FC-05	*Enabling Commercial PEM Fuel Cells with Breakthrough Lifetime Improvements; Dupont; Gonzalo Escobedo*	3.3		X			Understanding peroxide and mechanical-strength failure mechanisms is highly relevant to DOE objectives and durability goal. Continue to use University of Southern Mississippi analytical and post-mortem work to provide valuable insight into membrane degradation mechanisms. UTC mitigation technique appears successful. Reviewers would like to see more cost analysis primarily related to the impact of different mitigation techniques.
FC-06	*Development of a Low-Cost, Durable Membrane and MEA for Stationary and Mobile Fuel Cell Applications; Arkema Chemicals; Michel Foure*	3.0		X			The PVDF platform is an excellent scaffold for new ionomers that need more structural integrity. The high throughput casting method will help in developing a large scale manufacturing process. Morphology work is a good addition. Continue work on M40 material but future work needs to address drive cycle conditions, low RH, and high temperature.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FC-07	*Hydrocarbon Membrane; SNL; Christopher Cornelius*	2.7				X	Project successfully completed in FY06. Continuation subject to results of lab call. Consider CV to help elucidate structure-property relationships, helox experiments need to be run to investigate mass-transport phenomena, and AFM work to gain understanding of morphology.
FC-08	*MEA and Stack Durability for PEM Fuel Cells; 3M; Mike Hicks*	3.6		X			In FY07, continue MEA and stack development and testing; continue MEA degradation studies and statistical lifetime predictions.
FC-09	*Low Pt Loading Fuel Cell Electrocatalysts; BNL; Radoslav Adzic*	3.5				X	Project successfully completed in FY06. Continuation subject to results of lab call. Continue outstanding work with gold nanoclusters; test catalyst at higher temperature and include cycling at higher potentials.
FC-10	*New Electrocatalysts for Fuel Cells; LBNL; Phil Ross*	3.5				X	Project successfully completed in FY06. PI has retired.
FC-11	*Development of transition metal/chalcogen based cathode catalysts for PEM fuel cells; Ballard; Stephen Campbell*	2.4		X			In FY07, additional non-precious-component ternary catalysts will be studied.
FC-12	*Novel Approach to Non-Precious Metal Catalysts; 3M; Radoslav Atanasoski*	3.2		X			In FY07, focus on resolving stability and activity issues.
FC-13	*Novel Non-Precious Metals for PEMFC: Catalyst Selection through Molecular Modeling and Durability Studies; U of So. Carolina; Branko N. Popov*	3.3		X			In FY07, focus on durability and MEA optimization.
FC-14	*Non-Precious Metal Catalysts; LANL; Piotr Zelenay*	3.0				X	Project successfully completed in FY06. Continuation subject to results of lab call. Focus on Co and other transition metals, less on Ru.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FC-15	*Low-Platinum Catalysts for Oxygen Reduction at PEMFC Cathodes; NRL; Karen Swider-Lyons*	2.8				X	Project successfully completed in FY06. Don't spend too much time on gold unless very large activity increases are obtained.
FC-16	*Development of High-Performance, Low-Pt Cathodes Containing New Catalysts and Layer Structures; Superior MicroPowders; Paolina Atanassova*	3.3				X	Use of combinatorial screening, expertise in spray pyrolysis, and the scalability of the catalyst fabrication process offer real advantages to mass manufacturers. Compared with other screening techniques, the combinatorial method assures that the candidates initially tested for catalytic activity are made by the same process as those intended to be used in mass production of the final "commercial" catalyst. Achieving stable 2-3 nm particle size is excellent however more durability data is needed on the various compositions. This project will be completed in the fall of 2006 with FY06 funding.
FC-17	*Cathode Electrocatalysis: Platinum Stability and Non-Platinum Catalysts; ANL; Debbie Myers*	3.1		X			Include studies with different wt% of Pt on C (e.g., 50 wt %); characterize the changes in the carbon support that occur; studying stability of cathode catalysts while cycling the anode between H_2 and O_2.
FC-18	*Integrated Manufacturing for Advanced MEAs; E-TEK; Yu-Min Tsou*	3.1				X	Project successfully completed in FY06. Ion Beam Assisted Deposition (IBAD) results are good for a non-impregnated structure. IBAD may have significant advantages over other systems and for manufacturing if future cathode performance can be achieved. However, production costs and stack manufacturers' acceptance of cloth GDL material are still issues.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FC-19	*Advanced MEAs for Enhanced Operating Conditions, Amenable to High Volume Manufacture; 3M; Mark Debe*	3.7				X	Project successfully completed in FY06. Holistic and methodical approach. Important recognition that any chemical system must be amenable to continuous manufacturing operations. Reviewers said product development data would have more credence if results were reported collaboratively with stack developers. Stack is awaiting testing at ANL.
FC-20	*Development of High Temperature Membranes and Improved Cathode Catalysts for PEM Fuel Cells; UTC; Lesia Protsailo*	3.1				X	Project successfully completed in FY06. Good integration of experimental and modeling work. Well thought out approach that looked at high-temperature MEA, not just high temperature membrane. Determined the relationship between O_2 permeability and membrane durability and extended durability of PtIrCo catalysts. Reviewers would like to see TEM analysis from accelerated testing to look for changes to catalyst population and distribution.
FC-21	*Electrocatalyst Supports and Electrode Structures; LANL; Mahlon Wilson*	2.8				X	Project successfully completed in FY06. Continuation subject to results of lab call. Focus on durability of the new supports; show reduced surface mobility and increased Pt activity.
FC-22	*Fundamental Science for Performance, Cost and Durability; LANL; Bryan Pivovar*	2.7		X			Identify the causes and solutions of delamination rather than correlating degree of delamination with performance. Project goals are in three areas so may need increased focus on less topics to meet deadlines.
FC-23	*Fuel Cell Systems Analysis; ANL; Rajesh Ahluwalia*	3.0	X	X			Expand the collaborative information sources and include more "real world" experience.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FC-24	*Effect of Fuel and Air Impurities on Fuel Cell Performance; LANL; Fernando Garzon*	3.2				X	Project successfully completed in FY06. Continuation subject to results of lab call. Complete the work on sulfur and ammonia. Look at lower catalyst loadings in the range of the 2010 loading targets.
FC-25	*High Temperature/Low Humidity Polymer Electrolytes Derived from Ionic Liquids; LANL; Jim Boncella*	2.5		X			Consider splitting the test protocol up into mandatory tests and those which can be done in addition.
FC-26	*Neutron Imaging Study of the Water Transport Mechanism in a Working Fuel Cell; NIST; Muhammad Arif*	3.5		X			If possible, carry out experiments in a fuel cell stack. Try to ensure that reactants can be conditioned to the same temperature as the fuel cell when cold start experiments are performed.
FC-27	*Microstructural Characterization Of PEM Fuel Cell MEAs; ORNL; Karren More*	3.7		X			If possible, work on new membranes, catalysts, MEAs should be pursued.
FC-28	*PEM Fuel Cell Durability; LANL; Rodney Borup*	3.4		X			Spend more time on causes of degradation and mitigation.
FC-29	*Investigating Failure in Polymer-Electrolyte Fuel Cells; LBNL; John Newman*	2.8	X			X	Project successfully completed in FY06. Continuation subject to results of lab call. Coordinate with ANL to facilitate transfer of results between the modeling efforts.
FC-30	*Sub-Freezing Fuel Cell Effects; LANL; Rangachary Mukundan*	3.1				X	Project successfully completed in FY06. Continuation subject to results of lab call. Full-scale testing of complete systems would be valuable.
FC-31	*Back-up/Peak-Shaving Fuel Cells; Plug Power; John Vogel*	3.2				X	Project successfully completed in FY06. Fuel Cell unit is being tested at three independent locations.
FC-32	*Economic Analysis of Stationary PEM Fuel Cell Systems; Battelle; Harry J. Stone*	2.3		X			In FY07, study will be expanded to include emergency response back-up power (such as 911 call centers); specialty vehicles – airport tugs and fork lifts.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FC-33	*Scale-Up of Carbon/Carbon Bipolar Plates; Porvair Corp.; David Haack*	2.8				X	Project to be completed by Nov 2006 with FY06 funding. Remaining plan is to address shortcomings in the material development such as finding a sealant that will withstand durability testing, and improving process time for high volume production.
FC-34	*Cost-Effective Surface Modification for Metallic Bipolar Plates; ORNL; Peter Tortorelli*	3.1				X	Project successfully completed in FY06. Continuation subject to results of lab call. Consider more accurate cost evaluation methods and cost effectiveness.
FC-35	*Platinum Recycling Technology Development; Ion Power, Inc.; Stephen Grot*	2.8		X			Platinum recycling will be very important in maintaining a viable supply of fuel cell catalyst precious metals required for full scale fuel cell commercialization. The project has achieved success in clearly demonstrated the viability of platinum recovery and good progress in recovery of membrane ionomers.
FC-36	*Platinum Group Metal Recycling Technology Development; Engelhard; Larry Shore*	2.8		X			Platinum recycling will be very important in maintaining a viable supply of fuel cell catalyst precious metals required for full scale fuel cell commercialization. Very well planned future work with go/no-go criteria established. It is beneficial that the prime contractor is a domestic producer and refiner of Pt group metals.
FCP-08	*High Temperature Membrane; LBNL; John Kerr*	3.1	X			X	Project successfully completed in FY06. Continuation subject to results of lab call. Consider *in situ* evaluation of candidate membranes with metrics for conductivity over a temperature range and sensitivity to known failure modes of PEMs.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FCP-09	*Component Benchmarking; LANL; Tommy Rockward*	3.1				X	Project successfully completed in FY06. Continuation subject to results of lab call. Investigate more promising polymer types as well as preparing and characterizing polymer/ionic liquid blends.
FCP-13	*Montana PEM Membrane Degradation Study; Montana State Univ.; Lee Spangler*	2.4				X	Project successfully completed in FY06. Use the methods developed to probe into more fundamental degradation mechanisms.
FCP-20	*Residential Fuel Cell Demonstration by the Delaware County Electric Cooperative, Inc.; Del. Co. Electric Co-op; Mark Schneider*	2.9				X	Project successfully completing with FY06 funding; energy storage data collection will be completed in December 2006 and reporting will be completed in January 2007.
FCP-23	*Sub-Freeze Performance; ANL; Shabbir Ahmed*	2.9	X			X	Project successfully completed in FY06. Continuation subject to results of lab call. Add more fundamentals of transport of water during freeze/thaw.
FCP-25	*Corrosion Protection of Metallic Bipolar Plates for Fuel Cells; NREL; John Turner*	3.1				X	Project successfully completed in FY06. Continuation subject to results of lab call. Consider the impact of inclusions in the steels on the nitride protection. Develop a test plan for corrosion evaluation and formability evaluation.
FCP-26	*Development of Low-Cost, Clad Metal Bipolar Plates for PEM Fuel Cells; PNNL; Scott Weil*	3.3				X	Project successfully completed in FY06. Continuation subject to results of lab call. Breakout cost of the substrate, clad material, and any post processing and then add the cost of finishing operations to form flow fields. *In situ* experimentation on cladded plates will be able to confirm whether DOE targets are met.
FCP-27	*Advanced Catalysts for Fuel Cells; JPL; S. Narayanan*	2.6				X	Project successfully completed in FY06. Investigate origin of low values of the ORR onset potentials. Demonstrate viability of the Pt-M-Zr catalysts for fuel-cell type cathode.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
FCP-28	*Contaminant Effects; ANL; Debbie Myers*	3.1	X			X	Project successfully completed in FY06. Continuation subject to results of lab call. Carefully consider real pollutant and impurities that will be experienced by the early fuel cell cars in real air sheds.
FCP-29	*Non-Platinum Catalysts; ANL; Xiaoping Wang*	2.4	X			X	Project successfully completed in FY06. Continuation subject to results of lab call. Coordinate effort with other Labs.
FCP-40	*Tungsten Oxide Cathode Catalysts; ORSAM; Joel Christian*	2.2				X	This congressionally-directed effort was completed in FY06.
FCP-42	*Smart Fuel Cell Operated Residential Micro Micro-Grid Community Grid Community; U of S. Alabama; Mohammad Alam*	2.0				X	University of South Alabama is completing congressionally-directed work in FY07.

Technology Validation:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
TV-01	*DTE Energy Hydrogen Technology Park; DTE Energy; Rob Bacyinski*	3.2		X			This program has broad education and demonstration capability and is providing essential H2 delivery infrastructure facility and education in a key vehicle test area.
TV-02	*Power Parks System Simulation; SNL; Andy Lutz*	3.3		X			A good understanding of energy peak performance parameters. Strong effort to validate tools using real world systems.
TV-03	*High-Pressure Cold H2 Storage Vehicle Demo; LLNL; Salvador Aceves*	3.1		X			Reasonable plan to install cryo-gas tank on a hydrogen hybrid vehicle. 2nd generation hardware suitable for vehicle use.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
TV-04	*Development of a Natural Gas-to-Hydrogen Fueling System; GTI; Bill Liss*	2.9				X	Good technical progress in fuel reforming efficiency, fast-fill testing, and fuel dispensing. Little progress made in hydrogen compression technology.
TV-05	*Development of a Turnkey Hydrogen Fueling Station; Air Products; David Guro*	2.9		X			New PSA appears to be of high value. Insufficient station operation for complete demonstration.
TV-06	*Validation of an Integrated System for a Hydrogen-Fueled Power Park; Air Products; Greg Keenan*	3.0		X			Molten Carbonate Fuel Cell project relies on mature technologies. Engineering design should be able to be implemented. Phase 3, detailed design and fabrication, of the project should be done.
TV-07	*Hydrogen Vehicle and Infrastructure Demonstration and Validation; General Motors; Roz Sell*	3.4		X			These were well-directed projects critical to the support of the President's Hydrogen Fuel Initiative.
TV-08	*Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project; DaimlerChrysler; Klaus BonHoff*	3.7		X			These were well-directed projects critical to the support of the President's Hydrogen Fuel Initiative.
TV-09	*Hydrogen Fuel Cell Vehicle & Infrastructure Demonstration Program Review; Ford; Greg Frenette*	3.7		X			These were well-directed projects critical to the support of the President's Hydrogen Fuel Initiative.
TV-10	*Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project; Chevron Texaco; Linda Gallaher*	3.5		X			These were well-directed projects critical to the support of the President's Hydrogen Fuel Initiative.
TV-11	*California Hydrogen Infrastructure Project; Air Products; Mark Pedersen*	2.9		X			Work with OEM's station operators, and other objectives similar to the learning demonstrations, is good. The pipeline concept provides a low cost hydrogen production option while securing delivery capacity.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
TV-12	*Controlled Hydrogen Fleet & Infrastructure Analysis; NREL; Keith Wipke*	3.6		X			The Analysis conducted by NREL is critical to be able to convey information to the public.
TVP-01	*Hawaii Hydrogen Center for Development and Deployment of Distributed Energy Systems; Hawaii Natural Energy Inst.; Richard Rocheleau*	3.0		X			This is a good example of a hydrogen technology center with a good track record capable of reliable hydrogen and renewable energy technology project deployment, evaluation, and education.
TVP-03	*Novel Compression and Fueling Apparatus to Meet Hydrogen Vehicle Range Requirements; Air Products; Todd Carlson*	3.1				X	This is a good example of a hydrogen technology center with a good track record capable of reliable hydrogen and renewable energy technology project deployment, evaluation, and education.
TVP-05	*Chattanooga Fuel Cell Demonstration Project; City of Chattanooga; Joe Ferguson*	2.5				X	Actively communicating the collected information to the community and Hydrogen program stakeholders, as well as soliciting H_2 uses will round out the project.
TVP-06	*NextEnergy Microgrid and Hydrogen Fueling Facility; NextEnergy; Dave McLean*	2.6				X	The basic need to have a good hydrogen refueling center with capability to evolve with the rapidly emerging hydrogen vehicle technologies in the area is really the key feature of the facility.
TVP-08	*Hydrogen Filling Station; UNLV; Robert Boehm*	2.7				X	The approach is not clear, collaborators are not adequately addressed and the project is working on areas that don't fit together and would be better if focused on particular process improvements.
TVP-10	*Fuel Cell Powered Underground Mine Loader Vehicle; Vehicle Projects LLC; David Barnes*	2.5				X	Program has shown that Hydrogen and fuel cells can be deployed successfully in specialized applications. The project, while successful, is a somewhat less important project for fulfilling the President's H_2 Initiative.

Safety Codes and Standards:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
SA-01	*Hydrogen Codes and Standards; NREL; Jim Ohi*	3.4		X			This project is well organized with participation from domestic and international codes and standards development organizations and will continue to produce significant results toward the development of a comprehensive and performance-based set of codes and standards for the safe use of hydrogen and hydrogen systems.
SA-02	*Research and Development for Hydrogen Safety, Codes and Standards; SNL; Jay Keller*	3.5		X			A systematic approach to developing the experimental data needed on hydrogen behavior and to ensure that scientifically sound data is available for the codes and standards development process. This project also includes the development of a hydrogen materials compatibility database and risk assessment efforts.
SA-03	*International Standards and Regulations; LANL; Cathy Padro*	2.6		X			This project supports the development of a Global Technical Regulation for hydrogen vehicle systems under the United Nations Economic Commission for Europe, World Forum for Harmonization of Vehicle Regulations, and Working Party on Pollution and Energy Program (ECE-WP29/GRPE). This project supports and works with DOT/NHTSA and EPA to coordinate US position on the development of international hydrogen/fuel cell codes, standards, and regulations that are performance-based.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
SA-04	*First Responder Training Hardware & Incident Reporting Database; PNNL; Bruce Kinzey*	3.6		X			This project consists of establishing a web-based system for open sharing of lessons learned from hydrogen incidents and near misses and developing a Hydrogen Best Practices document to share the knowledge and experience already attained in industry, aerospace and elsewhere. Excellent progress despite the limited funding.
SA-05	*Hydrogen Safety Review Panel; PNNL; Steven Weiner*	3.3		X			This project helps ensure the safety of the DOE funded projects and brings expertise from industry, academia, government and the private sector to conduct safety reviews and to make safety recommendations. Excellent progress despite the limited funding.

Education:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
ED-01	*Hydrogen Technology and Energy Curriculum (HyTEC); U of Cal. Berkeley; Barbara Nagle*	3.4		X			Important project to address long-term education objectives. Good progress despite limited funding. Coordinate with related projects where possible, build on partnerships, and use established network for widespread dissemination and to extend project reach. Continue, pending FY2007 Appropriations.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
ED-02	*H2 Educate!; NEED; Mary Spruill*	3.5		X			Important project to address long-term education objectives. Excellent progress despite limited funding. Coordinate with related projects where possible, build on partnerships, and use established network for widespread dissemination and to extend project reach. Continue, pending FY2007 Appropriations.
ED-03	*Training for Safety and Code Officials; PNNL; Bruce Kinzey*	3.2	X	X			Important for near-term demonstration and deployment. Good collaboration with Safety, Codes and Standards program. Stakeholder input, pilot tests, and broad review greatly strengthen project. Continue, pending FY2007 Appropriations.
ED-04	*Hydrogen Community Education Program; The Media Network; Henry Gentenaar*	3.2	X	X			Project is in early stages but has good potential for success. Scope must remain realistic, given budget constraints. Simple messaging and partnerships are critical. Continue, pending FY2007 Appropriations.
EDP-01	*Hydrogen/Alternative Energy Center; Lansing Comm. College; Ruth Borger*	3.1				X	FY2004 Congressionally- directed project. Should be completed in FY2007 with no additional funding required.
EDP-02	*Shared Technology Transfer Project; Nicholls State U.; John Griffin*	2.1				X	FY2004 Congressionally- directed project. Should be completed in FY2006 with no additional funding required.
EDP-03	*Montana Hydrogen Futures Project; U. of Montana; Paul Williamson*	2.3				X	FY2004 Congressionally- directed project. Should be completed in FY2006 with no additional funding required.

Analysis:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
AN-01	*Hydrogen Production Infrastructure Options Analysis; Directed Techs.; Brian D. James*	2.9		X			Continue to support. The model enables analysis of optimum hydrogen production technologies and is critical to the hydrogen transition analysis.
AN-02	*Impact of Hydrogen Production on U.S. Energy Markets; EEA; Harry Vidas*	2.8		X			Continue to support. In FY 2006, the project was in the initial phase due to funding delays. The project will provide critical data for resource and hydrogen infrastructure.
AN-03	*Analysis of the Hydrogen Production and Delivery Infrastructure as a Complex Adaptive System; RCF, Inc.; George Tolley*	2.9		X			Continue to support. In FY 2006, the project is in the initial phase due to funding delays. The project will provide infrastructure analysis based on risk analysis, imperfect knowledge and behavioral changes of consumers.
AN-04	*WinDS-H_2 Model and Analysis; NREL; Keith Parks*	3.1		X			Continue to support. This model enables infrastructure analysis with available resources and optimum supply of hydrogen.
AN-05	*Macro-System Model; NREL; Mark Ruth*	3.7		X			Continue to support. This model is essential for linking models of varying complexity to enable complete pathway cost and well-to-wheels analysis. The test version of the model was issued in FY 2006 with links to the H2A and GREET models. Additional models will be linked in FY2007.
AN-06	*Hydrogen Transition Infrastructure Analysis; NREL; Margo Melendez*	2.7				X	The project is complete. The project information will be used as inputs to the other analysis projects.
AN-07	*Hydrogen Transition Modeling and Analysis: HYTRANS v. 1.0; ORNL; David Greene*	3.4		X			Continue to support. This project is co-funded with PBA. The project should continue since it will be a useful model for vehicle penetration and transition analysis. The model is planned to be linked with the Macro System Model.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	Summary Comment
AN-08	*Hydrogen Analysis Support; PNNL; Marylynn Placet*	3.1		X			Continue to support. The hydrogen data base and website will be updated annually with the most current information related to hydrogen. This information center provides critical information for consistent analysis.

TABLE OF CONTENTS

INTRODUCTION

This report is a summary of comments from the Peer Review Panel at the FY 2006 DOE Hydrogen Program Annual Merit Review, held on May 16-19, 2006, at the Gateway Crystal Marriott in Arlington, Virginia. The work evaluated in this document supports the Department of Energy (DOE), and the results of this merit review and peer evaluation are major inputs utilized by the DOE in making its funding decisions for following fiscal years.

The objectives of this meeting were to:

- Review and evaluate FY 2006 accomplishments and FY 2007 plans for DOE laboratory programs and industry/university cooperative agreements.
- Provide an opportunity for program participants (developers of hydrogen production, delivery, storage, and fuel cell technologies) to shape the DOE sponsored R&D program so that the highest priority technical barriers are addressed. The meeting also serves to facilitate technology transfer.
- Foster interactions among the national laboratories, industry, and universities conducting the R&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, attended the meeting and provided comments on the projects presented. These panel members are peer experts from a variety of hydrogen and fuel cell related backgrounds including national laboratories, developers of hydrogen and fuel cell technologies, universities, and other U.S. Government agencies. Each member was screened from a conflict of interest (COI) perspective per the Peer Review Guide. A complete list of the meeting participants is presented as Appendix A to this report.

Table 1: Peer Review Panel Members

No.	Name	Organization
1	Tarek Abdel-Baset	Daimler-Chrysler Corporation
2	Kev Adjemian	consultant
3	Radoslav Adzic	BNL
4	Shabbir Ahmed	ANL
5	James Alkire	GFO
6	Arlene Anderson	U.S. Department of Energy
7	Tim Armstrong	Oak Ridge National Laboratory
8	Radaslov Atanasoski	3M
9	Paulina Atanasova	Cabot Superior Micropowders
10	Balu Balachandran	Argonne National Laboratory
11	Olga Baturina	Naval Research Laboratory
12	Farshad Bavarian	Chevron Texaco
13	Bud Beebe	SMUD
14	Harold Beeson	White Sands Test Facility
15	Thomas Benjamin	Argonne National Laboratory
16	Jeff Bentley	CellTech Power
17	Larry Blair	LANL/Retired
18	Chris Bordeaux	Bordeaux International Energy Consulting, LLC
19	Arun Bose	NETL
20	Lynnae Boyd	National Renewable Energy Laboratory

21	Eric Carlson	TIAX
22	Joe Carpenter	DOE
23	Daniel Casey	ChevronTexaco
24	Richard Chahine	U. of Quebec
25	Bill Chernicoff	DOT
26	Biswajit Choudhury	DuPont Fuel Cells
27	Deryn Chu	ARL
28	Helena Chum	NREL
29	Whitney Colella	Stanford University
30	Bill Collins	UTC Power/Fuel Cells
31	Mario Conte	Italian National Agency - ENEA
32	James Cross	Nuvera
33	Maria Curry-Nkansah	BP
34	Dennis Curtin	DuPont
35	Mark Debe	3M
36	Lutgard DeJonghe	LBNL
37	Jeff DeLaune	Wisconsin Power
38	Millie Dresselhaus	MIT
39	Anthony Eggert	UC Davis
40	Glenn Eisman	RPI
41	Elam Carolyn	U.S. Department of Energy
42	Mohammad Enayetullah	Protonex Technology Corporation
43	Erich Erdle	DaimlerChrysler
44	William Ernst	Plug Power
45	Linda Eslin	Concurrent Technologies Corporation
46	Dave Farese	Air Products
47	Jim Fenton	UCF
48	Karl Fiegenschuh	Ford Motor Company
49	Constantina Filiou	European Commission
50	Florian Finsterwalder	DaimlerChrysler Corporation
51	Scott Freeman	DaimlerChrysler Corporation
52	Robert Friedland	Proton Energy Systems, Inc.
53	George Froudakis	University of Crete
54	Tom Fuller	GTI
55	Alexi Gabrielov	Shell Hydrogen
56	Jennifer Gangi	Fuel Cells 2000
57	Jason Ganley	Howard University
58	Bob Glass	Lawrence Livermore
59	Raghubir Gupta	RTI
60	David Haberman	IF, LLC
61	Steve Hamrock	3M
62	Jonathan Hardis	NIST
63	Marianne Harmon	GE Global Research
64	Barbara Hennessey	National Highway and Traffic Safety Administration

65	Andy Herring	Colorado School of Mines
66	Steve Herring	INEL
67	Mike Hicks	3M
68	Shinichi Hirano	Ford Motor Company
69	Katsuhiko Hirose	Toyota
70	Raymond Hobbs	APS
71	Doug Hooker	DOE
72	Mark Janney	Porvair Advanced Materials
73	Craig Jensen	U. Hawaii
74	Scott Jorgensen	GM
75	Erik Kallio	TACOM
76	Junji Katamura	
77	Richard Kelley	DOE
78	John Kerr	LBNL
79	Merrill King	NASA
80	John Kopasz	Argonne National Laboratory
81	Curt Krause	ChevronTexaco
82	Theodore Krause	ANL
83	Romesh Kumar	Argonne National Laboratory
84	Nobuhiko Kuriyama	AIST
85	Pete Langlois	Ernst & Young
86	Stephen Lasher	TIAX
87	Jay Laskin	Consultant
88	Michele Lewis	ANL
89	Ludwig Lipp	FuelCell Energy
90	Melissa Lott	Alliance Technical Services
91	William Lueckel	Renewable Fuels Association
92	Andy Lutz	Sandia National Laboratory
93	Stanislav Malyshenko	Russian Academy of Sciences
94	Robert Mantz	ARO
95	Len Marianowski	Consultant (retired from GTI)
96	Nenad Markovic	ANL
97	Victor Maroni	ANL
98	David Masten	GM
99	Tony Mazza	Hydrogenics Corporation
100	Jim McGetrick	BP
101	William McLeod	Consultant
102	Shawna Mcqueen	Energetics
103	Stephon Melancon	Entergy Nuclear
104	James Miller	ANL
105	Rana Mohtadi	Toyota Technical Center
106	Henk Mooiweer	Shell
107	Graham Moore	ChevronTexaco
108	Tom Moore	Consultant

109	Ted Motyka	Savannah River National Laboratory
110	Deborah Myers	Argonne National Laboratory
111	Gene Nemanich	Consultant
112	Cathy Padro	Los Alamos National Lab
113	George Parks	Conoco Philips
114	Pinakin Patel	FuelCell Energy, Inc.
115	Dilo Paul	NETL
116	Mike Pero	Hydrogen Safety, LLC
117	Mike Perry	UTC Fuel Cells, LLC
118	John Peters	Montana State University
119	John Petrovic	DOE/Retired
120	Guido Pez	Air Products & Chemicals
121	Peter Pintauro	Case Western
122	Bryan Pivovar	LANL
123	Walter Podolski	ANL
124	Joseph Poindexter	Teledyne Energy Systems, Inc.
125	Michael Quah	NextEnergy/CTC
126	Venki Raman	Protium Energy
127	Dan Rastler	EPRI
128	Robert Remick	Colorado Fuel Cell Center
129	Vernon Roan	University of Florida
130	John Robbins	ExxonMobil
131	Mark Roelofs	DuPont
132	Jerry Rogers	General Motors Corporation
133	Phillip Ross	Lawrence Berkeley National Laboratory
134	Leon Rubinstein	Shell Hydrogen
135	Gary Sandrock	Retired
136	Dave Schiraldi	Case Western
137	Steve Schlasner	ConocoPhillips
138	Jesse Schneider	DaimlerChrysler RTNA
139	Patrick Serfass	National Hydrogen Association
140	John Shen	DOE
141	Dave Sjoding	Washington State University
142	Ed Skolnik	Energetics, Inc.
143	Ken Stroh	Los Alamos National Lab
144	Karen Swider-Lyons	NRL
145	Hazem Tawfik	State University of New York & BNL
146	George Thomas	DOE
147	John Titchen	Hydro Tasmania
148	Doanh Tran	DaimlerChrysler Corporation
149	John Turner	NREL
150	Nicholas Vanderbogh	Consultant
151	Henry Voss	PolyFuel
152	Fred Wagner	Energetics

153	Fred Wagner	General Motors Corporation
154	Jim Waldecker	Ford Motor Company
155	Sharlene Weatherwax	DOE
156	Alan Weimer	University of Colorado
157	Steve Weiner	PNNL
158	Cory Welch	National Renewable Energy Laboratory
159	Ed Wenzinger	MPR Associates
160	Rose Wesson	NSF
161	Doug Wheeler	consultant
162	Robert Wichert	USFCC
163	Mahlon Wilson	LANL
164	Chris Wolverton	Ford Motor Company
165	Chao Wu	Southern Company
166	Jung Yi	Arkema Inc
167	Tom Zawodzinski	Case Western
168	Piotr Zelenay	LANL
169	Richard Ziegler	Sentech, Inc.

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

The Peer Review Panel members provided a number of comments and recommendations that apply to the Annual Merit Review and peer review process, as well as overall management of the DOE Hydrogen Program. These comments are provided in Appendix C of this report. DOE will utilize these comments to improve both the program and future review meetings.

ANALYSIS METHODOLOGY

As shown above, **169** panel members participated in the merit review process. A total of **167** projects were reviewed at the meeting and a total of **1015** evaluation forms were received from the Peer Review Panel (not every panel member reviewed every project). These panel members were asked to provide numeric scores (on a scale of 1 to 4, with 4 being the highest) for five aspects of the research on their Evaluation Form, a sample of which can be found as Appendix C.

The five criteria and weights were:

- Relevance to overall DOE objectives (20%);
- Approach to performing the research and development (20%);
- Technical accomplishments and progress toward achieving the project and DOE goals (35%);
- Technology transfer and collaborations with industry, universities, and other laboratories (10%); and
- Approach to and relevance of proposed future research (15%).

All the individual criterion scores from various reviewers were averaged together to obtain average scores for each of the five above-mentioned criterion for every project. These average scores were then weighted and combined to produce a final overall score for that project. In this manner, a project's final overall score can be compared to other projects. Following is the formula used to calculate the weighted average overall score:

*Final Score = Score1*0.20 + Score2*0.20 + Score3*0.35 + Score4*0.10 + Score5*0.15*

A few new projects were reviewed, where the third criterion (Technical Accomplishments) did not apply because of the project's recent startup. In this case, the other four criteria were scaled proportionally in the weighting calculation and the following formula was used:

> *Criterion 3/ Technical Accomplishments weighted at 35% not included; therefore, weighting value for remaining scores = (weight +35/65*weight)*
>
> *Final Score = Score1*(0.20+(35/65)*0.20) + Score2*(0.20+(35/65)*0.20) + Score4*(0.10+(35/65)*0.10) + Score5*(0.15+(35/65)*0.15)*
>
> *So, Final Score = Score1*0.31 + Score2*0.31 + Score4*0.15 + Score5*0.23*

A maximum final overall score of 4 signifies that the project satisfied the above mentioned five criteria to the fullest possible extent, while a minimum score of 1 implies that the project did not satisfactorily meet any of the requirements of the five criteria mentioned above.

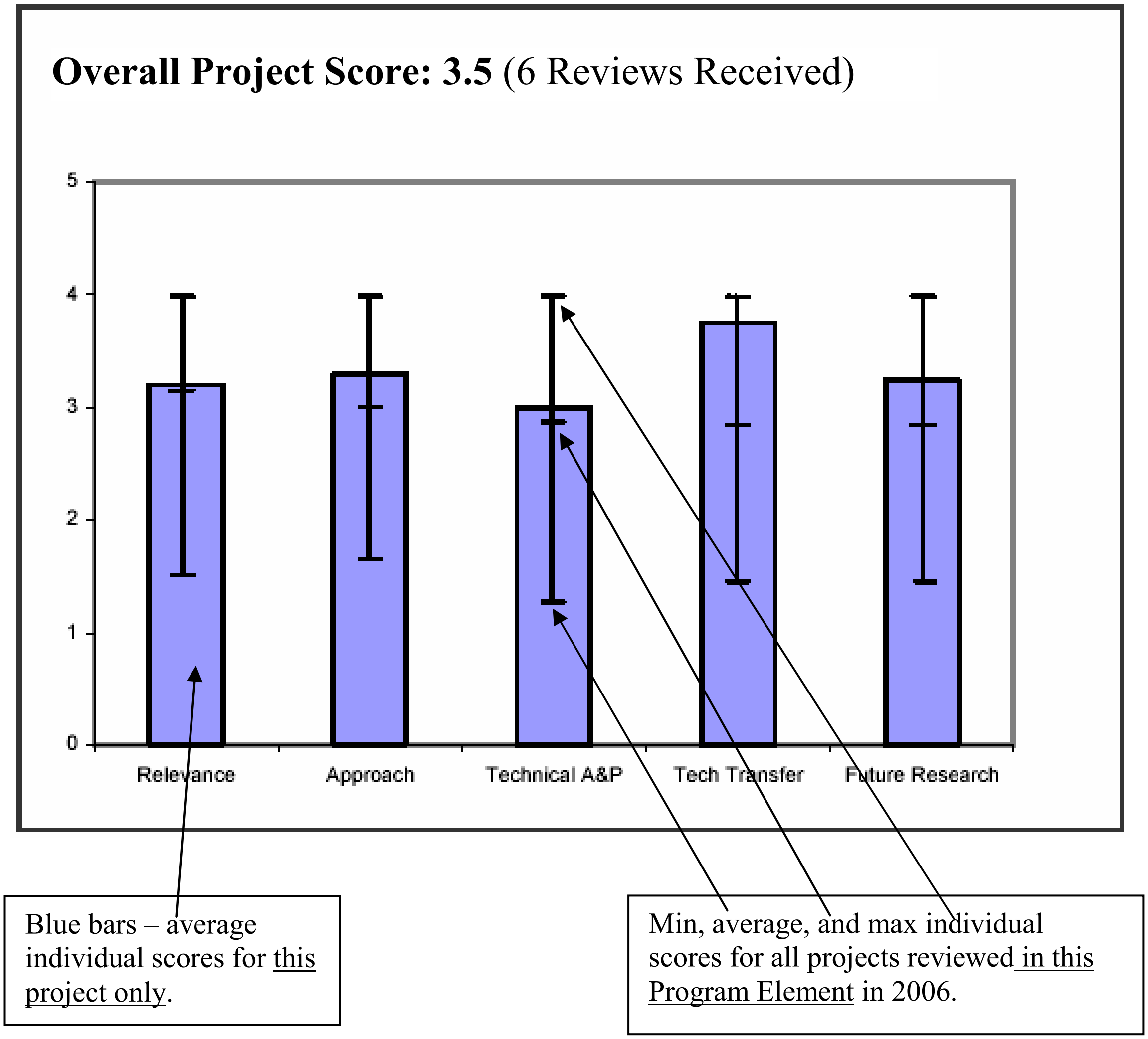
Reviewers were also asked to provide qualitative comments on the five research aspects, as well as the specific strengths and weaknesses of the project, and any recommendations for additions or deletions to the work scope.

These comments, along with the quantitative scores, were placed into a database for easy retrieval and analysis. These comments are summarized in the following sections of this report.

ORGANIZATION OF THE REPORT

This report is organized in seven sections, in an effort to group projects according to the program elements in which they fall in DOE Hydrogen Program planning. A brief description of the general type of research being performed in each category is presented at the beginning of each major report section.

The remaining pages of each section present the results of the analysis for each of the projects discussed at the merit review. A summary of the qualitative comments is provided, as well as graphs showing overall score and how the particular project compared with all other projects presented within each program category. An example of a graph is provided below:



The project comparisons illustrated in the report are criteria based. Each rectangular blue bar in the chart represents that project's score for that particular criterion of the project. The displayed score for each criterion of a project was obtained by averaging the individual reviewer scores for that particular criterion of the project.

This project's score for each particular criterion (each blue bar) was then compared with the maximum, minimum and average score for that same criterion of all the presented projects (across all sub sections of the Hydrogen program). The maximum, minimum and average scores for a criterion across all the presented projects is graphically displayed by the black line bars which overlay the blue rectangular bars.

For clarification purposes consider that only three projects were presented and reviewed. The hypothetical projects were scored by reviewers as displayed in the table below:

	Relevance	Approach	Technical A&P	Tech Transfer	Future Research
Project 1	4	2	1	4	3
Project 2	1	4	4	3	2
Project 3	2	3	2	1	4
Max	4	4	4	4	4
Min	1	2	1	1	2
Average	2.3	3.0	2.3	2.6	3.0

In this case, the chart for project 2 would contain a blue rectangular bar with a value of 1 (reflecting the score obtained by project 2 for the relevance criterion) and a black line bar with max, min and average values of 4, 1, and 2.3 respectively for the relevance criteria. Below is a sample calculation for the Project 1 weighted score.

*Final Score = 4*0.20 + 2*0.20 + 1*0.35 + 4*0.10 +3*0.15= 2.4*

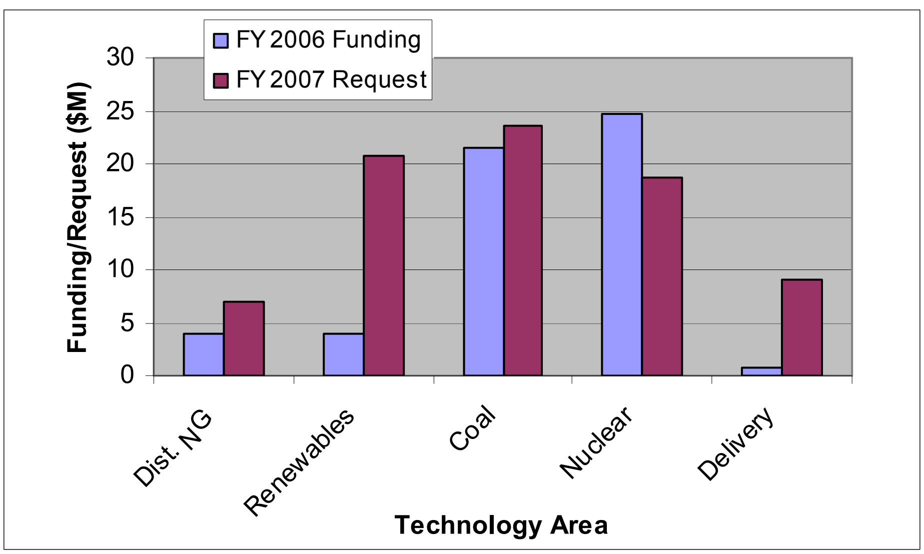
Hydrogen Production and Delivery

Summary of Annual Merit Review Hydrogen Production and Delivery Subprogram

Summary of Reviewer Comments on Hydrogen Production and Delivery Subprogram:

This review session evaluated hydrogen production and delivery research from all DOE activities working on the President's Hydrogen Fuel Initiative, including: the Offices of Science; Fossil Energy; Nuclear Energy, Science & Technology; and Energy Efficiency and Renewable Energy. The production and delivery projects are considered to be well-aligned with the goals and objectives of the Hydrogen Program (program). Projects focused on diverse energy sources and technologies for hydrogen production including natural gas reforming, electrolysis, bio-derived renewable liquids reforming, solar-driven thermochemical cycles, nuclear-driven thermochemical cycles, photoelectrochemical hydrogen production, biological hydrogen production, and hydrogen production from coal. The projects were judged to have made considerable progress, despite a lack of funding in some cases. The major concerns identified in some areas by reviewers were: 1) collaboration roles with some industry partners and other research organizations need to be expanded and clarified; 2) some projects need to better define objectives to align with the Program's technical targets; 3) more project test data is needed to assess progress; and, 4) specific go/no-go decision points are needed on some projects.

Hydrogen Production and Delivery Funding by Technology:



Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the production and delivery projects were high to average, with scores of 3.6, 3.1 and 2.3 for the highest, average and lowest scores, respectively. The scores are indicative of the technical progress that has been made over the past year. Recommendation and major concerns are summarized below.

Distributed Natural Gas and Renewable Liquid Reforming: The diverse project teams were reviewed favorably in terms of technical approach and achievements. Some reviewers requested more data on long-term performance of reformer hardware, and greater detail on future research plans. The projects reviewed are thought to be well-aligned with the Program goals.

Electrolysis: Projects in electrolysis development received generally favorable reviews. Most of the projects were regarded as well-aligned with current program goals and objectives. Reviewers suggested increased collaboration with industrial partners and other DOE-funded projects. Reviewers suggested that one project should be discontinued due to lack of alignment with program objectives. DOE is considering this recommendation.

Solar-Driven High Temperature Thermochemical: The two projects reviewed in the area received favorable comments. The projects had achieved considerable progress and were viewed as a good R&D investment. The reviewers suggested shifting more effort from screening potential thermochemical cycles to obtaining more experimental results and systems analysis.

Photoelectrochemical Hydrogen Production: The projects in this area received mostly high ratings from the reviewers. The projects were viewed to be in-line with the program's long-term goals. The projects have achieved good scientific progress in materials research and have established effective collaborations. Reviewers suggested that increased research effort be devoted to materials durability and systems development.

Biological Hydrogen Production: One relatively new project was reviewed in this area. Some reviewers suggested increased collaborations with industry to apply the exploratory results obtained from this project. Reviewers commended the creativity of the combinatorial approach to catalyst selection and gave high ratings to the well-defined research plan.

General Separations: The projects reviewed in this area received a range of ratings. Reviewers emphasized that the research is fully relevant to RD&D Plan objectives to lower cost of hydrogen production and to improve hydrogen quality for fuel cell applications. Noting the need to test separation technologies under realistic gasification streams, reviewers suggested increased communication with industrial partners to obtain such data.

Hydrogen from Coal: The projects reviewed in this area received a range of ratings. Some of the projects had well-defined targets and had achieved good progress, according to the reviewers. Reviewers observed that other projects needed further alignment with the program's goals and objectives. Also, reviewers suggested that some projects need to narrow their focus to obtain data and results in an accelerated timeframe.

Hydrogen Production Using Nuclear Energy: In general, the projects reviews in this area were favorable. Reviewers approved of the breadth of collaboration for some projects and the well-focused approach of other projects. The projects were judged to be well-aligned with the program's goals. Reviewers recommended that research be driven by materials and cost issues, and that downselects on thermochemical cycles be made. Economic analyses and high-level assessments of licensing issues were also recommended as areas for future efforts.

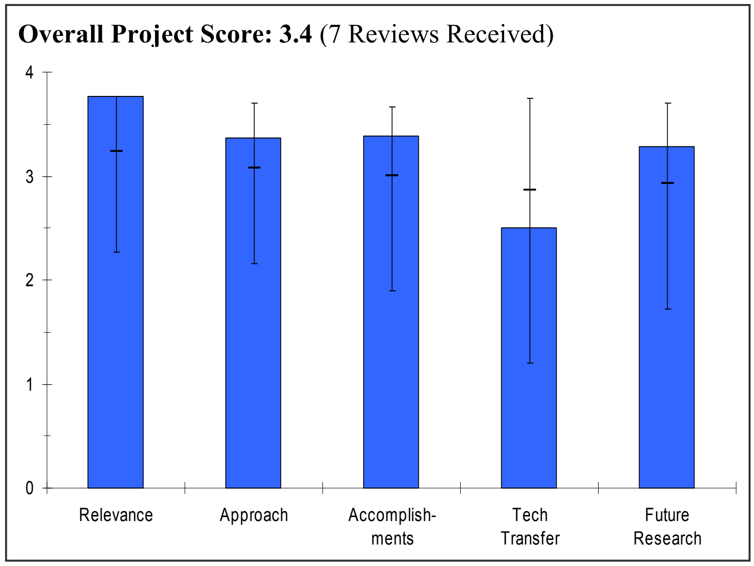
Hydrogen Delivery: The projects reviewed in this area received a range of ratings. Some projects, despite limited funding, had achieved considerable progress. Reviewers recommended continued collaboration with industry to validate analysis assumptions. Reviewers requested that one project improve alignment with program goals and other currents program activities.

Project # PD-01: Low-Cost Hydrogen Distributed Production Systems

Frank Lomax, Jr.; H2Gen Inno. Inc.

Brief Summary of Project

H2Gen Innovations is conducting the development, fabrication and testing of an advanced steam methane reformer and pressure swing adsorption (PSA) system that will produce ~10,000 scfh (565 kg/day) of 99.999% pure hydrogen at over 200 psig, at the DOE cost target of $3/kg. A catalyst suite suitable for use with fuel grade ethanol to facilitate renewable hydrogen production will also be developed. In 2006, hydrogen production capacity will be increased by 30% and a pilot reactor will be operated on ethanol for >1,000 hours.



Question 1: Relevance to overall DOE objectives

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

- This project follows directly with DOE's goals of lower cost hydrogen production from natural gas reforming. DOE's goals were very prominent in the presentation.
- Clearly this project has the potential to meet DOE hydrogen cost targets hence advancing the hydrogen economy imitative.
- Relevant proposal for the DOE H_2 program considering program's emphasis on forecourt hydrogen production. Also appropriately addresses the issue of renewable feedstocks.
- Directly addresses Hydrogen Fuel Initiative goal of low cost distributed reforming of natural gas for hydrogen production.
- Distributed generation of hydrogen is critical to achieving national goals. This project is highly relevant to DOE objectives.

Question 2: Approach to performing the research and development

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

- The project is technically feasible and has a good chance of success.
- Although this project has the potential to meet DOE hydrogen cost targets, the current approach to streamline and use mostly parts optimized for HGM 2000 may yield a less than optimal system and hence may not justify the expenditure.
- Aggressively pursuing capital cost and operating cost reductions via improved hardware design and increasing thermal efficiency.

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

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

- The PI has made most of their goals for 2005.
- More work on the catalyst to reform fuel grade ethanol would have been useful.
- Nothing in the presentation indicates that issue of footprint for the larger (HGM 10000) system is being addressed. Will the unit be of the same size and fit in one container? Limited data on reliability has been presented - still unclear whether the catalyst would be stable for sufficient amount of time under real world

conditions (specifically with different sulfur odorants in different parts of the country. The impact of long runs on the intricate heat exchangers is still unclear (scaling from water).
- Meeting key technical objectives in improving efficiency via heat recovery, increasing capacity, durability, and increasing hydrogen recovery. Many objectives achieved despite significant reduction in funding.

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

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

- Collaboration between partners appear to be minimal. More 3rd party work on PSA development and testing would be helpful as well to assure process credibility.
- Additional presentation detail needed

Question 5: Approach to and relevance of proposed future research

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

- The future plans are well-thought out and are technically feasible.
- Not clear how EtOH reforming will be approached. Potential issues with EtOH conversion into "pseudo syngas" not discussed (ethylene production results in coking). Long term reliability of the system not addressed adequately in the plan.
- Good progress on improved catalysts and adsorbents. Re-emphasize testing in multiple start - stop cycles to address real world durability issues.
- Presentation needs clarification in this area.

Strengths and weaknesses

Strengths
- The 2005 goals have been accomplished in that hardware has been built and operational hours accured. - Good feasible design for the HGM 10,000, - Good ability to meet DOE cost targets (with the larger units) - good improvements in heat recovery with the HGM 2000 - Good improvements in PSA hydrogen recovery target.
- Work to date provides an excellent basis for developing the larger unit -Strong skills present in the company with expertise in various relevant areas.
- Project is well focused on DOE objectives and on scaling up to a useful size for early demonstration and application.
- Team seems knowledgeable and focused on results.

Weaknesses
- Additional production data from the hardware testing would be a welcome addition to the results.
- Uncertain whether the 30% increase in hydrogen production for the HGM 2000 was significant enough to warrant the focus.
- Lack of data on long term performance and lack of clarity how this data will be obtained (especially for the ethanol case). Lack of clarity in ethanol reforming strategy. Process might end up being too sensitive to real world conditions (heat exchanger plugging, variable odorant composition).
- Future plans are unclear
- It appears that the team assumes continued success and hence has no critical review points.

Specific recommendations and additions or deletions to the work scope

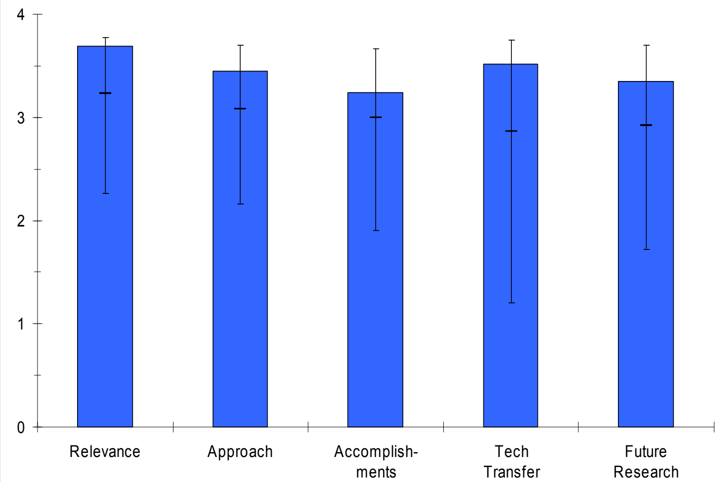
- Need plan to address durability under multiple start-stop cycles.
- Consider running economics cases against "worst case" alternatives.

Project # PD-02: Integrated Hydrogen Production, Purification & Compression System

Satish Tamhankar; BOC Group, Inc.

Brief Summary of Project

Overall Project Score: 3.4 (8 Reviews Received)



The objective of this project is to demonstrate a low-cost option for producing fuel cell quality hydrogen from natural gas or propane to meet the DOE goal of making hydrogen in fuel cell vehicles cost competitive with gasoline and diesel in combustion vehicles. The project team will develop a fuel processor system that directly produces high-pressure, high-purity hydrogen from a single integrated unit. This will be accomplished by combining a membrane reformer developed by Membrane Reactor Technology and a metal hydride compression system developed by HERA USA in a single package. In 2006/2007 the objective is to build and experimentally test a proof-of-concept integrated reformer/metal hydride compressor system.

Question 1: Relevance to overall DOE objectives

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

- The work scope is consistent with the objectives of the Hydrogen Fuel Initiative.
- This project may have the potential to meet DOE hydrogen cost and efficiency targets at volumes of 500 units/90% capacity factor through a design that reduces costs in areas like balance of plant and system maintenance.
- The project provides a sound approach for the production, purification, and compression of hydrogen in one single unit as an effort toward process intensification.
- Approach to capital cost reduction employs several novel components integrating process steps to reduce capital costs.
- This project appears to be highly focused towards commercially viable, cost effective, hydrogen generators. The focus on meeting a fuel grade hydrogen as defined by the CaFCP guideline (which has been superseded by SAE J2719) demonstrates the real world focus. Additionally, flagging the deficiency in adequate published laboratory methods and in-line instrumentation emphasizes this fact.

Question 2: Approach to performing the research and development

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

- Ambitious work plan from POC-scale development to Advanced Prototype for commercial site demo in only 3 years.
- Unclear if the membrane reactor combines methane reforming and WGS reactions or, in addition, combines the separation of hydrogen step.
- More description is needed for the integrated system.
- It is not clear what the driving force for hydrogen separation is.
- It is not clear how hydrogen mass transport, the two reaction kinetic rates, and the rate of the compressor suction head are matched in the system.
- The metal hydride compressor needs more description.
- This is a well designed novel approach to producing high pressure high purity hydrogen from a single unit.

- The project integrates several technologies - steam reforming of natural gas, the WGS reaction, hydrogen separation, and hydrogen compression into two unit operations, namely the membrane reactor and the hydride thermal compressor. While the concept is an example of process intensification, scant details were provided on the working details of the membrane reactor, which presumably integrates three unit operations - reforming, WGS reaction, and hydrogen separation. Integrating these three unit operations is a significant challenge and more details, particularly on matching the kinetics of each unit operation should be provided to avoid skepticism over whether this technical barrier has truly been addressed.
- Integrating hydrogen production and separation eliminates need for water gas shift reactor; hydride compressor eliminates need for costly and high maintenance mechanical compression.
- The approach to this new membrane separation technology appears well grounded in solid best practices. The reformation processes selected are well known and reliable. If the goal of the project is distributed generation, an implicit high content of CO in the tail gas raises some concern with safety; however mitigation can be accomplished by a number of approaches including the inclusion of a shift converter between the reformer and the tail gas burner. Flagging implicit concerns with "hard" starts is an example of real world experience and sound engineering practices.

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

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

- Reported results relate (well) to the original purpose of work. .
- Cost analysis assumptions are unclear for example, is the validity of a one year versus.a two year membrane life based on actual experiments or projections?
- A credit is taken for the higher process efficiency translating to lower natural gas requirement, however, the source of this data is uncertain; it is not clear if the upstream catalytic fluid bed natural gas reforming process is considered commercial; if still in the development stage and steam reforming being an endothermic reaction, the validity of the assumption is not clear.
- Why the membrane replacement costs go down so significantly between one prototype and 200 prototypes are not clear.
- It is not clear if compression costs (1,400 psig to 6,500 psig) are included in calculating the hydrogen
- The techno economic analysis and subsystem performance indicates that the technology is feasible. Significant progress made in modeling overall process and designing test unit. Although experimental confirmation of the analyses is needed, technical accomplishments presented are excellent. Balance of Plant costs appear to be a major driver. However, this is not atypical with first generation hardware and should decrease with design turns. Reductions in BOP costs of a factor of three may be aggressive; a factor of six may not be obtainable.

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

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

- The partnership which includes the gas supplier who can integrate and commercialize the unit with the producers/experts of the individual subsystems makes for a very credible project.
- The project has two collaborators, all of whom appear to be well qualified to participate in a project of this nature.
- There appears to be close coordination between all participants. Close collaboration with suppliers of novel technology components (MRT and HERA).
- Presentation did not name specific collaborators nor describe nature of collaboration.
- The data present was for a proof of concept prototype. The membrane technology will require further development to become commercially viable. Design turns on Balance of Plant, integration of components, and reduction of maintenance and operating costs will require close collaboration.

Question 5: Approach to and relevance of proposed future research

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

- The accomplishment to date supports the proposed follow-on work.
- The remaining three tasks to build, test, and develop a proof of concept prototype and design the concept for mass production is well focused on addressing key barriers. However, more specific task descriptions are recommended.
- Need to re-examine design in ways that permit further cost reduction opportunities in order to meet DOE target H_2 costs.
- Need to include plan to directly measure attrition rate of fluid bed reforming catalyst and erosion rate of membrane under real operating conditions.
- Future opportunities for improvement were defined and stated either implicitly or explicitly. This project appears to be well planned and managed.

Strengths and weaknesses

Strengths

- Favoring equilibrium by separating hydrogen from the reaction site with notable strengths including: 1) the successful operation of the fluidized bed reactor, 2) the efficiency improvements using residual heat from the processor to heat the metal hydride compressor, 3) the reduced number of compression stages, and 4) the potential to reduce both footprint and operation costs.
- Integrating fixed bed membrane reactor and metal hydride compressor is a novel idea. Aggressive pursuit of reducing component count and complexity should be pursued.
- The strengths of this project seem to be the engineering and focus. This project should be a technical success.
- Clear plans and goals were outlined.

Weaknesses

- The approach combines existing knowledge products or technical know-how and potential operational difficulties should be addressed, especially the transport-kinetic coupling;
- Use of air rather than oxygen counters the intent for lowering plant footprint.
- It is unclear how the fully integrated system will perform especially once most of the heat exchangers are removed from the optimized design. The 25 micron membranes may pose a challenge under the proposed reactor conditions.
- It is unclear if the proposed membrane reactor will perform as described - insufficient details were provided to ascertain if this is possible. Need to address the obvious contingencies e.g. Can one technology continue if it is successful, while the other experiences difficulty overcoming obstacles? Other than membrane work, presentation provided little indication of early risk reduction by identifying and selectively addressing higher risk elements before constructing prototype.
- Question whether reductions in cost due to volume and/or scale up of specialty hardware (high temperature compressors, high temperature valuing, membranes, etc.) can be realized.

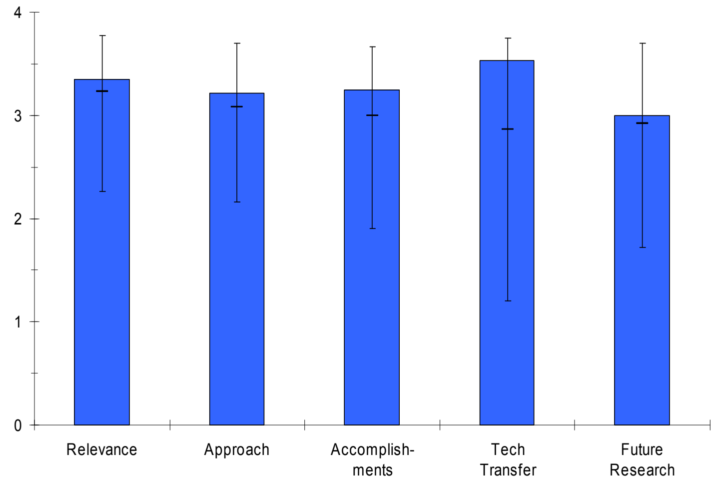
Specific recommendations and additions or deletions to the work scope

- Only if these values are in keeping with DOE's targets should Task 2 (building the POC-scale prototype) be initiated.
- Need to experimentally determine catalyst attrition and membrane erosion rates under working conditions.
- As the project evolves through the various development steps, lessons learned in deficiencies in design and model codes should be flagged and supplied to the codes and standards tech team so that these deficiencies can be addressed. Mention of an on going risk assessment and a FMEA on this design would demonstrate the industrial/commercial focus.
- Possible catalyst attrition and/or carryover may need to be considered as scale up occurs.

Project # PD-03: Integrated Short Contact Time Hydrogen Generator (SCPO)

Ke Liu; GE Global Res.

Overall Project Score: 3.3 (6 Reviews Received)



Brief Summary of Project

The objective of this project is to develop the state of art staged catalytic partial oxidation (SCPO) technology for H2 production that has an efficiency of at least 71% on a LHV basis using natural gas, and a delivered cost of hydrogen less than $3/kg. In 2006, high-pressure catalytic partial oxidation (CPO) and steam methane reforming (SMR) units will be built and testing will be initiated. Two patent applications were filed with the U.S. patent office.

Question 1: Relevance to overall DOE objectives

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

- Directly addresses Hydrogen Fuel Initiative goal of low cost distributed reforming of natural gas for hydrogen production.
- This projects involves fairly mature technology.
- Approach of combining CPO and SMR is a very clever way of creating a "synthetic ATR". Advantages of this approach vs. Conventional ATR did not clearly come out in the presentation but presumably this approach might allow for wider range of feedstocks including renewables.
- Developing low cost hydrogen from natural gas is relevant to DOE objectives.
- Solid use of established technology to build a commercial scale hydrogen generator.

Question 2: Approach to performing the research and development

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

- While the approach is based on established hardware components, the innovative aspect of this project is the catalyst discovery and testing that is done by ANL and U of Minn.
- CPO catalyst manufacturing insufficiently addressed.
- Solid effort to integrate short contact time partial oxidation reactor with steam reforming to reduce size and cost of natural gas reformer.
- Utilizes a number of reactor modeling tools in design phase to minimize number of reactor builds in construction and testing phase. This includes modeling of other integrated components such as heat exchangers, etc.
- Uses well documented technology and applies pragmatic experience in order to produce a commercial grade generator.
- While the presenter indicated a number of potential benefits, the priorities are unclear.

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

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

- Progress toward stated goals and objectives is good.
- Cost estimate is positive although 10-year hardware life and 5-year catalyst lifetime seems optimistic.

- Many project goals were obtained but it is difficult to directly relate these accomplishments to improved performance because they have not yet built the hardware. The PI has shown a lot of catalyst research but the data is confusing to the less informed.
- Project team made significant progress in several key areas (catalyst screening and selection, process and materials modeling, heat exchanger selection, PSA selection, measurement of sulfur impacts)

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

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

- Good collaborations - partners have probably made the most significant technology advances.
- Excellent use of U of MN expertise in the CPO area and ANL experience with SMR catalyst testing. Each group of researchers focuses on what it does best.
- In one project area (catalyst), good involvement from government and educational institutions is obvious.
- There is clearly a close working relationship amongst the team. From data presented it is obvious that all the team members are making important and unique contributions.
- Excellent integration of the team's talents and skills; academic, applied research and commercial manufacturing.

Question 5: Approach to and relevance of proposed future research

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

- Future plans seem unnecessarily extended – should accelerate schedule to complete project.
- Would be good to see a broader range of feedstocks to be tested in the system, especially impure bio derived feedstocks.
- Future plans were not addressed adequately.

Strengths and weaknesses

Strengths
- GE has strong supporting background in related activities – good engineering and economic analysis capabilities.
- Partners add significant innovation in catalyst development.
- Unique and very clever integration between CPO and SMR. Excellent use of resources in academia and National Labs.
- The PI has a good plan for the project. The PI has made good progress and shown a lot of actual data.
- Excellent use of team skills and resources.

Weaknesses
- Project is based on mature hardware components - little innovation or advancement of the state-of-the-art in reformer technology.
- Advantages versus conventional ATR or SMR did not come out as obvious in the presentation.
- The presentation was disorganized and hard to follow for someone not very familiar with reformers. Data was not explained. They have not yet built hardware.
- None at the moment, construction and testing of generator will show weakness, if any.
- Presentation implies that no problems will occur in the upcoming activities on the project. It would seem that some of the analysis done to reach that conclusion could have been presented.

Specific recommendations and additions or deletions to the work scope

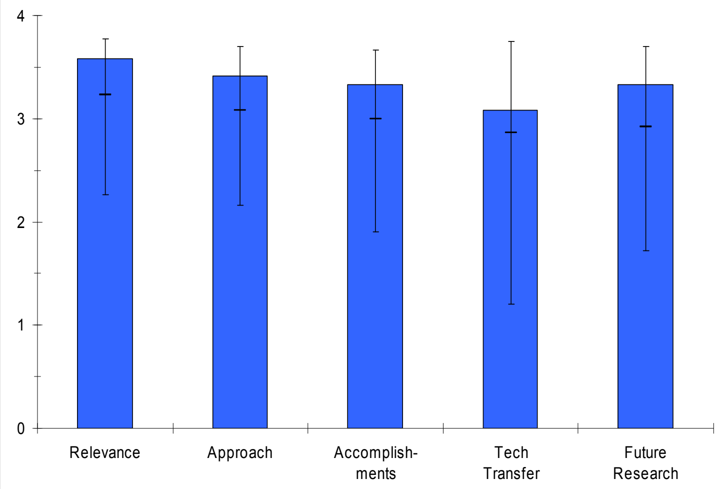
- Shorten project schedule – accelerate prototype fabrication and testing – and let industry move to product development and market introduction if appropriate.
- None at this time.

Project # PD-04: Hydrogen Generation from Biomass-Derived Carbohydrates via Aqueous-Phase Reforming Process

Randy Cortright; Virent Energy Sys.

Brief Summary of Project

Overall Project Score: 3.4 (6 Reviews Received)



This project will combine the expertise of Virent Energy Systems (Virent), Archer Daniels Midland Company (ADM), and the University of Wisconsin to demonstrate the feasibility of generating high yields of hydrogen from biomass including, corn, corn stover, and sugar cane bagasse. The production concept takes advantage of the fact that biomass contains large amounts of carbohydrates which can be extracted and converted to glucose and sorbitol. The resulting aqueous solutions can be fed to Virent's novel aqueous-phase reforming (APR) process that generates hydrogen in a single reactor. The effluent gas from the APR process can then be efficiently purified to produce high purity hydrogen utilizing pressure swing adsorption. The long-term objective is to produce hydrogen for less than $3.00/gge from distributed renewable resources.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The concept technology has the possibility of "$C0_2$ neutral" H_2 generation, however this has not been completely quantified. The speaker presented a somewhat confused message relative to if the work is directed towards making only H_2 or also towards hydrocarbon fuels, or both?
- This project addresses the need to reduce the cost of feedstock for hydrogen production.
- Targets hydrogen production from renewable biomass-based feedstocks
- Use of renewable feedstocks for H_2 production is an important element of the H_2 fuel initiative.

Question 2: Approach to performing the research and development

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

- The approach is good for refined carbohydrates: it's claimed applicability also to lignocellulosic materials was not fully evident.
- An apparently good alternative to fermentation to ethanol (a slower process). Potentially applicable also to hydrogen from ethanol.
- Glucose to hydrogen is a good approach but why hydrogenate the glucose to make sorbitol and then make hydrogen from sorbitol? Sorbitol-to-hydrogen route does not seem to be a good approach to follow.
- Recognizes need to find process that use low cost feedstocks, i.e. Biomass that requires little upfront processing such as hydrogenation and minimum amount of hydrolysis.
- Search for APR (aqueous phase reforming) catalysts to convert glucose to H_2 represents a promising way to reduce H_2 production cost from renewable feedstocks.
- The APR catalyst selectivity and its implications were not adequately discussed. For example, what are the byproducts and how will these byproducts be handled?

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

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

- Good progress and accomplishments, but with the limited funding only really demonstrated for glucose and sorbitol. Selectivities for H_2 are moderate to low (70-30%).
- Funding reduction in FY 06 limited the work to catalyst development. APR improvements shown were made before the start of this contract.
- Some progress has been accomplished. Considering the start of September 2005, this is good.
- Excellent progress in improving H_2 yield and system productivity via improved catalysts.
- The project is in early stage of work. The experimental data generated are very limited.

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

This project was rated **3.1** for technology transfer and collaboration.

- Good collaboration with ADM and University of Wisconsin.
- University of Wisconsin developed the original technology. ADM is supplying feedstock and will host demonstration. There is no technology interaction.
- Getting good support from ADM in providing feedstocks and analytics as well as support from other interested businesses in broadening scope of biomass to hydrogen applications (including IC engines). Impressive progress in light of funding cuts.
- The scope of industry collaboration could be broadened to include (a) biomass hydrolysis experts, and (b) food industries beyond glucose producers.

Question 5: Approach to and relevance of proposed future research

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

- Need to put relatively more effort on dealing with low cost lignocelluosics – a significant challenge beyond glucose and sorbitol feedstocks.
- Plan to design a 50 kg/day hydrogen production unit using APR is good. Doesn't make good sense to evaluate the sorbitol route. ADM is a good place to demonstrate the hydrogen production system using this technology.
- Planned research builds upon existing technology. However, there could be more technology advancement. For example the purification step may be improved taking advantage of the low cost of pressurization.
- Focusing limited 2006 funding on identifying catalyst that processes lower cost glucose (vs. Sorbitol) feedstock. Will use data from this and process studies to select feedstock for demonstration system.
- A good plan was proposed for future research.
- The plan could be improved with a ranking of work priority order.
- Need a go or no-go decision point.

Strengths and weaknesses

Strengths

- Good capabilities in reactor and system design cost modeling and access to bio sources.
- Good collaboration with ADM. Good catalyst work.
- Project builds upon the experience of Virent in Aqueous Phase Reforming.
- Aqueous phase reforming at high pressure lessens the parasitics associated with gas compression.
- Reforming of renewable feedstock lessens the carbon footprint.
- ADM is a good industry partner for feedstock supply and demonstration.
- Work on the use of renewable feedstocks for H_2 production offers a viable option leading to sustainable H_2 production in the future.
- Project appears to be making progress; a number of possible obstacles exist and are identified.

Weaknesses

- Need to be clear on the intended product: is it H_2, or fuel? If the answer is either, this has to be clearly stated.
- The project has little technology collaboration, especially in the technology development. Virent is the sole developer of this new technology in this project.
- There does not appear to be any advancement in purification technology. The APR project has a low cost for pressurization which may lead to a unique purification strategy.
- Project needs to consider the cost of H_2 pressurization for vehicle fueling applications and optimize the reforming pressure.
- The use of cheap biomass feedstock such as bagasse would offer the most promise to make the H_2 from renewable feedstock cost competitive. The issues related to these applications were not adequately discussed.
- The potential hurdles in the development of an APR catalyst to convert glucose to H_2 were not adequately discussed.
- The objective of this project is to find a pathway to produce hydrogen from biomass derived carbohydrates. The end product should be hydrogen. What is the relevance of the GEM and electricity generation to the main objective of this project?

Specific recommendations and additions or deletions to the work scope

- Consider the addition of a purification and/or compression partner. The system should be optimized around the reforming pressure considering the impact on purification and savings on compression to 350 bar. There may be a system sweet spot for operating pressure of the reformer which may not be the ideal pressure of the reformer alone.
- Should begin considering catalyst deactivation issues at an early stage. Especially important to start working with commercial grade feeds and determine impact of feedstock impurities on catalyst life. Increasing catalyst activity by modifying catalyst composition or increasing temperature is highly desirable, as capital cost of the process will benefit from higher weight hourly space velocities (lower cost catalyst; smaller, lower cost reactors.
- One goal of the project involved placing a hydrogen production unit at a fueling facility; in that event consideration of byproduct use/disposal at the site should be considered as an add-on to the scope.
- Work with low cost lignocellulosics as early as possible.
- Continue glucose to hydrogen route. Delete the sorbitol route. Focus on hydrogen generation.

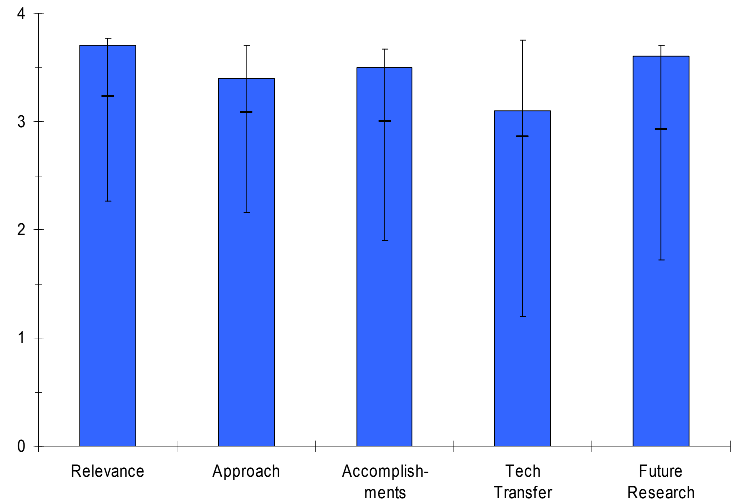
Project # PD-05: Distributed Bio-Oil Reforming

Bob Evans; NREL

Brief Summary of Project

In this project, the National Renewable Energy Laboratory is developing the necessary understanding of the process chemistry, feedstock compositional effects, reactor configuration, catalyst chemistry, deactivation, and regeneration strategy as a basis for process definition and assessment for automated distributed reforming of whole bio-oil. The long-term objective is to produce hydrogen for less than $3.00/gge. The objective for 2006 is to demonstrate partial oxidation and show that it can reduce the required catalyst loading in the reforming step by 50%.

Overall Project Score: 3.5 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Need better explanation of the importance of bio-oil as a feedstock in magnitude, cost and availability. Lower temperature for reforming is promising.

Question 2: Approach to performing the research and development

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

- For the most part, this seems like a very reasonable approach. There is some confusion as to what effect the addition of MeOH is having on the reactions other than making the bio-oil easier to handle. Methanol testing may be warranted.
- Stabilization of bio-oil followed by oxidative cracking was proved as concept. Planned follow up work is technically sound.
- It appears there are many challenges, prioritizing and focus should be keys steps in going forward.

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

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

- Good progress during first year, however progress toward long-term target limited. Achieved low residue of 6% and promising results from oxidative cracking.
- In trying to prove out the oxidative cracking process, the approach seems to be working successfully. Additional detail concerning the overall bio-oil to hydrogen process is needed.
- Significant accomplishments to date include the stabilization of the bio-oil. The development of the injector as a means to study the reaction, and proof of concept for oxidative cracking as a means to reduce carbon deposition. However, the conversion of carbon into carbon monoxide and carbon dioxide to reduce carbon deposition seems obvious.
- No comprehensive performance data was presented indicating conversion / selectivity / yields. The team appears to be far from designing a real working model of the process.

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

This project was rated **3.1** for technology transfer and collaboration.

- Limited collaboration with CO School of Mines, who provides cracking mechanisms, but clarification as to the role of CO School of Mines is needed.
- The parallel contract with Chevron is good. It seems that there is significant overlap between tasks with the prime contractor and the collaborator. Would be useful to more closely include catalyst expertise.
- Good collaborations with academic and research institutions and excellent team communications.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- Justification for needed research into catalyst work was not fully developed.
- Target Go/No-go decision in FY 2007 is good.
- Project plan appears reasonable.

Strengths and weaknesses

Strengths
- The ability to make hydrogen from whole bio-oils would make the separation of bio-products unnecessary. This would make for a more efficient and less costly process.
- Successful demonstration of proof of concept and good interactions with Chevron-Texaco.
- Very promising area to enable low cost biomass transportation.
- Promising results in volatilization and oxidative cracking.

Weaknesses
- Cost estimates (lacking) in relation to other projects reviewed. The analysis should address how this approach compares with direct gasification of biomass. Lack of economic evaluation information in the presentation.
- Changing the methanol content for one process when not doing it in another leads one to believe that the difference in results could be the methanol. (It is never good to change two variables at once without experimentally determining the effect of each.)

Specific recommendations and additions or deletions to the work scope

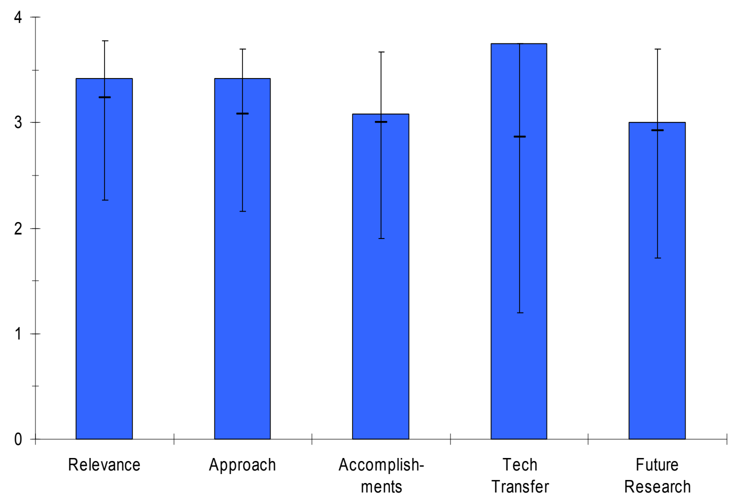
- Need to investigate ways to eliminate any methanol addition for future commercial process. Need ensure that the results are a function of bio-oil and process, not bio-oil process and methanol.
- Start working with process economic analysts to identify critically important process variables that will make or break commercial viability.
- While this project is in its early stages, looking forward should be based on potential market place uses of the technology.
- Experiment with bio-oils from various sources.

Project # PD-06: Photoelectrochemical Hydrogen Production: UNLV-SHGR Program Subtask
Eric Miller; U of Nevada

Brief Summary of Project

In this project, Hawaii Natural Energy Institute is developing cost-effective photoelectrochemical processes for efficient hydrogen production. Critical components of this work include the design, fabrication and testing of stable multi-junction photoelectrodes which incorporate low-cost thin-film materials and are suitable for use in commercial-scale systems.

Overall Project Score: 3.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This is a long term technology – it is unclear if it will meet the DOE solar to hydrogen efficiency targets.
- The project is in direct line with DOE objectives and the presenter did an excellent job of putting the research project in the context of the specific goals and objectives the project is addressing.
- Project target is to advance hydrogen from water using renewable (solar) energy. This supports the longer term objectives of the Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

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

- The rapid throughput bandgap screening technique has been an effective approach to identifying viable materials.
- The project is thorough and highly focused on the barriers.
- The project is highly focused on generating low band gap photoelectrochemical semiconductor junction materials. The focus on lowering band gap and increasing current frequency is appropriate but there is a bit of a disconnect in the materials in terms of a device and an overall device efficiency at this point.
- A great improvement over last year's PEC hydrogen programs. Researchers from a number of institutions have joined forces and established multidisciplinary collaborations to attack this very difficult problem. I especially appreciate the combination of experiment, modeling, and high throughput methods being deployed.

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

This project was rated **3.1** based on accomplishments.

- The STH efficiency of the tungsten oxide materials exceed the 2005 target.
- Very good progress was presented for the synthesis of low band gap materials with interesting results and the ability to correlate functional properties lays a nice groundwork for future work. The problem with device fabrication was not elaborated in the context of the long term goals and implementation of materials.
- Impressive progress made on the WO_3 system – establishes value and validity of the multidisciplinary, multi-laboratory approach. This should give even more interesting results as they move to more novel materials as per plan.

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

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

- The project is excellent at leveraging collaborations with experts in oxide materials design, fabrication and combinatorial discovery.
- The project has an excellent set of partners for collaboration in both university, national lab, and industry. The organizations are focused on their core capabilities.
- The collaborations appear very strong but in the context of the results the role of the individual collaborators were not clearly defined. Several collaborators were introduced in a vague way as having a role in various spectroscopic approaches and it was not clear the importance of these techniques to the overall goals of the project.
- See comments above... Much evidence of strong collaboration amongst researchers at HNEI, NREL, MVsystems, Intematix, UNLV, etc.

Question 5: Approach to and relevance of proposed future research

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

- Unclear about the need to work with industrial scale production of films given the advancements that are still needed. Seems a bit premature.
- Future plans build on the completed research and focus on the items not yet achieved.
- The presentation of the future work was the weakest part of the presentation and although doping clearly may provide some improvement in the lowering/tuning of the bandgap, the approach here does not appear to be very systematic and the presenter did not provide compelling justification for the approach to doping during the question and answer.
- Focus should continue on building science base and expanding scope of materials being tested.

Strengths and weaknesses

Strengths
- Innovative technology with potential for breakthrough.
- The set of partners is diverse and technology transfer between partners is excellent.
- The team has made significant progress towards the targets of the project and the DOE goals.
- The combination of synthesis and physical characterization and the correlation of physical characteristics of materials with band gap and current efficiencies is an effective strategy. A nice ground work has been laid for the synthesis of materials and characterization. Clearly progress is being made in the context of DOE goals.

Weaknesses
- Unclear how to concentrate and compress hydrogen made at low pressure spread over large areas. At this point the advantage of PEC over PV-electrolysis combo is unobvious as PV-electrolysis produces high pressure hydrogen in a relatively small unit. Potential safety, operations and maintenance issues would be difficult to resolve.
- There appears to be little progress and no future plans on one barrier: "AQ: Materials Durability".
- Future goals in terms of synthesis of new materials to further lower the bandgap was not systematically defined in the presentation. The lack of a clear plan here was made more evident in the question and answer session. Problems with the fabrication of a monolithic device were not elaborated and it is difficult to gauge the importance of being able to overcome this obstacle in the context of the overall project.
- Development of PEC is a long-term endeavor with substantial, fundamental performance improvements required to achieve DOE targets. While the MYPP Technical Plan recognizes the importance of economically viable manufacturing routes, the PEC portion of the MYPP contains no near-term manufacturing or cost targets. In light of the significant research remaining to achieve the required performance targets, engineering analyses performed by MVsystems could be premature and a distraction from the basic discovery required to achieve those targets.

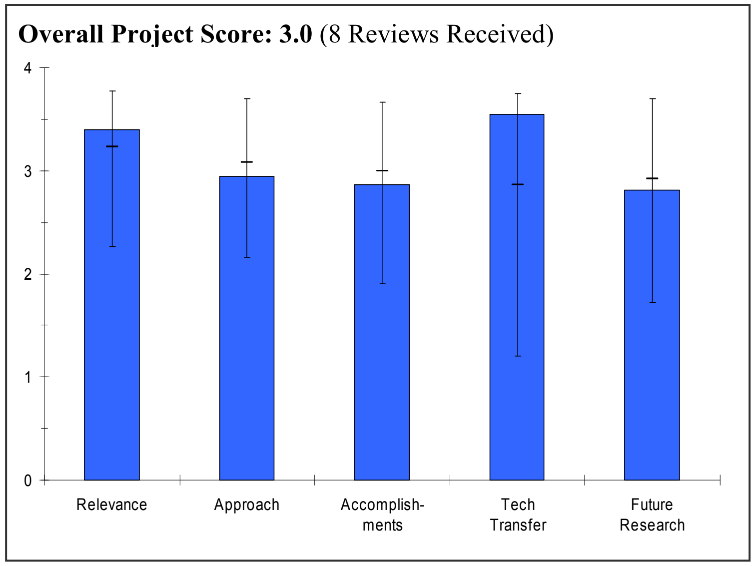
Specific recommendations and additions or deletions to the work scope

- Evaluate the feasibility of engineering a workable H_2 generation system based on this technology (including compression, safety analysis etc).
- Add to plans for future work on Materials Durability.

Project # PD-07: Renewable Electrolysis Integrated System Development and Testing

Ben Kroposki; NREL

Brief Summary of Project



This National Renewable Energy Laboratory project examines the issues with using renewable energy to produce hydrogen by electrolyzing water. Objectives are to characterize electrolyzer performance under variable input power conditions, test and evaluate the electrical interface with renewable (PV, Wind, Hydro, Geothermal, etc.) and/or hybrid/grid power (dedicated hydrogen production, electricity/hydrogen cogeneration), design and develop shared power electronics packages and controllers to reduce cost and optimize system performance, and develop and verify integrated renewable electrolysis systems (via performance modeling, simulation and testing; and addressing Safety, Codes and Standards requirements). The project will verify the DOE goals of: grid-connected electrolysis cost of $2.85/kg by 2010; and renewable hydrogen production cost of $2.75/kg by 2015.

Question 1: Relevance to overall DOE objectives

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

- Project examines pathways for renewable hydrogen generation which would provide clean, renewable, domestic energy production.
- The project directly addresses system integration issues of electrolyzers (both PEM and alkaline) to renewable power (wind). Project will answer an important question-how will electrolyzers perform under variable input power.
- The project will establish the viability of using wind resources to produce hydrogen, a key near-term opportunity for renewable hydrogen production.
- The program is focused on developing Renewable Energy - Electrolysis which is an important component in the future hydrogen economy.
- It is relevant in that it addresses integration of renewable energy/electricity with hydrogen production (electrolyzers). I recommend that this project focus to a greater extent on the hydrogen production aspect of electrolysis using renewable electricity and the technical issues therewith.
- The project appears to focus on a critical element of the overall H_2 program.

Question 2: Approach to performing the research and development

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

- Project is generally well thought out, but needs sharper focus on identifying opportunities for improved system efficiency and cost reduction. Focus should be on squeezing cost out of the system.
- Project needs more definition of out year targets, milestones, and direction.
- Much of the work in this project could have been done by industry, particularly system integration and component development. NREL functions as a test bed site, and should do analysis only. Project suggests several integration scenarios but given limited numbers of electrolysis systems should focus and obtain data.
- The focus on a system approach is critical to understanding and assessing the technical and economic viability of wind.

- Modeling capabilities support engineering work.
- Incorporates a variety of strategies (e.g., using H_2 for energy storage, integrating power electronics, etc.) to lower system cost.
- Addressing key aspects of power electronics for integration of wind electricity to electrolyzers.
- Using grid electrolysis as a baseline against which other configurations can be measured seems like a rational approach, so long as the research focus remains on the issues that are unique to wind and solar. Focus should be on maximizing electrolyzer efficiency under the conditions imposed by renewable electricity. Collaborative approach seems to be used effectively. The most important issue this project may face is limiting the scope to focus on the key technical barriers and issues. Integration of analysis, research, and demonstration is apparent.
- Water treatment and electrolyzer cooling were not addressed.
- The use of grid power in the experimental process is necessary and understandable; however, the issues driving the project to do so should be identified and addressed - either as part of this effort or in a separate effort. Since wind and solar are intermittent energy sources, analyses should consider dual use and partial use in investment recovery modeling.
- It appears that "off-peak" H_2 production and "on-peak" electricity generation might be commercially viable - this may be further explored, if appropriate.
- Some aspects of the work presented did not have any significance in solving key technical barriers such as HUG formation.
- No approaches to address cost reduction and efficiency improvement of the electrolyzer system.

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

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

- Technical accomplishments appear modest 2.5 years into the project. With exception of some economic analysis, participation in HUG, and data showing need for more appropriate power electronics - not much else is indicated.
- Results to date not impressive given the investment. It is unclear what the project end date is. HUG connection useful, but not important to technical accomplishments.
- Involvement in supporting the "HUG," while not a "technical" accomplishment will be important to getting utility input and sharing results with these important potential end users.
- It is unclear what progress has been made over the past year, but the project seems to be moving along and producing a lot of results on a limited budget.
- Good results on wind-electrolysis economic analysis.
- Good progress on identifying power electronics as a key enabling technology and defining a development plan to achieve greater efficiency.
- This project has determined the electricity price required to make a renewable electrolysis hydrogen production system competitive. This electricity price is below the selling price of electricity in most parts of the country - thus, if correct, it is unlikely that electricity would be applied toward hydrogen production unless there are other drivers besides economics. Project has succeeded in identifying and improving understanding of electrolytic hydrogen production issues, but the information presented was insufficient to effectively evaluate the technology being developed.
- Instability and variability in the energy source appear to be a critical barrier to commercialization. These must be addressed satisfactorily before renewable energy is commercialized in this application.
- Good work on analyzing the cost of H_2 production from distributed and centralized H2 production via wind.
- Using wind turbine simulator to understand the current output from wind turbine at low and high wind speed is an excellent tool.
- However, the simulator did not address continuous variable wind turbine speed in the real world, which was the objective of this work. A simulated wind turbine speed profile should be developed for 8760 hours to understand the intermittent nature of the wind power.

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

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

- Project has coordination mechanisms (HUG) and is working with a variety of partners.
- Coordination mechanisms appear sound.
- Project does have a high level of industry cost share.
- The project has lots of collaborations with various relevant partners; however, if Xcel is only CRADA partner (that puts in money), it is questionable how much information may be shared with other partners.
- Good collaboration with utilities, electrolyzer manufacturers, and utilities.
- Good leveraging of resources within NREL and partnering with universities gives the project good value for the DOE investment.
- Continuing involvement with HUG will be useful to transfer results to utilities.
- Good collaborations with industry and utility groups.
- There is obviously a good amount of effective collaboration going on. Having utilities involved is critical to technology transfer and to developing technologies that will be commercially relevant. The value of the university collaborations was not apparent from the presentation. The number of collaborators should be limited to a number that can be optimally utilized to meet the project's objectives.
- The PI needs to collaborate with modern power electronic companies.
- Outstanding job of collaboration.
- Listed many partners in this project, but it was not clear what contributions some of the partners made towards the success of the project.

Question 5: Approach to and relevance of proposed future research

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

- Project does not have clear end date nor established out year activities and specific targets.
- Project duration is too long (7+ years) and appears open ended.
- No apparent off ramps exist nor go/no go milestones.
- Again, milestones are not that impressive given the investment.
- Good plan for building on past work to design and test a larger, improved system.
- Good plan to move to real-world testing of wind-electrolysis.
- Schedule seems very conservative -- scale-up to MW scale should be possible earlier than FY 2008-2010.
- Testing will validate or invalidate the analysis results.
- The PI should include all systems needed for wind electrolysis and add a power electronics company. The approach should be redone to include water cooling battery management system.
- Future work might also include comments above.
- Did not address capital cost and efficiency of the electrolyzer system, which are the two barriers listed for the project.

Strengths and weaknesses

Strengths
- Project is clearly aligned with President's Hydrogen Fuel Initiative and MYPP.
- Wind-powered hydrogen production is probably the best near-term renewable hydrogen generation option.
- Project can evaluate system integration issues for wind power/electrolysis and the effect of variable input power. Good number of collaborations but unclear how much information will be shared. NREL can provide good analysis support to industry.
- Combines engineering and modeling at the system level.
- Good collaboration with industry and end users.
- Excellent collaborations both formal and informal.
- Effective collaborations. Seemingly strong contributions to advancing renewable energy technology and its role in hydrogen production. Research is supported by analysis.

- Wind electrolysis is very relevant. Wind should be a major source of renewable H_2.
- PI did an excellent job in the presentation of the project. This effort is clearly in line with our nation's overall long term goals for energy.
- Using simulation tool to analyze wind power output without relying on the availability of the wind speeds up the whole analysis process

Weaknesses
- Technical accomplishments are modest at this point.
- Task should be more sharply focused on improving efficiency and lowering costs in stages between renewable energy production and production of hydrogen via electrolysis.
- Effort does some things industry should do (or, could do) alone.
- Since the project covers so much ground, it is difficult to get a clear idea of what the project was focused on over the past year. Suggest including in the presentation the specific problems that the project is trying to address.
- The project presentation did not clearly communicate what had been learned and accomplished over the past year. This would be useful.
- Schedule is not aggressive enough.
- The scope of the project as presented seems too broad.
- The approach should include all systems needed to produce hydrogen. Water cooling and battery management should be included.
- No significant weaknesses were identified. It does seem, however, that more focus on the commercial viability seems appropriate. Wind is a variable source of energy – what can be done to better utilize it with these inherent limitations?
- Project did not address the efficiency of the system and how to improve it.
- Project did not address the cost of the system except DC-DC converter.
- Economic analysis of centralized H_2 production should include transportation and storage cost for a fair comparison to distributed production.

Specific recommendations and additions or deletions to the work scope

- Focus attention on one, maybe two, integration scenarios so they can get to the data collection and system analysis.
- Examine accelerating the schedule.
- Establish formal links to other wind/H_2 programs based on existing informal contacts.
- Need to maintain clear focus on specific technical issues directly relevant to producing hydrogen with renewable electricity. Any issues that are not unique to hydrogen production from renewables should be excluded (if not done so already) from this project's scope, and supporting/coordinating tasks should be limited to those that facilitate progress in the research.
- Add collaborations, such as large electrolysis unit producer and modern power electronics developer.
- As indicated above, an assessment of (to include technical as well as economic modeling) "off-peak" H_2 production and "on-peak" electric generation might be considered.
- Cost of electricity should be quantified under "Grid Connected Mode" for the process of wind electricity --> H_2 --> compression/storage --> H_2 ICE --> electricity to determine if this pathway makes sense.
- Develop a wind turbine speed profile for a complete 8760 hours of the year.

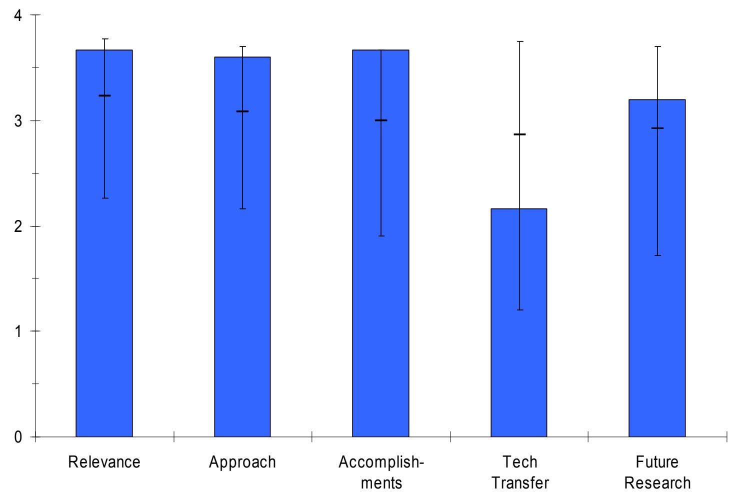
Project # PD-08: Advanced Alkaline Electrolysis

Richard Bourgeois; GE Global Res.

Brief Summary of Project

The GE Global Research hydrogen production team is researching methods to achieve considerable reduction in alkaline electrolyzer system costs compared to prevailing prices of available new equipment. They will do this through technological advances in production methods and materials of construction. Appropriate physics-based models will be used to optimize the system for practical performance at low cost. Cell stack 2010 targets include 76% efficiency and $0.39/kg contribution to hydrogen cost from capital expense.

Overall Project Score: 3.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project makes use of existing or near term technology.
- The technology being developed produces hydrogen without the (on-site) release of carbon dioxide.
- The emphasis in the project is on development of an economical, near-term commercial product.
- The PI and his company have the background to develop and market the electrolyzers described.
- Project is addressing efficiency and cost targets for electrolysis systems, which will be critical to cost-competitive renewable hydrogen production.
- Project addresses capital cost of electrolysis systems for distributed hydrogen production. Project uses innovative GE technologies to reduce capital cost to attain (projected) hydrogen production cost goal of $2.85/kg. Did not have as a goal the demonstration of realistic size systems for hydrogen production. Project does not (adequately) address delivery of pressurized hydrogen.
- Improvements in the technology for electrode manufacture demonstrated in this talk show good progress towards meeting the President's goals of producing hydrogen efficiently using alkaline electrolysis.

Question 2: Approach to performing the research and development

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

- This is a relatively short-duration (21 mo) first phase of the project, limiting the amount of long-term testing that could have been performed.
- The design and testing has the strength of being a customer-driven approach.
- The planning for the project is evidently part of a larger effort on the part of GE and appears to be well-thought out.
- Good mix of fundamental, computational, and experimental work.
- Addressed market issues up front (customer interactions).
- Applied core GE capabilities and expertise from other sectors to solving materials issues.
- Good use of GE coating technology for catalyst deposition and low cost manufacturing of plastic parts to address capital cost reduction. In addition to electricity costs and O&M costs, reducing capital cost of electrolysis is a key to reaching hydrogen cost goals. Because there is so much proprietary information about materials and designs in this project, this cannot be evaluated.

- Commercial approach that identified customer and his needs is appropriate. Path to reduce costs with the greatest impact is sound. Taking advantage of GE's special expertise to reduce costs was well done. No mention of the safety aspects of high pressure operation. No numbers that indicated the cost savings expected to be realized by the new technologies. Effect on costs would have added to this presentation.
- Excellent and systematic approaches in addressing the capital cost of electrolysis system.
- The project did not mention anything on renewable integration though it was listed as one barrier to be addressed within the project.

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

This project was rated **3.7** based on accomplishments.

- It is very difficult to judge the technical accomplishments of the project because much of the relevant data is proprietary. Many of the graphs contained either units or numerical values, but not both. Other graphs were dismissed as only being representative and worse than the actual performance of the devices under test.
- Qualitatively, the data and photos of the apparatus appear to shown reasonable technical progress.
- Met technology targets.
- Proposal submitted to scale up to larger unit.
- Project went beyond stated goals, going to a 1kg/hr system that is 100x the size of the benchtop system goal. It is impressive that they have tested cells up to 40k hours with no failures. Nice CFD analysis relating decreased current density to hydrogen bubble build up in cells. Also, optimization codes would allow you to select optimal equipment sizes for all the pieces for a given application.
- Use of proprietary expertise greatly aided the success of this project. Efficiency goal for 2010 appears to have been met. Long term data is necessary for final assessment and these tests are in progress.

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

This project was rated **2.2** for technology transfer and collaboration.

- So much of the quantitative information shown in this presentation is being held as proprietary that the project may only benefit GE.
- Though a collaboration with SUNY Albany is listed, there is no other indication of technology transfer or industrial collaborations.
- This is an industry-led project with a university partner.
- There were no collaborators on this project and obviously no intention to do technology transfer.
- Technology Transfer/Collaborations. N/A.
- The project should have more collaboration with potential customers.
- Additional partner in the renewable/utility field will add value in the project.

Question 5: Approach to and relevance of proposed future research

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

- The project has concluded and, from the proprietary information withheld from this presentation, any continuation could be expected to be even more proprietary.
- Proposal to continue with next phase.
- Identified materials have potential to meet targets.
- Experimental system is flexible enough for a range of operations.
- Project is complete. Not many details on future work proposal. GE proposes continuation of work with 1 kg/hr system.
- The scale up plan appears reasonable. The power park concept for three sets of customers is sound. Additional data on electrode performance is needed and is in the plan. No mention was made of the safety aspects of operating an alkaline electrolysis unit at high pressures. This issue was discussed in more detail in another talk.

Strengths and weaknesses

Strengths

- The CFD modeling of cells provides a useful optimization tool. There was not indication that this CFD tool would be any benefit to the wider hydrogen production community.
- Balanced approach.
- Good corporate approach to project-leveraged GE technologies in coating technology from aircraft industry and low cost manufacturing from plastics. Both technologies are important in developing mass production protocols leading to capital cost reduction.
- The major strength in this work is recognizing the application of established technologies in GE's portfolio to the technical challenges associated with alkaline electrolysis.
- This project addresses the largest need for both renewable hydrogen and hydrogen economy participation by the power industry.
- Excellent approach from market analysis, system design, cell test to bench scale testing sets a pathway to success.

Weaknesses

- Since the presentation reveals few of the operating parameters and efficiencies, it is very difficult to judge progress.
- Project did not address direct electrochemical pressurization. Project will provide little benefit to other industry developing electrolyzer technology.
- Renewable integration was not addressed.

Specific recommendations and additions or deletions to the work scope

- It is very difficult to judge the strength of the project without access to at least the I-V curves of the cells. While the general, qualitative approach seems to be sound, this reviewer must leave overall judgment of the projects progress to the DOE program managers, who presumably have access to the proprietary data.
- Continue on to larger system evaluation and long-term testing and analysis. Show pressurization potential.
- Move program forward.
- Add renewable integration into next phase analysis.

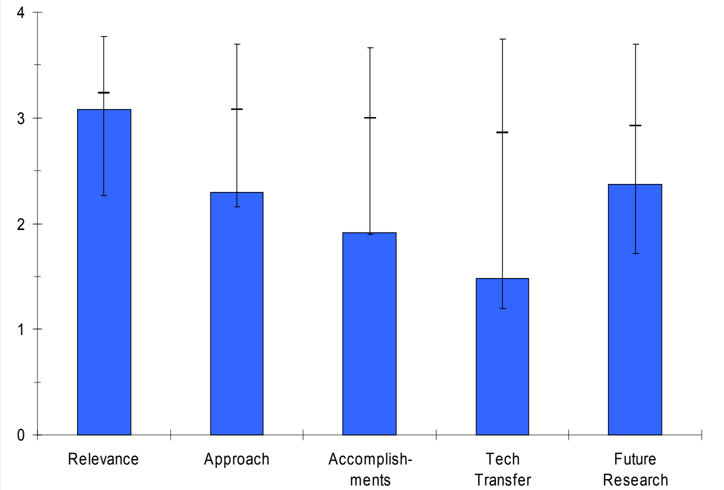
Project # PD-09: Alkaline, High Pressure Electrolysis

Samir Ibrahim; Teledyne

Brief Summary of Project

The goal of this Teledyne Energy Systems Inc. project is to advance water electrolysis technology and develop an electrolytic hydrogen generator, designed for production of >10,000 units per year, with the following features: hydrogen delivery at high-pressure (5,000 psig); relatively inexpensive hydrogen generation and pressurization; production capacity of 10,000 scfd; high conversion efficiency; and reliability and durability with low maintenance cost.

Overall Project Score: 2.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Project had as an original goal the production of high pressure hydrogen but switched to reduced capital cost through higher volume manufacturing. Both would lower the cost of (pressurized) hydrogen produced. With current technology it will not be possible to reach DOE cost objectives. Current projected cost is $4.75/gge H_2.
- The work in this project supports the President's Hydrogen Fuel Initiative when it is conducted. However the interruption due to Katrina (which ended only three weeks ago) and the change in project direction have resulted in relatively little progress.
- The work seems to be guided more by Teledyne internal business priorities and opportunities than by overall support of the Hydrogen Fuel Initiative.
- Efficiency is no longer a focus of this project. Reducing capital costs is relevant to reducing cost of hydrogen via electrolysis, but analysis is needed to determine whether capital cost reduction alone will be sufficient meet DOE costs (may be a task of DOE, not this project).
- Cost-effective electrolytic hydrogen production is a candidate for hydrogen production in the transition and in the longer term.

Question 2: Approach to performing the research and development

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

- TESI seeks to develop electrolyzer technology very similar to current marketed technology that will deliver hydrogen at 500 psi. Project will investigate this benchtop technology.
- Teledyne has an extensive product line in low and moderate pressure electrolyzers and they have been using the relevant economic analysis tools in evaluating new designs.
- The project was originally funded for the development of high pressure electrolyzers with the hopes of improved efficiency and lower capital costs. The change in project direction to low pressure electrolysis and post-electrolysis compression means that the project is essentially testing Teledyne's existing product line.
- It was not clear from the presentation that government funding is appropriate for this project which is highly focused on future sales for Teledyne. A positive aspect of the approach is the flexibility to change the research focus as lessons are learned.
- The project fails to conduct analysis in the early stage of the project to understand the issues with high pressure system. Too much money and time were spent before a decision to change the direction to a low pressure system.

- Very little information was provided on the approach for achieving the new objective: complete design of a low cost, 150psi alkaline generator using DFMA.
- The PI needs to make clear what technical barriers are being addressed so that this is not perceived as merely incremental improvements to existing Teledyne equipment.

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

This project was rated **1.9** based on accomplishments.

- Project interrupted to focus on supplying hydrogen generators for industry in the area affected by Katrina. TESI completed hardware cost and safety analyses, and benchtop testing of a three-cell, 500 psi benchtop prototype. They concluded that going to higher pressure systems was not economical because of higher cost of materials (i.e., thicker walled, heavier systems) and other safety-related needs.
- Because of the interruption due to Katrina and because of the change in project direction to lower pressure, relatively little technical progress has been made during the past year.
- The work in FY-05 on the pressure control system has been largely obviated through the change to more conventional low pressure operation.
- Not much work was done due to work stoppage following Hurricane Katrina. This is not a reflection of the quality of the work, but rather a business decision. Very little DOE money was spent in the last year. Funding should be continued if focus can be on increasing the state of the art in electrolysis and progressing toward DOE targets. Timeline may need to be extended.
- No significant accomplishment in this project. Cost and safety issues which result in the change of research direction should be realized without spending significant money and time.
- The main "progress" reported was that the project determined that high-pressure electrolysis is economically infeasible and is re-directed towards 150psi electrolysis. Progress towards the DOE goal has not yet been achieved.
- Showing something doesn't work can be valuable.

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

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

- TESI originally had partners in compressors and ac-to-dc converters but presently has none.
- The change to lower pressure operations has meant the loss of previously-arranged collaborations.
- There seem to be no efforts to attract industrial or universities collaborators.
- There are no collaborations. Teledyne will be able to use its accomplishments to commercialize its electrolysis products, but it is not clear that sufficient knowledge is being generated that can be used outside of Teledyne. Teledyne should explore any work that is being done at the national labs that might be relevant to reducing hydrogen production costs, if it has not already done so.
- Project stopped and restarted with new scope. Therefore, no partners were identified.
- The project has no partners and little apparent collaboration with other institutions.
- No publications of data/results.

Question 5: Approach to and relevance of proposed future research

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

- Future efforts are focused on testing a benchtop prototype delivering hydrogen at 500 psi and also a lower pressure system (which TESI will fund independently).
- The presentation noted that the original contract scope is being closed out. It is doubtful whether the new contract objectives are sufficiently beyond the present state of the art to warrant further funding.
- Given the current result of the project and change of direction, it is questionable if future planned work will be able to address cost and efficiency barriers.
- The project needs to improve its focus on achieving DOE goals and reporting results.
- It is not clear what the schedule is for future work.

Strengths and weaknesses

Strengths

- TESI provides insight into business-related considerations of pressurization. Participants have decided that because of safety-related design considerations that they will not pursue high pressure systems (at least greater that 500 psi).
- Teledyne has extensive experience in the manufacture of conventional electrolyzers.
- The work, particularly in FY-05 and early 06 showed that the added capital costs of high pressure operation outweigh the benefits of lower compression requirements.
- Research focus is based on lessons learned.
- New scope of project may contribute to the DOE goal of low-cost electrolysis.
- Project addresses a real need for development. Project addressed a potential path that proved non-viable - good work.

Weaknesses

- Not much innovation in this work.
- The project seems to be a relatively low priority with Teledyne Energy Systems. The decision to abandon the primary objective of higher pressure operation was apparently a business decision. The nine-month curtailment of work due to Katrina was apparently not due to damage to their facilities.
- Not clear what this project is contributing to DOE program going forward.
- Poor planning in project approach which results in objective change after significant money and time were spent.
- More information on specific barriers being addressed, approach to resolving the barriers, and associated accomplishments (supported by data) needs to be in next year's review.
- PI has little progress to show for effort.

Specific recommendations and additions or deletions to the work scope

- Funding should not be continued for this project. DOE funding will not assist TESI in accomplishing anything they would not already do in developing and selling commercial products.
- DOE management should carefully consider whether the reduced benefits of the new project objectives warrant continued funding.
- Need to re-evaluate the new scope to make sure that future work is properly planned to address barriers without repeating the current failure.
- The approach to development should be fully planned with low cost as the emphasis.

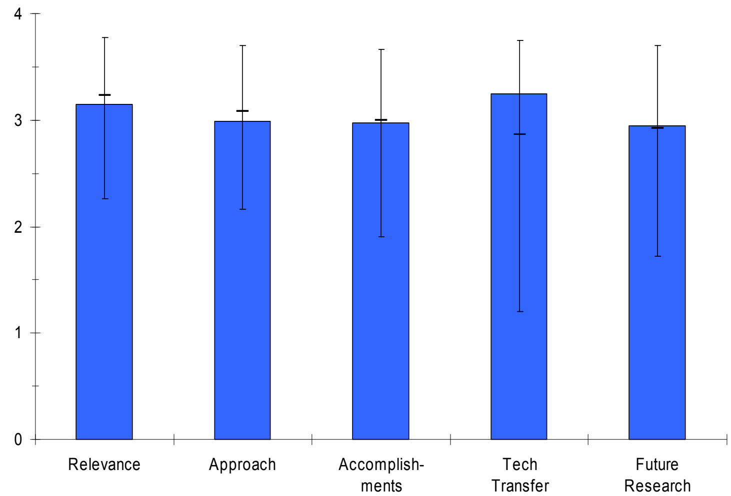
Project # PD-10: Development of Solar-powered Thermochemical Production of Hydrogen from Water

Alan Weimer; U of Colorado

Brief Summary of Project

The objectives of this project are to: 1) Identify a cost competitive solar-powered water splitting process for hydrogen production; 2) Continue experimental cycle studies needed for final quantitative selection; 3) Evaluate numerical and experimental solid particle receiver performance; and 4) Optimize heliostat/tower/secondary concentrator characteristics and configurations for various operating temperatures. 353 unique cycles have been found in the literature and scored with 12 found to be worthy of further experimental study: 5 are currently under active study.

Overall Project Score: 3.0 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Presentation indicated success requires significant advances in multiple technologies. There was no indication provided of how the time frame to achieve success fits into the DOE 2010, 2015 go-no-go milestones.
- The funding level appears high for the level of effort.
- Much of the evaluation work had been done previously for High Temperature Nuclear Reactors; although temperatures here are somewhat higher.
- Production of hydrogen in this manner could provide an important future source of renewable hydrogen in certain geographical areas.
- There is still a lot of uncertainty over whether this has a chance of being cost-competitive with other renewable hydrogen production methods.
- This project addresses four barriers in the DOE's multiyear research plan. Those barriers are: high-temperature thermochemical technology, high-temperature robust materials, concentrated solar energy, and coupling of solar energy and thermochemical cycles.
- The project supports the production of hydrogen without the emission of carbon dioxide, except through the production of construction materials.
- The project offers an alternative to nuclear-thermal hydrogen production. This may become more acceptable to consumers by being renewable-based.
- The project addresses four of the identified barriers for the hydrogen initiative, and the work plan is clearly designed to reflect the relative contribution of each component towards a specific barrier.
- Relevance to to the hydrogen program is outstanding. Would suggest, however, that the project team also consider the potential commercial aspects of the technologies in their assessment of cycle feasibility.

Question 2: Approach to performing the research and development

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

- The cycle screening process utilized may have been more comprehensive and costly than warrented.
- Downselection: literature review and scoring is appropriate.

- Materials issues (corrosivity, temperature shocks, maximum allowable temperatures, etc.) and materials transport issues need to be explored.
- Systematic approach to evaluating cycles and use of existing literature is good.
- It is good that the project is now focusing more on testing candidate cycles and cycle classes.
- It would be interesting to see how the "CR5 Thermochemical Engine" works with some of the better candidate materials.
- The approach builds on the previous screening of potential cycles and has focused on a few cycles according to engineering and thermodynamic considerations.
- It is beneficial to the whole program that the sulfate cycles were investigated and eliminated due to side reactions.
- The experimental down-selection approach appears to be sound, but the presentation appeared to show snapshots. For example, why is the zinc cycle shown in detail, including details both for the zinc decomposition step and the hydrogen generation step, while the manganese cycle shows only a manganese reduction step? The approach would be more understandable if a summary slide showing these five candidates in parallel were given.
- The project approach is logical and clearly-presented, from the initial process of downselection of known thermochemical cycles to a limited set of cycles to carry forward to experimental evaluation and validation. The cycles under current study represent a good cross-section of potential technologies, and good justification was presented for elimination of certain cycle categories (such as metal sulfates) due to unfeasibility. The specific experimental parameters for each of the cycles were clearly presented, with good description of the potential for future development.
- The approach and processes seem to address barriers well. One possible improvement seems to be in the materials area. It may be that the presentation time limitations precluded any appreciable discussion on the subject but the high-temperature materials area seemed somewhat lacking in R&D.

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

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

- Progress seems slow, probably because there are so many options.
- Literature review, although repetitive of other efforts in this field, is an appropriate screening tool.
- Multiple teams working on the project are focusing on specific potential cycles.
- The project is now reporting experimental data which will add to the knowledge base.
- Evaluated over 300 possible thermochemical cycles and selected 12 cycles for further experiments. Excellent work to eliminate the metal sulfate cycles. Very good CFD and simulation studies.
- The project has a useful range of tasks, from the bench-scale fundamental chemistry to the systems engineering of a production plant.
- Apparently most of the technical progress to date has been in the evaluation and choice of candidate cycles.
- Most of the work is too preliminary to judge the overall efficiency or economics.
- It is difficult to determine where this project will be when it ends. Will they be down-selecting to one system? What are the criteria for selection? Has any analysis been done to show the trade-offs, for instance, between reaction temperature and material costs?
- Many of the cycles had progressed beyond initial proof of concept and the future work was well-delineated for each cycle. The investigators have a clear concept of critical testable components for each downselected cycle, and gave convincing arguments for the advantages and disadvantages of each cycle. The investigators did not discuss efficiency or cost for all the cycles tested, so this represents an unfinished component of their work plan.
- Accomplishments are outstanding. Quite impressive work and results appear to be quite salient to overall H_2 program.

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

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

- While collaboration is good in principle the PI needs to drive downselection or else everything will be studied forever.

- Large number of partners working on various cycles.
- The project is using a mix of universities, labs, and industry to conduct work, according to their particular areas of expertise and research capabilities.
- Excellent partnership with UNLV, SNL, ANL, NREL, GA, and ETH-Zurich. Not clear about collaboration with General Motors and General Electric.
- There are a wide range of partners in this project, several of whom have extensive experience with surveys of potential thermochemical hydrogen production cycles.
- There are certainly enough partners; the overall relationship, however, is unclear. Who is leading the project?
- The project involves substantial interactions with partners in academia, industry, and national labs; international collaborations and partnerships are also included. The investigators clearly have the capability to initiate and maintain robust working partnerships. The investigators did not comment on whether the work had led to any patents.
- Outstanding collaborative effort. It appears that different partners are in fact focused on different cycles or segments of the overall effort. The presentation did not clearly describe how the different activities were integrated. Presentation time limitations may have precluded this level of detail.

Question 5: Approach to and relevance of proposed future research

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

- The PI didn't express a great drive to down-select to 1 or 2 candidate cycles and start to see what the other problems are in actually executing a specific concept.
- Good plan to down-select cycles.
- Planned cost estimation needs to appropriately incorporate temperature cycling and special materials issues.
- Glad to see that using H2A process economics is in the future work plan.
- Reasonable plan for future work.
- Future work should focus on testing materials compatibility, particularly in the high temperature regions.
- Additional evaluations should be made of overall resource requirements (e.g., zinc), with the inclusion of realistic material loss rates.
- Future systems analysis should include the potential for oxygen use.
- The plan itself appears sound. However, there seems to be a lot to do.
- The future work plan is very clearly defined, with the final stages to close the cycle analysis for each of the five downselected cycles very explicitly spelled out. The investigators have a very good idea of potential problems – no contingencies were presented, as presumably the completion of evaluation of all five cycles will result in a single preferred closed cycle. The investigators need to complete the economic cost analysis and to complete running their data through the H2A model.
- Future plans appear to be satisfactory. In addition to H2A economic assessments, it is suggested that economic modeling consistent with realistic commercial deployment scenarios be considered.
- Additionally, since the Cu-Cl cycle seems feasible at lower temperatures (few high-temperature material barriers) further assessment of this cycle appears prudent.

Strengths and weaknesses

Strengths

- Good people working in an area often produce some amazing technology, usually unrelated to the specific project goals. There are enough different activities that some valuable technology will evolve.
- Lots of fundamental knowledge is being generated by experts in the field.
- Excellent team.
- The PI has eliminated several cycles through new experimental data.
- The project combines several facets, from the fundamental chemistry to the integrated systems engineering.
- The individual experiments, storage study, solar receiver work all seem good.
- There seems to be good progress toward identifying material systems.
- Very impressive collection of expertise in the assembled university, national lab, industrial and international consortium for this project.

- Clear understanding of scientific issues and interpretations of cycle evaluations.
- Broad and comprehensive coverage of potential thermochemical cycles, with good justification for downselection.
- Very robust data collection and high resolution analysis.
- Very good justification for the need for experimental validation of thermodynamically-predicted cycles.
- Project appears to objectively assessing the thermochemical cycles. Approach is good and the collaborative effort appears to be outstanding.

Weaknesses
- Management structure is very unclear - how are decisions made for down-selections?
- Too much focus on theory versus experimental results and systems analysis.
- Significant material challenges.
- The overall needs for materials of construction need to be evaluated against global resource constraints and the greenhouse gas emissions due to manufacture.
- If this is a three year project, perhaps they should have concentrated on one aspect – such as materials identification. A final result in one area is more valuable than partial results in three.
- Investigators did not clearly define the method by which they would select their final cycle to scale up to production scale.
- Incomplete description of decision support tools for optimization of design, size and performance of the heliostat reactors.
- More emphasis should be placed on lower temperature processes. At least at this stage, the high-temperature processes are faced with materials barriers and although these barriers may be overcome with further R&D, resources might well be applied on the promising lower temperature processes.

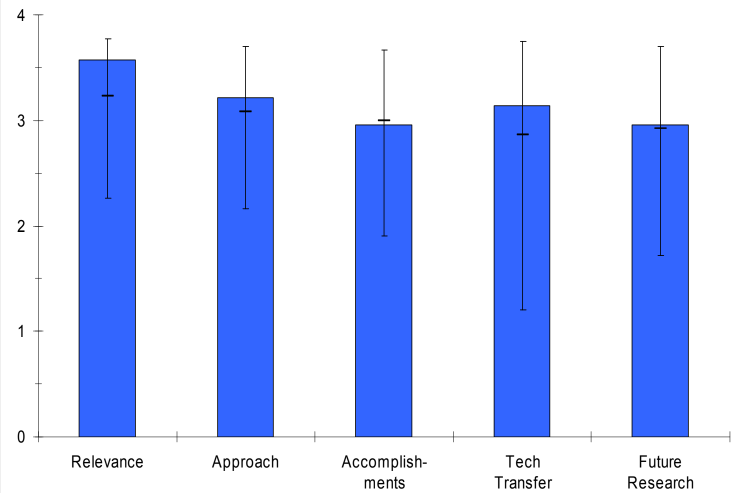
Specific recommendations and additions or deletions to the work scope

- Force a down-select to 1 or 2 cycles ASAP and try to move toward a small scale demo. Even if it isn't 100% successful it will uncover new issues that should be addressed.
- Put more effort on solving the materials challenges.
- The Program needs to allow the researchers to determine what the top one or two systems are and run them through in-depth testing. A successful hydrogen generation process using solar thermal processing could be most valuable.
- Excellent overall project, however, very little was presented on the lower temperature Cu-Cl cycle. I recommend that additional resources be targeted on this low temperature cycle.

Project # PD-11: Hydrogen Delivery Infrastructure Options Analysis

Bruce Kelly; Nexant Inc.

Overall Project Score: 3.1 (7 Reviews Received)



Brief Summary of Project

The objectives of this project are to: 1) Refine technical and cost data in H2A Component and Scenario Models to incorporate additional industrial input and evolving technology improvements; 2) Explore new options to reduce hydrogen delivery cost; 3) Expand H2A Component and Scenario Models to include new options; and 4) Provide bases to recommend hydrogen delivery strategies.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The project attempts to analyze and answer key questions.
- Analysis vital to identify gaps and guide delivery R&D.
- Need to harmonize different tools.
- This is important work and the only project addressing the delivery aspect of the hydrogen economy.
- A rigorous and structured analysis of hydrogen infrastructure options is an underappreciated but key component of understanding what will be needed to bring hydrogen to a large retail market.
- Very relevant to DOE's hydrogen strategy. Emphasis on pipelines is most welcome.

Question 2: Approach to performing the research and development

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

- The technical barriers associated with the challenges of converting hydrocarbon pipelines to hydrogen or hydrogen blends is not properly covered. Risk assumption factors, risk analysis, and practical operation (including leakage of H_2) of the transportation system is not adequately addressed. The study is to high level and theoretical and lacks or failed to convey the necessary inclusion of practical and realistic design and operation factors particularly for pipelines.
- Work with industry to validate H2A inputs is good.
- Greenhouse gas and criteria emissions will be good addition to existing models.
- The PI is using sound thought processes and good judgment to eliminate options and focus on others.
- Wide range of hydrogen delivery options analyzed in great detail using appropriate tools and information sources.
- Lack of depth in some analysis areas: reforming of liquid feedstocks; local pipeline issues; impact of pipelines on optimizing forecourt storage needs.

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

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

- The results do not seem to present new information beyond what was currently available for most options covered. No substantive conclusions seemed to be presented that would facilitate actions or decisions in the near term or long-term.
- Good summary of pipeline issues.

- Pipeline storage results encouraging.
- Not a lot of results yet, but reasonable progress for first year.
- Expect significant progress in next year of project.
- The PI has made good progress toward his goal and in answering the overriding question of which delivery methods we should focus on and in which to invest in eventually.
- Some progress in identifying potential alternatives to onsite storage (e.g. utilize capacity of oversized transmission line). Disappointing understanding of costs and limitations of the liquid hydrocarbon carrier options (no consideration of reactor size, cost, feedstock cost, etc.).
- Good new information on pipeline conversion / constructions. Some of the conclusions are a bit simplistic (i.e. that all liquid products pipelines will be available for conversion, this is most likely not the case due to mechanical issues for old pipelines).

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

This project was rated **3.1** for technology transfer and collaboration.

- There is a good team in theory, however the presenter gives the impression that their skill sets were not fully utilized and that their input was highly compartmentalized to single questions. There is nothing in the presentation to indicate that the pathways selected are accepted or endorsed by industry.
- Partners are a good mix of industry and national labs.
- Good incorporation with H2A, TIAX and other existing efforts.
- There is good involvement from both government and commercial institutions.
- An important element of this work is gathering input from relevant industries and organizations to evaluate a large number of different delivery pathways. Nexant clearly worked hard with these partners to get this input and integrate it into their analysis.
- Broader set of collaborators would be desirable (especially someone from Engineering and Construction industry with experience in hydrogen pipelines).

Question 5: Approach to and relevance of proposed future research

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

- Proposed future work appears to be duplicating work already completed, or ongoing with other entities. Given the degree to which the available information was overlooked or ignored in the current analysis it is not clear if the performers have an understanding on how to incorporate it when evaluating the suitability of existing infrastructure to convert to hydrogen.
- Proposed future work closes gaps in current program.
- Project plan looks good.
- Expecting lots of results next year.
- Scope of future work appears to be excessively broad. Since delivery infrastructure is so localized, emphasis on one or two specific cases and issues related to these cases would have been very welcome. Also would be good to add an element looking at permitting and societal acceptance of various hydrogen distribution methods, especially pipelines.

Strengths and weaknesses

Strengths
- Project draws upon existing technology and data.
- Integration of various analysis tools with resulting improvements to individual tools.
- The presentation was informative and easy to follow. The PI is making sound judgments and presenting the results in a concise and simple manner.
- Good information on some delivery methods, especially pipelines. Comprehensive attempt to update H2A and add new options to it.

Weaknesses

- While the project scope may not include political issues of building infrastructure it has some severe disconnects between the theory and the reality when assessing the cost and viability of pipeline and other proposed or analyzed delivery options. When the researchers talk about using natural gas pipelines they only evaluate the capability; not the reliability or integrity of the lines.
- Not a lot of results yet.
- The size of the scope detracts from more detailed study of particular delivery methods. The PI should consider removing some delivery methods from the study. All economic factors have not yet been addressed, for example, cost of obtaining licenses, permits, and land access for distribution lines.
- Very skimpy info on MeOH and EtOH - info provided on these technologies does not appear to realistically reflect reforming capital costs. Lack of consideration on pipeline permitting aspects.

Specific recommendations and additions or deletions to the work scope

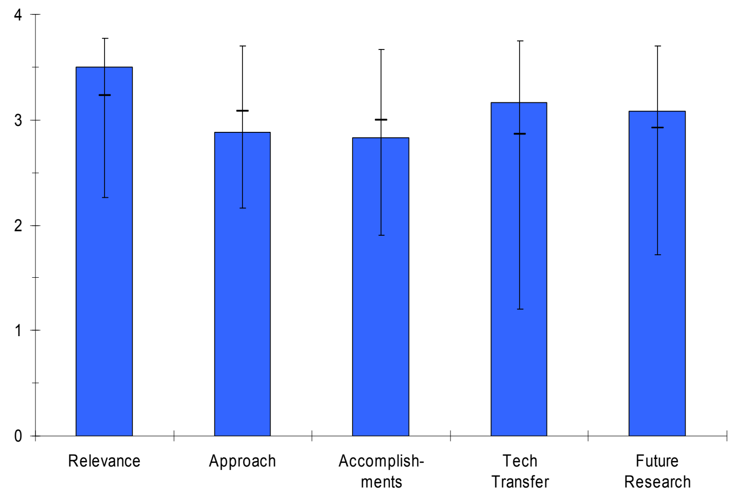
- The project needs to work with industry to validate assumptions and confirm partners who are willing to implement the pathways suggested.
- Title a little confusing. "Options analysis" has a meaning all its own. Would be better described as "Analysis of Options…"
- Blending gas transmission is a good option to remove. Evidence shows that it is one of the least practical methods.
- Recommend adding CO_2 pipeline options to the scope of work (for CO_2 sequestration).

Project # PD-12: Scale-Up of Microporous Inorganic Hydrogen-Separation Membranes

Rod Judkins; ORNL

Brief Summary of Project

Overall Project Score: 3.0 (6 Reviews Received)



This Oak Ridge National Laboratory project will investigate the scale-up of microporous inorganic hydrogen separation membranes for use in coal gasification systems. The candidate membranes are based on those developed on the DOE Office of Fossil Energy Advanced Research Materials Program, which are robust, have high hydrogen flux, and high separation factors. This project will investigate the scaling factors for fabrication of approximately one-meter long tubular membranes, the effect of composition of the coal-derived synthesis gas which is the feed material to the membranes, temperature and pressure of operation, compatibility of the membranes with the synthesis gas, total gas flows, hydrogen gas flow through the membranes, the cut (fraction of total gas that passes through the membranes) of the gas, and membrane system design and configuration.

Question 1: Relevance to overall DOE objectives

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

- Cost-effective hydrogen separation technology to produce hydrogen from coal gas responds to Hydrogen Fuel Initiative RD&D scopes and proposed work satisfies that need.
- The work scope supports the FE Hydrogen from Coal Program plans and objectives.
- Supportive in the context of providing a way to recover H_2 from a coal gasification stream as well as pressurized CO_2 for potential "green" disposal.
- Not a one-step process for high-purity hydrogen.
- This project addresses the cost, durability, and flux barriers in producing hydrogen. Contradictory information provided regarding achieving required purity (for fuel cell vehicles) as 95% purity projected.
- Microporous membrane systems offer the high promise to reduce cost for H_2 separation, and are critical to the success of H_2 fuel initiative.
- The project clearly addresses four key barriers in the hydrogen production and delivery program.

Question 2: Approach to performing the research and development

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

- Reported flux data appear to be significantly lower than dense membranes.
- A seeming necessary, additional purification step would increase cost.
- H_2S may not chemically alter material properties or impact H_2 permeance, but physically can block pores; or, can contaminate the product phase (H_2~2.98 A, H_2S~3.6 A).
- CO permeation is not discussed.
- Good technology design approaches in constructing the membrane tube, particularly in the fabrication of the microporous separating layer.
- The arguments for using helium instead of hydrogen, putatively for safety reasons, were not convincing.
- Membranes should be tested in gas streams containing hydrogen, CO, CO_2, H_2S, etc not present data on He separation.

- Determining the operating conditions and performance criteria for the membranes, testing the candidate membrane materials for compatibility in operating environment, utilizing technology developed through FE Advanced Research Materials Program to fabricate 1-meter long membranes, and assembling these new membranes for testing in gasifier facilities seems sufficient to overcome anticipated barriers.
- The technical barriers related to the proposed technology were well discussed, including the use of staged separation systems.
- Specific discussions were presented on the issues related to the integration of this technology into the coal to H_2 plant.
- The investigators have a clear understanding of the relative advantages of selective membrane composition and configuration. They have done a good job in designing and scaling their metallic membrane for improved selectivity and separation of hydrogen from coal syngas.

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

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

- Some progress has been made in the past year; planned efforts for improving flux to achieve the DOE targets are not discussed.
- The permeance thru Knudsen mechanism is not commercially relevant.
- The product purity via nanoporous membrane is very encouraging.
- Some preliminary system analysis results would have been opposite.
- A 1m tubular membrane was constructed demonstrating a good He: CO_2 selectivity.
- Need to focus on obtaining separation data with real life gas mixtures (H_2 from CO_2 with some H_2S) as planned in the future.
- Good deposition of micro/nano porous membranes inside tubes Versatile fabrication technique. Good partner collaborator – Performance of membranes in CO-containing gas streams is missing.
- The project has already developed its micro-porous membrane to a near-commercial state and the partner was expected to commercialize this membrane in 2004 – lacks references to whether this effort is on-going
- Data presented showed promises, including the advancement in support tube fabrication technology which can be resistant to sulfur poisoning.
- The project has succeeded in the design and fabrication of a pilot scale amount of unsupported, selective metallic membrane composite material. Some aspects of durability and the performance under simulated syngas or using helium are questionable in terms of extrapolation to commercial scale.

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

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

- The project has identified facilities for material and performance tests including pilot scale tests.
- Very good partnership with membrane manufacturing company - DOE National Energy Technology Laboratory to validate the permeance results & providing funds for this research
- Good collaboration with five well qualified participants.
- It is unclear if any of the participants will invest in and lead the effort to commercialize the concept or if their level of participation will remain on a fee-for-service basis.
- Good plans were presented on the interactions with industrial companies to demonstrate the technology at different commercial sites with coal gasifiers in operation.
- The investigators have a successful track record in collaborating with an industrial partner, and have initiated discussions with membrane supplier and partner for phase II development.
- In relation to technical barriers, the PI stated that :"Cost - Technology is already commercial in other membrane types". The statement is unclear since no cost data for PI's membranes is provided or relevant cost comparisons with commercial membranes.

Question 5: Approach to and relevance of proposed future research

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

- Some design calculations for the PSDF and UNDEERC tests facilities are provided.
- While future work is reasonable (first dealing with the material problems in the very corrosive acid-gas environments, then evaluating the membranes under these conditions) small systems rather than 1m modules (would) be employed for preliminary flux and selectivity measurements with the "dirty" gas feeds
- During 2005 review downselection was mentioned regarding a compatible material for fabrication of long length tubes. This information was repeated during the 2006 review. It is unclear why there is such a long delay evaluating the materials compatibility issue
- The remaining four tasks to build, test, and demonstrate an industrial-scale membrane using a slipstream from the Wilsonville facility is well focused on addressing key barriers.
- A good future work plan was presented with details, including the test plan at coal gasifier sites.
- The membrane performance test-plan for bench scale units was not adequately discussed. More data are needed in this area before moving to the large scale testing.
- The future research plan is logical and clearly-stated, including analysis of junction integrity and completion of NETL's system analysis. The investigators did not discuss specific pitfalls or contingencies. The time frame for the remaining work seems to be unrealistic.

Strengths and weaknesses

Strengths
- Relevant technical info on advanced membrane separation de-classified over a year ago; it is unclear if the PI is using the knowledge to resolve the hydrogen separation flux, purity and selectivity challenges.
- Excellent team that can leverage resources well.
- The investigators have demonstrated expertise in the area of metallic membranes for selective applications.
- The investigators demonstrate a strong understanding of scale-up issues and fabrication issues, including how to ensure junction integrity of their membrane. They also describe various possibilities for the supported membrane material.

Weaknesses
- The project progress appears less than expected.
- System analysis results are not presented.
- The flux and selectivity data to date for candidate materials are lower than DOE projected commercial targets.
- Use of He instead of H_2 could, particularly with very small pored membranes, lead to misleading results.
- Systems can be engineered to perform safely with H_2.
- Uncertain whether silica membrane will withstand steam.
- The process has remained at its near-commercialization stage for several years. It is incumbent upon the PI to move the project along and complete it successfully.
- PI might consider establishing an in-house capability to test membrane performance for $H_2/CO/CO_2$/steam separations which can provide guidelines for future membrane performance improvements.
- The potential to use this membrane for water-gas-shift reactor applications should be explored.
- The use of the simulated shift syngas may not accurately reflect performance of the membrane with respect to different levels of CO.
- The investigators did not present a cost analysis or demonstrate the economic impact of their membrane material compared to existing technology.
- The investigators presented a scenario in which a 3-component staged separation scheme might be employed, with their metallic membrane used as the first two phases to be followed up by a nanoporous phase. The relative impact on cost, flux, and efficiency using the staged separation was not discussed.

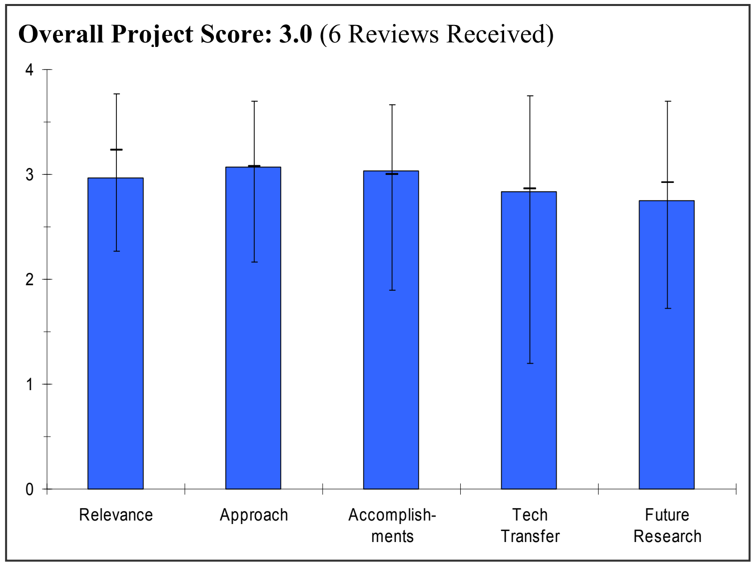
Specific recommendations and additions or deletions to the work scope

- R&D thrust required to improve flux, purity, and selectivity. Particular attendtion should be paid to the DOE 2007 target of 100 ft^3/ft^2/hr/100psi trans-membrane delta P.
- Finish down-selecting compatible materials for tube fabrication. Report permeation results using "real-world" gasification streams.
- Ensure that the project PI aggressively drives the project forward and does not allow it to stagnate.

Project # PD-13: Scale-up of Hydrogen Transport Membranes for IGCC and FutureGen Plants

Paul Grimmer; Eltron Research Inc.

Brief Summary of Project



This project involves the development of an environmentally benign, inexpensive, and efficient method for separating hydrogen from gas mixtures produced in industrial processes such as coal gasification. In addition to producing purity hydrogen, when combined with water-gas shift, the membranes also provide carbon dioxide capture at high pressure. The membranes operate at 300-450^{o}C at pressures up to 1,000 psi, which is compatible with water-gas-shift conditions in gasified coal feed streams. System costs in a FutureGen-type plant that produce hydrogen as well as capture carbon dioxide are estimated to be half that of systems incorporating pressure swing adsorption (PSA).

Question 1: Relevance to overall DOE objectives

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

- This FutureGen project seeks to use membrane technology to produce low cost H_2 from pulverized coal-derived syngas. It is a continuation of a Vision 21 project. Technology may not lend itself to distributed production or reduce CO_2 emissions (without sequestration).
- The use of synthesis gas and the water gas shift reaction for the production of hydrogen, supports the President's Hydrogen Fuel Initiative. However, in order to be climate-change neutral, sequestration of the carbon dioxide produced would be required.
- Lower cost H_2 separation membranes offer flexibility in system design.
- The project aligns well with the President's Hydrogen Fuel Initiative and the Hydrogen RD&D Plan by developing a critical element needed for hydrogen purification – a hydrogen separation membrane. The membrane, if successfully developed, will help reduce the cost of hydrogen production and produce hydrogen with sufficient purity for fuel cell applications.

Question 2: Approach to performing the research and development

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

- The approach for scale-up seems well thought out and is logical. Good attention to rate, purity, pressure of permeate, effect of sulfur. System has higher flux rate than Pd or Pd alloy membranes although the membrane is not as thin. Cannot comment on suitability of alloy catalyst since that is proprietary. Membrane lifetime is questionable and needs long-term investigation.
- The general approach to integrate the water gas shift reaction and the separation of hydrogen from carbon dioxide is innovative, particularly in the elimination of the need for palladium.
- Membrane sulfur and mercury tolerance is crucial for the process viability.
- High purity hydrogen is produced, in the bench-scale experiments.
- The need for a 1000 psi delta p across the membrane would seem to result in a large loss in energy in the process. Unclear if energy is deposited as heat in the membrane.
- Question the complexity of the design as to it being economical (for hydrogen production for fuel cell vehicles).

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

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

- Comparative data gathered using a palladium membrane is impressive, as is the tolerance to sulfur.
- Although the 11 month durability curve shows excellent superiority to palladium, the slope of the line appears to cross the relatively stable palladium line in only a few months.
- In the past year the investigators considered process design and costs; demonstrated 1000 psi differential pressure across the membrane; eliminated the sweep gas; and overcame hydrogen embrittlement issues to demonstrate 270 psi outlet pressure, which will improve economics. Appears to met 2010 targets and approach some 2015 DOE targets.
- The team has demonstrated good long-term performance through the 11-month run with simulated syngas. Simulant did not include sulfur or mercury and thus the contaminant tolerance of the membrane has not been similarly demonstrated.
- The meeting or exceeding of the 2015 FutureGen Targets is an impressive technical achievement.
- Elimination of the need for palladium is also an important strategic and economic achievement.
- Question why performance degraded by a factor of 3.
- Accomplishments lacking. There is a large degradation in the membrane shown without an explanation.
- Accomplishments including the elimination of the sweep gas, increased outlet gas pressure of 270 psi, and improved sulfur tolerance of 200ppm are significant improvements and indicate that future barriers could also be overcome.

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

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

- Partner collaboration is unclear.
- The collaborations seem to be well-directed toward the achievement of the project goals. The need to arrange for a "replacement subcontractor" for the last three years of the project could be an obstacle.
- Although prior collaborations are good, future collaborations are yet to be determined.
- Participant roles are unclear.
- It is unclear if any of the participants will invest in and lead the effort to commercialize the concept or if their level of participation will remain on a fee-for-service basis.

Question 5: Approach to and relevance of proposed future research

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

- Continued scale-up work appears on track.
- Results with "real" coal syngas would be good.
- The planned future research is aggressive in the expansion to a 4 ton/day plant. Thus, it is imperative that a subcontractor for the construction and operation of that plant be brought on board.
- Long-term testing of the membranes in contaminated syngas streams should be carried out as soon as possible to determine the degree of degradation in performance beyond the 250-hour tests.
- Future work is not sufficiently specific although it includes further research on the top/bottom catalyst layer to increase sulfur tolerance with alloys on the top layer and testing cermets to reduce cost. The effort builds on past progress and should address known and unknown barriers in due course.
- Root cause for membrane degradation during the 11-month test should be known. This knowledge will help improve this membrane's stability, the one feature of this membrane that does not meet DOE's targets.

Strengths and weaknesses

Strengths
- Excellent work plan.
- Some very encouraging results so far.
- High flux membranes key for hydrogen production cost reductions. Much lower cost than PSA systems because does not need to pump away CO2.
- The team has demonstrated impressive production and separation of hydrogen from syngas.
- The membranes have met or exceeded 2015 targets for such production/separation.
- The membrane avoids the use of palladium.

Weaknesses
- Membrane shows deterioration over the 11 month test and it is unclear that it will have required durability. Degredation is not understood. Have not considered mass manufacturing protocols in depth.
- Compression costs have not been included in the hydrogen cost analysis.
- Reasonable costs for carbon sequestration need to be included in the overall cost of hydrogen.
- If tubular vs. plate (planar) design was analyzed, results were were not sufficiently addressed. Need to understand why the flux degrades by a factor of 3 over the 11 months test. Question whether the degradations are worse with real syngas rather than simulated syngas.
- It is apparent, knowing the literature, what the metallic membrane system must be composed of. As such it is also known based on past work what the interferrants are that will lead to performance degradation. There needs to be a comprehensive study to evaluate these interferrants and determine their linkage to the membrane degradation. Further, one test is not a durability study. Need to know performance scatter between multiple membranes. Need to know if there are any mechanical durability issues that may result from time on test or the interferrant in the gas stream.
- Insufficient details are provided to justify the comparative cost of $41 million used for PSA. Information should be provided on the type of PSA unit to support the extremely high cost used for comparing the Eltron membrane with PSA.

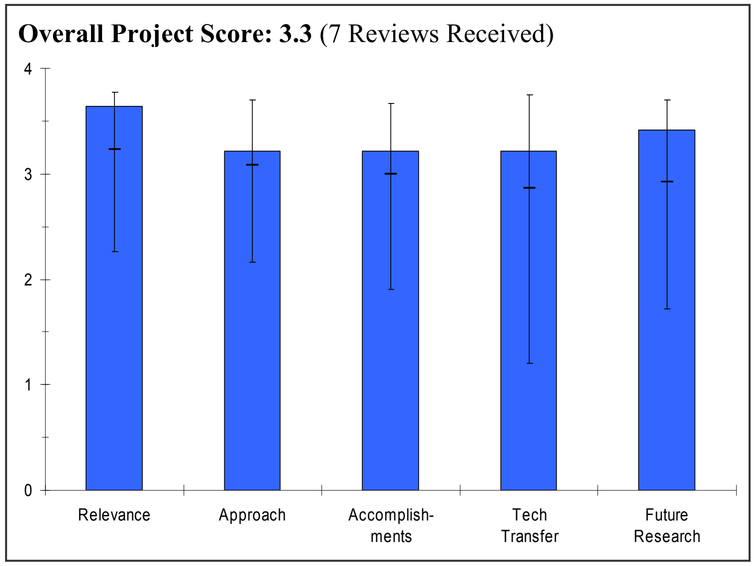
Specific recommendations and additions or deletions to the work scope

- Need to compare the cost of a more durable membrane with that changed out more often. Long-term membrane stability needs to be demonstrated prior to building higher volume production system.
- In moving from the bench-scale to small pilot-scale plant, it will be necessary to demonstrate economical production of the membranes in increasing sizes. Sputtering apparatus would be suitable for limited production, but a continuous process would have to be considered for large-scale manufacture.
- The causes for the degrading performance over 11 months in a clean stream will have to be understood and the corresponding degradation in a sulfur and mercury-contaminated stream will have to be determined.
- Seems to be more focused on CO_2 recovery and sequestration than on H_2 production.
- Test protocol needs to be improved to look at gas stream compositions similar to coal gas.

Project # PD-14: Cost-Effective Method for Producing Self-Supporting Pd Alloy Membrane for Use in the Efficient Production of Coal-Derived Hydrogen

James Arps; SwRI

Brief Summary of Project



The overall goal of this project at Southwest Research Laboratory (SwRI) is to develop technologies that effectively and economically separate hydrogen from mixed gas streams that would be produced by coal gasification. SwRI is developing a process methodology for the cost-effective manufacturing of extremely thin, dense, self-supporting palladium (Pd) membranes; reduce Pd membrane thickness by >50% over current state-of-the-art; demonstrate viability of using ion-assisted vacuum processing to engineer a membrane microstructure and surface that optimizes hydrogen permeability, separation efficiency, and lifetime; demonstrate efficacy of large-area manufacturing of membrane material with performance and yields within pre-defined tolerance limits; and establish scale-independent correlations between membrane properties and processing parameters.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Work will benefit both FE and EERE hydrogen research efforts -Well aligned with the President's Hydrogen Fuel Initiative and the Hydrogen RD&D Plan as it develops a critical element needed for hydrogen purification – a hydrogen separation membrane. The membrane, if successfully developed, will help reduce the cost of hydrogen production and produce hydrogen with sufficient purity for fuel cell applications.
- Fabricating new hydrogen separations materials with high flux, high selectivity, low cost, and applicability for commercial manufacture directly addresses technical goals of the Hydrogen Fuel Initiative.
- Development of Pd alloy membranes with high flux for H_2 separation will make good contributions to the H_2 fuel initiative.
- Project objectives are relevant to four key barriers in the program objectives.

Question 2: Approach to performing the research and development

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

- Good approach and facilities to carry out the work. - Question whether complex design can be economical -
- Free-standing very thin membrane will not be suited for real world applications. Deposition of thin film membranes on porous support structure should be done.
- More data on durability should be developed.
- The presented states that it is nearly 85% complete and appears to be on track. These approach is well focused on the major technical barriers that must be addressed to develop a sound membrane technology.
- Although stated as being 85% complete, there remains the task of demonstrating the separation efficiency of the membrane in a commercial-type fuel processor using mixed gas streams. It is questionable if this can be accomplished in the remaining 4 months of the project, based on the rate of progress over the past three years.

- Fabrication of thin, defect-free high flux Pd-Cu membranes for hydrogen separation has presented a challenge for some time. This project was highly focused on use of sputter deposition methods and with careful attention to experimental variables appears to have achieved its goal.
- Proposed approach was based on the expertise of PI in the area of vapor deposition technique.
- The project is supported by coal to H_2 program for central H_2 production. The scale-up of proposed technology for large H_2 plant operations was not adequately discussed: (a) will the membrane need a support for high pressure operations? If so, how to proceed? (b) what was the commercial scale intended for the applications of these membranes?
- The approach uses well-validated technologies to produce thin, self-supporting palladium alloy membranes for hydrogen separation from coal gasification streams. The technical barriers have been clearly addressed and the work plan to finalize the project is very logical, including the respective contributions of each of the partners.

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

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

- Very high flux for Pd-alloy membrane has been demonstrated in this project specifically, vacuum deposition process can be used economically to produce large area membranes. Influence of permeation on temperature is questionable.
- Project should have more progress on module development and testing this late in the project life.
- Elimination of the sweep gas, increased outlet gas pressure of 270 psi, and improved sulfur tolerance of 200 ppm are significant improvements and indicate that future barriers could also be overcome.
- It is unclear what is meant by "Pd = 15, Pd-Cu alloy = 8".
- Many of the project objectives have been completed, including the very challenging objective of identifying sputter deposition and annealing conditions for reproducible fabrication of thin, defect-free membranes. It appears likely that the remaining Year 3 objectives outlined on Chart 4 can be completed in the time remaining.
- Use of vacuum deposition technique in membrane preparation was carried out with good results.
- The reproducibility issues for membrane fabrication technique were not adequately discussed. As an example, the reported membrane thickness varied over a wide range although it is unclear whether this was intentional.
- The membrane sulfur tolerance test was not performed.
- Unclear what the pressure tolerance for the membranes is.
- The project plan is ahead of schedule and has surpassed the DOE near-term flux targets.

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

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

- Good collaboration with partner. Some interaction with organization developing or producing supports would greatly strengthen this project. Both partners appear to be well qualified.
- It is unclear if any of the participants will invest in and lead the effort to commercialize the concept or if their level of participation will remain on a fee-for-service basis. Unclear where technology transfer will be carried out
- No mentioning about the targeted market for the membrane developed.
- The project team has already demonstrated a strong working relationship, with other commercialization projects with the industrial partner, and the investigators have clearly described new collaborations that have been initiated for the next generation ternary alloy development.

Question 5: Approach to and relevance of proposed future research

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

- Pathway to prototype not clear, how is the film to be supported?
- Membrane performance must be tested under coal gas conditions before proceeding to pilot unit demonstration. Future work should focus on depositing thin (3 micron) membrane on porous support structures. Can their technique be used to deposit thin film membranes on curved (tubular) surfaces?

- The future work builds on past progress and should address known and unknown barriers in due course.
- There is some concern that some of the tasks, i.e., establishing reliable sealing methods, developing membranes with ternary alloys, the full scale prototype module demonstration, and the detailed cost analysis of membrane production process may not be completed by the stipulated end date of the project (Sept. 2006).
- Project appears to be in wrap up phase. Interesting new work planned with collaborating researchers on testing ternary alloy systems designed.
- Future work is closely aligned with current project objectives, and logically continues to address DOE program objectives.

Strengths and weaknesses

Strengths
- Very strong knowledge/expertise in vacuum deposition technique. Excellent deposition facilities
- The performance of the membrane is good, both flux and H_2 purity.
- The project has developed a novel vacuum deposition method to fabricate free standing Pd alloy hydrogen separation membranes and has evaluated their performance. The membranes are the largest produced so far in DOE's projects.
- The investigators have demonstrated a well-designed and clearly-executed work plan, with project goals met ahead of schedule.
- Good understanding of relevant off-the-shelf technologies and methods for refinement of their palladium alloy material.
- Good demonstration of understanding of scale-up capabilities and potential pitfalls.
- Good understanding of materials characteristics required for their thin film alloy, as demonstrated in plans for design for ternary alloy.

Weaknesses
- It was not clear in the presentation how the films will be supported on a large scale. There needs to be some thought addressing the support and how it will be used. Also, the cost study should be rerun with the support considered. Durability testing is a must and should be carried out looking at impurity gases that may lead to degradation of the Cu-Pd.
- Not to focus on depositing films on support structure. Free-standing extremely thin membranes are not useful for practical applications.
- Project is near completion date (September 2006) and the work is behind schedule, i.e. There is no cost projection performed, module work is incomplete, no durability testing.
- The project may end up behind schedule since some of the more involved tasks, i.e., establishing reliable sealing methods, developing membranes with ternary alloys, the full scale prototype module demonstration, and the detailed cost analysis of membrane production process have yet to be completed.
- Need to resolve issues with membrane robustness and integrity in testing chambers—the investigators probably should have foreseen that the gaskets would not exhibit surface smoothness commensurate with the fabricated testing material.
- Need to complete detailed cost analysis.
- Need to clearly correlate thickness of palladium alloy membrane with desired flux, to broaden utility of their material.

Specific recommendations and additions or deletions to the work scope

- Test protocol needs to be improved to look at gas stream compositions similar to coal gas. Ternary alloy development is good. Before moving into full scale prototype demonstration, need to demonstrate that these membranes are immune to trace impurities found in coal gasification streams.
- Need to test membranes in H_2S-containing gas to ensure that thin Pd Cu films behave similar to thicker filmas in terms of S effects on flux and selectivity.

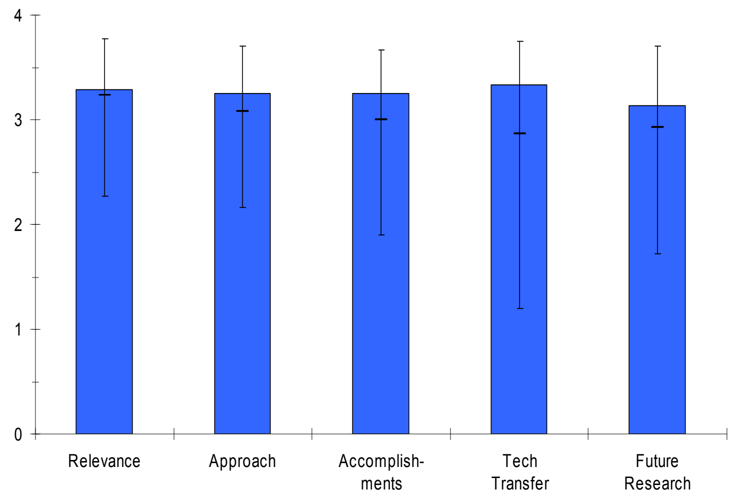
Project # PD-15: Sulfur-Iodine Thermochemical Cycle

Paul Pickard; SNL/INL/GA

Brief Summary of Project

The Sulfur-Iodine (S-I) thermochemical cycle is being investigated as a potential method to produce hydrogen from water using nuclear energy. Current research focuses on lab scale experiments to demonstrate the basic reaction steps and candidate materials for the three major component reaction sections that make up the S-I cycle. The project will: 1) determine process operational parameters; 2) Evaluate engineering approaches and materials of construction; 3) Evaluate reactor –process interface and control technologies; 4) Provide a basis for cost projections and comparisons; and 5) Support the DOE Nuclear Hydrogen Initiative technology selection decision (FY2011).

Overall Project Score: 3.3 (9 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- To be useful, this project must be developed successfully, there must be an new concurrent gas-cooled Reactor Project, and a practical hydrogen delivery system from the reactor site to the "forecourt" is required. The PI didn't address any of these issues in the presentation, yet they bear heavily on the timetable for decision in the hydrogen infrastructure program.
- Thermochemical H_2 productions offers the potential for highly efficient H_2 production.
- This project looks into the production of hydrogen using the sulfur-iodine cycle using nuclear energy. This project is relevant to the Nuclear Hydrogen Initiative.
- This project is very relevant to the DOE program goal to generate H_2 via a water splitting thermochemical cycle using nuclear heat. Also, materials issues are being addressed on a number of fronts and will be beneficial to a number of projects.
- The future of this technology is tied to the development of the Gen IV Nuclear Reactor which is still in question.
- The project directly supports the President's Hydrogen Fuel Initiative.
- The project supports major objectives of NHI R&D plans and overall DOE Hydrogen Program plans.
- The President has recently called for the increased use of nuclear energy to meet the Nation's energy needs, and this particular work provides an efficient approach for the utilization of nuclear energy in the production of hydrogen.
- Thermochemical cycles appear to be one of, if not the most, promising methods for large scale emissions-free hydrogen production. It is clearly relevant to our nation's H_2 program.

Question 2: Approach to performing the research and development

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

- There is good focus on proving the feasibility of the cycle.
- The goal is to adapt this cycle to a high temperature nuclear reactor. Part of the approach should be to identify all of the steps to achieve this goal. This project is the first step, but the path to the end point should be identified.

- Not clear how the phase 1 & phase 2 objectives lead to meeting the primary objective: Determine potential of the S-I cycle using nuclear energy.
- Technical barriers related to the NHI program are addressed in this project. The objective for FY 2006 is to complete development of the three major reaction sections. SNL's effort is well integrated with other research.
- It is advised to stop research with Pt catalyst since a cost of $1200/gram and the fact that much of it is lost in the process in a short operating time. Lifetime and more in-depth performance studies need to be done on non -Pt catalysts. Also, alternative catalysts indicate that only about 40 to 50% of the SO_3 is converted to SO_2. This, combined with the fact that some recombination occurs, brings up issues relating to the effect of unconverted SO_3 on the efficiency of the process. However, if in fact the amount of SO_3 doesn't matter as the speaker has indicated, then there should not be much effort directed to developing any improved catalyst. In that case, research to improve the SO_2 yield from SO_3 may not be worthwhile. The project does a good job of directing resources to make progress in solving a lot of problems.
- Research staff understands the issues and is moving to address each issue in a timely fashion.
- The approach in the S-I process of having the three sections developed by GA, Sandia and CEA is a good use of international collaboration. However, integrating the three sections and the varying design philosophies will be difficult.
- Approach and barriers addressed. Approach to technical feasibility verification discussed. Other research being integrated to improve process design.
- Overall, the approach is solid as described by the PI. There should be improvement in the description of both the catalyst development and reactor materials selection tasks. For example, details on the screening methods for spinel or perovskite composition would be helpful, and detailed development of such a method could allow for a better project end result.
- The project's approach appears to be addressing known barriers adequately. Coordinating the activities of the three major participants seems to be effective - in what could otherwise be an extremely inefficient operation.

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

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

- Significant amounts of experimental data have been obtained. How will SO_2 and SO_3 be separated?
- Alternative flowsheets for all sections evaluated & selected. Significant amount of work done on H_2SO_4 decomposition experiments. More work needed in the area of the HI decomposition step.
- The progress made in the area of dissociation reaction materials, i.e. SiC is very good. Other progress made in the area of using membranes to separate out water is also very good. Preliminary reactive distillation experiments are good.
- Research staff has made good technical progress during the past year. Especially noteworthy is the development of a bayonet type SiC heat exchanger.
- The successful test of the sulfuric acid decomposition step at Sandia and the development of the integrated SiC-based decomposer are encouraging for the overall promise of the cycle.
- The degradation of catalyst in the sulfuric acid section and the high mass flow rates of iodine are problems that must be solved for a viable process.
- Accomplishments/Progress reviewed. Major program baseline milestones discussed. Major progress has been made, but more clarity is needed in current development status relative to support of the planned pilot and engineering scale milestones. Not clear whether French CEA partner is on schedule to support integrated lab system (ILS) milestones.
- The PI has shown that some hurdles have been overcome, and others have been identified. Of the problems yet to be overcome, there are plans of varied detail for their solution and the steps to follow. Overall progress has been significant, if not outstanding.
- Technical accomplishments and progress appear to be on track and given the three tiered or segregated structure of the overall effort, it seems incredible that things are progressing as well as they are.

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

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

- The PI clearly values technology transfer and collaborations.
- They have several different team members addressing individual aspects of the overall problem.
- Very good collaboration with several organizations.
- In general, the collaboration is good relative to academic and industry partners. It is unclear what benefit UNLV brings to the project in terms of materials. Also, it is not clear why INL is the best partner for developing an H_2SO_4 dissociation catalyst. Having GA involved is good.
- Good mix of industrial partners who can carry this technology forward to commercialization.
- There is good collaboration among the developers of the three process sections.
- There is relatively little interaction with utilities and other potential users of the process.
- Multiple laboratories and industry partners participating. University team supporting material and heat exchanger development. Partnership with French CEA in the development of the Bunsen process section. Other laboratories looking at membranes and catalysts.
- The PI presents little evidence of the extent or value of collaborations between the various team partners; however it is clear how such interaction will allow for the successful completion of the project.
- The segregated responsibilities (Sandia, GA, and CEA) certainly encourage collaboration and exchange of R&D results. This segregation probably makes the effort more cumbersome to manage but the results and progress to date suggest that this has been quite a successful effort and that the exchange of information is very good.

Question 5: Approach to and relevance of proposed future research

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

- They propose to continue the current approach to complete evaluation of the different cycle steps.
- Good plans for the future. Too much Pt is lost in a very short period of time. Would like to see more research to minimize Pt consumption.
- It seems as if more emphasis should be made to develop low cost dissociation catalysts since only about 50% conversion is being demonstrated and the catalyst is deactivating or lost. It is suspected that so much SO_3 recycled in the process will have a big impact on reduced process efficiency.
- Future work will demonstrate the chemistry in a closed loop cycle and look for contaminants carried over from one step to the next.
- The next steps will involve the integration of the three sections for the S-I process and the integration of the sulfuric acid decomposition and electrolytic sections for the hybrid sulfur process.
- Long-term demonstration of materials' durability and of control of the process will be necessary.
- At some point in the next year or two, it will be necessary to incorporate the best prediction of material flow rates into the flowsheet for economic and efficiency analysis.
- Discussed details of ongoing R&D work and plans addressing the major barriers for each of the major process sections. Noted options and improvements available to address technical and economic feasibility issues.
- The next steps and the future plans appear to be right on target and logical given the experience gained to date.

Strengths and weaknesses

Strengths

- The breadth of collaboration.
- Many different significant aspects are being investigated.
- Excellent team to make the S-I cycle a viable process to produce hydrogen using nuclear heat. This team has expertise/capabilities to successfully complete this project.
- Seems to be a well organized project with a good knowledge of process challenges.
- This is the most well studied of the thermochemical water splitting processes and is the most advanced. It also has the least unknowns so far as surprises in the process chemistry and the materials science. The efficiency calculations are likely to be the most reliable for this cycle.
- Good progress has been made in the development of materials, and of the all-ceramic decomposer.
- The two processes have the potential to be quite efficient when driven by a high temperature reactor.

- Extremely well organized and easy to follow presentation/program. Good future R&D planning to address known and potential issues.
- The lab-scale experiments have produced valuable results. If and when materials issues are resolved, the team will be in excellent position for the pilot-scale demonstration.
- Good project management and apparently there is good cooperation among the primary contributors.

Weaknesses

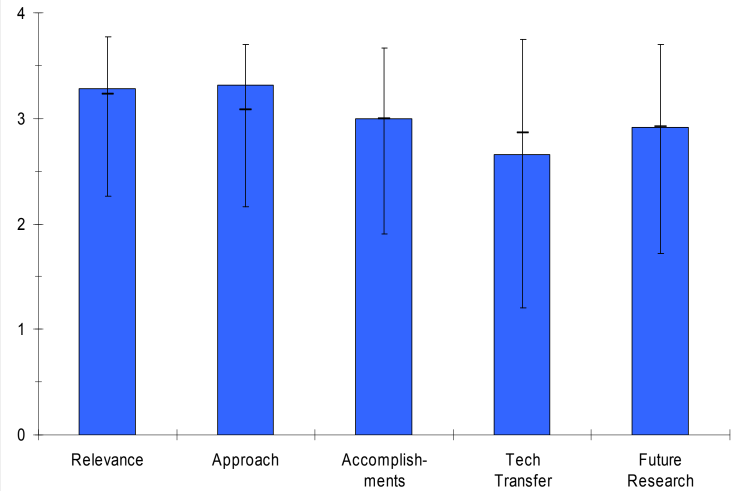
- The hesitancy to down select cycles.
- Need to use process analysis as the basis for study of individual steps, including heat sources and species separations.
- Lack of work to minimize Pt loss.
- The PI should show in a future meeting that a low SO2 content has little impact on overall H2 cost. It doesn't seem that if the S-I process would only get say 30% SO2 and 70% SO3 that it would be economical relative to 70% SO_2 and 30% SO_3. It seems that cost of all the recycling and the size of process equipment (capital and utilities) would be significant for this. This is not an efficiency issue. It's a cost issue.
- There are significant problems with the Pt-based sulfuric acid decomposition catalysts that do not have a simple solution. The S-I Process is not a simple process but has many steps that will require a large capital investment in processing equipment. SiC and super alloy heat exchangers are costly.
- It will be a challenge to integrate the three sections of the S-I process into a single integrated experiment.
- High material recycle rates will have to be reduced for improved efficiency.
- A slide on process efficiency including the impact of recent developments would be helpful. Add some discussion of process section interface issues and integrated controls for the ILS.
- The material concerns that arise due to the extremely high temperatures required by the sulfuric acid dissociation step place this particular cycle at an engineering disadvantage. The interaction between the various entities involved in this project is reported to be extensive, but it is not clear from the presentation that a sufficiently interactive infrastructure exists to best accomplish the work.
- If there is a weakness in this effort, it may be that little collaboration from industrial partners (gas companies, refiners, etc.) has been solicited/obtained. It may be early in the process, so this may be a "later in the project" item. It does seem that commercial insight would help insure that the end results are commercially viable and that minimal non-productive efforts are undertaken. - Just a thought -

Specific recommendations and additions or deletions to the work scope

- Focus on down selecting to one or two options and then putting available resources to see if the project goals can be achieved.
- Need to place greater emphasis on separations and on interacting issues (with the nuclear heat source). Overall efficiency analyses?
- More effort in the sulfuric acid decomposition step. Explore membrane concepts to enhance the sulfuric acid decomposition reaction.
- The program needs to get a better handle on capital costs for processing equipment. If the process chemistry is sufficiently well known for a HYSIS spreadsheet to be developed, then enough information exists for a good chemical engineering school to do a first level cost estimate on a complete plant. From this point on the R&D should be driven both by materials issues and cost issues.
- Siting, toxic materials inventory and plant separation issues have both economic and safety implications. These need to be included in the overall analysis.
- It is recommended that the PI include further interaction or details of interaction with the industrial and academic partners. It is likely that there will be faster solution of project barriers if a more cohesive effort is realized.

Project # PD-16: Evaluation of a Continuous Calcium-Bromine Thermochemical Cycle
Richard Doctor; ANL

Overall Project Score: 3.1 (9 Reviews Received)



__Brief Summary of Project__

Argonne National Laboratory Argonne is evaluating the Ca-Br cycle for H_2 production using nuclear energy and assessing whether it is practical. The two focus areas of research examining cold plasma or electrolytic methods for hydrogen generation as a replacement for the iron bromide/oxide reaction beds in the UT-3 cycle (earlier Japanese work), and investigation of the feasibility of a continuous molten spray reactor approach for the HBr generation step.

__Question 1: Relevance to overall DOE objectives__

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

- The President's initiative focuses on 2010 and 2015 decision points. The project presumes the existence of nuclear high temperature reactor technology and a distribution system for centralized hydrogen production. I don't expect the PI to be able to alter those things but a discussion of how those realities are impacting this project (or not) should have been a part of his presentation.
- The objective of this project is to evaluate Ca-Br cycle for hydrogen production using nuclear heat. This project is far behind some of other projects that are funded by the NHI.
- Thermochemical processes for H_2 production add flexibility to the use of nuclear and other heat sources in H_2 production.
- The objective of the DOE research effort to use nuclear heat to split water via a thermochemical cycle is relevant.
- This process supports the President's Hydrogen Fuel Initiative and has the added advantage of operating at a somewhat lower temperature than competing thermochemical processes.
- It is important to have alternatives to the S-I cycle, especially ones running at a lower maximum temperature.
- The President has recently called for the increased use of nuclear energy to meet the Nation's energy needs, and this particular work provides an efficient approach for the utilization of nuclear energy in the production of hydrogen.
- This thermochemical water-splitting process is one of several that warrants research – particularly in light of the lower relative temperatures required.

__Question 2: Approach to performing the research and development__

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

- Good focus on defining and attacking the barriers.
- Two approaches are followed/explored for the dissociation of HBr. Even though significant amount of work is done on using a plasma technique to decompose HBr, it seems this technique may not be suited for practical application. The PEM approach is a good one.
- Although a plasma process will eliminate materials issues associated with electrolysis, it is not clear that a plasma process makes sense for any commodity chemical such as H_2. This is not a specialty coating or film, some of which are plasma deposited. This reviewer is not aware of any commercial plasma process used

commercially to produce a commodity chemical. In addition to the cost of the unit operation, it is clear that compression will be expensive as well.
- The difficulties in the cycle are in development of a continuous cycle and in the plasma or electrolytic dissociation of HBr to hydrogen and bromine.
- Elimination of the supported calcium bromide is a good step as is looking for alternatives for HBr decomposition.
- Barriers addressed. Considered work of Japanese building upon their R&D results. Discussed past work on HBr process by others. Project focused on evaluating the technical and economic feasibility of the process with goal of go/no-go decision for further development. Discussion of process technical advantages and disadvantages.
- The goals and challenges faced are well described and understood by the PI. Additionally, the PI recognizes that the main barriers to address (implementation of a continuous process, thermal integration) have been addressed for a myriad of processes in the chemical industry, and his plan of attack is both straightforward and likely to succeed.
- Approach is generally very good.

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

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

- The goals were limited but they were achieved.
- Efficiency of 45% appears reasonable.
- Somewhat disjointed discussions of work and results.
- The project seems to have just gotten started and so not a lot of progress is expected as of yet.
- Experiments are not yet running, though a decision on the process is due this summer.
- The use of the plasmatron seems to introduce a large measure of uncertainty and complexity in the design, perhaps too complicated for large-scale hydrogen production.
- PEM electrolytic approach seems to introduce the need for electricity at a cell voltage only about 50% below that for pure electrolysis.
- Good progress was made for the funding available.
- Timeline & percent complete discussed. Major milestones discussed. Discussed results of initial laboratory proof of principle tests and process cycle evaluation results. Experimental program progress towards assessing process efficiency goal discussed. Anticipates about a 3 year program to get an integrated laboratory scale test accomplished. This should support plans for thermochemical pilot scale decision point.
- The ultimate success of the CaBr cycle is yet to be demonstrated in a continuous process, but the PI shows that solid plans exist for the solution of identified problems. The added operating cost per kg of H_2 obtained from HBr using the method of plasma dissociation will be an important consideration.
- More work clearly needs to be done before engineering scale demonstration but accomplishments thus far seem to suggest that continued research is in order.

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

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

- Good within the present project but there was no collaboration for moving forward into the next phase.
- Collaborates with a university to explore the possibility of using PEM electrolysis for the dissociation of HBr. No results on this were presented.
- Collaborations with the university of South Carolina.
- None were apparent other than some peripheral work at Univ. of South Carolina.
- Collaboration with the University of South Carolina seems to be fruitful.
- There is not collaboration with manufactures of plasmatrons, PEM cells or potential users of the process.
- Admittedly this is a small project. Nevertheless there should be an industry representative, ideally from the chemical process industry, either sitting on a board of technical advisors or as a full partner in this work.
- Collaboration/partnership with University of SC in development of PEM system for the back end of the process.

- The PI does not provide evidence or detail for the level of collaboration with the research partner (University of South Carolina). We are informed of some general HBr work, but not of any interaction. If there is technology transfer planned, it is not detailed here.
- Although collaboration with the University of South Carolina appears well established, this project did not seem to interface or even communicate with other similar efforts.

Question 5: Approach to and relevance of proposed future research

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

- This was lacking in the presentation.
- Very difficult to read the future plans chart.
- Future research activities were not discussed clearly.
- This reviewer is not sure if carrying out experiments with a plasma reactor is warranted. It seems that this process would be too expensive.
- The plans for developing a continuous process are laudable and necessary for the economic production of hydrogen.
- The plans for both the plasmatron and for the PEM electrolysis steps seem to introduce a good deal of complexity and electrical energy into the process. Perhaps this complexity is necessary, but it detracts from the economics of the overall process.
- The current future plans concentrate on solving materials issues.
- Discussed conceptual design and plans for laboratory scale testing. ILS testing should provide insight into longer term operation performance of materials.
- Future plans are well outlined in the Gantt charts. This approach shows a great deal of focus.
- Future plans seem consistent with past experience and results.

Strengths and weaknesses

Strengths
- Good, workmanlike effort.
- Experimental data under harsh reactions conditions.
- The process seems pretty simple (2 steps) and does not require any concentration of a liquid acid/base .
- Operates at lower temperatures than the S-I Cycle: 760 versus 900C. Therefore there will be less risk in developing this cycle than the S-I Cycle in the event that a Gen IV Nuclear reactor and attendant heat transfer system can't deliver 900 C heat. There appears to be fewer corrosion issues with the CaBr Cycle than with the S-I cycle especially if a PEM electrolysis cell is used to decompose the HBr.
- Well organized and good technical depth in presentation. Strong technical basis presented to justify further process development.
- This work on the improved, continuous CaBr thermochemical cycle for the production of hydrogen from steam is proceeding well, has made strong strides, and has identified several key barriers. The PI makes a strong case for continuation of the work.
- The most notable strength appears to be the intent or focus that characterizes this effort. Often, projects lack this focus and fail to accomplish meaningful results.

Weaknesses
- The future steps weren't addressed in the presentation. Hopefully that's a deficiency of the presentation and not of the project. If the project hasn't addressed the "next steps" they need to do that ASAP.
- Do not have solid plans.
- Did not provide a summary of the cycle at the beginning of the talk - took a lot for granted on the part of the audience.
- Solids handling for CaO and $CaBr_2$ (Particularly for liquid $CaBr_2$).
- Plasma process requires vacuum?

- The process seems expensive. The plasma option does not seem to make sense. The PI should provide convincing evidence of an existing commercial process that uses a plasma to make a commodity chemical. Some processes, like making carbon black, have been proposed. However, this reviewer is not aware of any actual commercial commodity chemical plasma based chemical processes.
- Volume change of CaO to CaBr is a barrier.
- Need for unproven processes to decompose HBr.
- Plasmatron seems like a laboratory apparatus. Efficiency impact is unclear.
- Refrigeration may be necessary for the handling of the bromine. Refrigeration costs not included.
- Requires energy input in the form of electricity for either the plasmatron or the PEM electrolysis cell. A PSA system for the recovery of hydrogen from the plasmatron is less desirable than a membrane approach to H_2/Br_2 separation.
- This project could benefit from a higher level of interaction between the partners involved; or at the very least, this relationship should be better described in order to make the roles of the team members clearer.
- The only perceived weakness is the relative isolation of the project from other ongoing efforts. Recognizing that "focus" is cited as a strength, this project team must not lose sight of parallel results and the benefits of a wider collaborative effort.

Specific recommendations and additions or deletions to the work scope

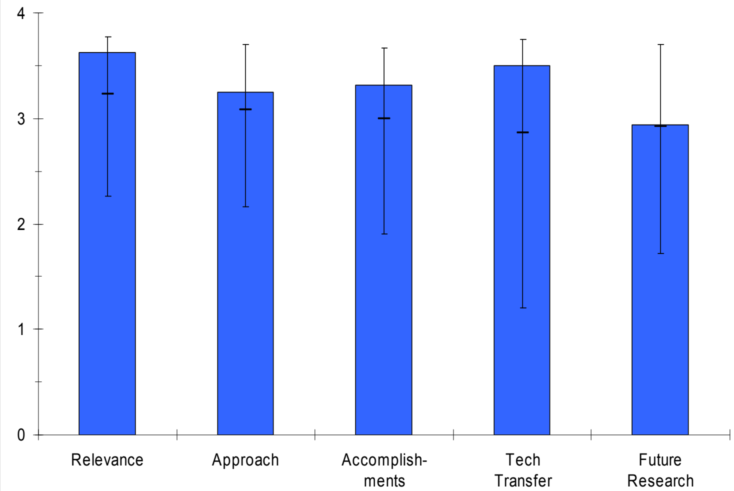
- Serious effort must be focused on using an electrolyzer for HBr dissociation.
- Future work should have been discussed more explicitly.
- Unless the PI can indicate a current commercial plasma process to make a commodity chemical, the plasma reactor option should be deleted from the project. This indicates that an electrolyzer should be used.
- A go/no-go decision on this process is scheduled for this summer. The evaluation should include both the promise of lower operating temperatures and the complexity of the steps brought about by the HBr decomposition.
- Future plans should include preliminary process design studies suitable for assessing costs and cycle efficiencies using engineering principles that are familiar to the chemical process industry. It is not clear that the efficiency calculations for the plasmatron are consistent. Was the 75% efficiency based on electricity in and the lower heating value of the hydrogen out, or did it also consider the inefficiency of generating the electricity from the nuclear heat source and, if so, what efficiency was used? This has always been a problem when comparing pure chemical cycles with hybrid cycles.
- The project scope is quite appropriate as it is.
- At this juncture, a possible addition to the effort that might be considered is the inclusion of industrial chemical plant personnel in an advisory role. This insight may help steer research to achieve a commercially viable process with minimal dead ends. - just a thought.

Project # PD-17: Laboratory-Scale High-Temperature Electrolysis System

Steve Herring; INL/ANL/Ceramatec

Brief Summary of Project

Overall Project Score: 3.3 (8 Reviews Received)



Idaho National Laboratory is currently researching and developing high temperature processes to produce hydrogen through chemical cycle-water splitting technology or other non-carbon-emitting technology utilizing heat and electricity from nuclear or solar sources. The project is seeking to develop energy-efficient, solid-oxide electrolyzer cells (SOECs) for hydrogen production from steam. Key goals are to reduce ohmic losses to improve energy efficiency; increase SOEC durability and sealing with regard to thermal cycles; minimize electrolyte thickness; improve material durability in a hydrogen/oxygen/steam environment; and develop and test integrated SOEC stacks operating in the electrolysis mode.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- A concept tied to the development of a yet to be committed high temperature nuclear reactor is questionable.
- Unclear what the context of the High Temperature Reactor is in relation to the go-no-go decisions in 2010 and 2015 with respect to the President's Hydrogen Fuel Initiative.
- Good use of by-product nuclear heat to supply part of the energy needed to split water, making the process less electricity dependent and therefore less expensive. .
- This program strongly supports long-term future hydrogen generation issues..
- High temperature electrolysis (HTE) is one of the viable options which show good promise to produce H2 from nuclear power that could be competitive with H2 from other sources.

Question 2: Approach to performing the research and development

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

- No project milestones later than 2006 were presented and no go-no-go milestones from 2006 to 2015. Builds on Solid Energy Conversion Alliance (SECA) (through partners) and EE/RE-funded work to advance the development of laboratory prototypes to proceed from a 25 cell stack to larger stacks for pilot scale demonstration (200 kW) in 2-4 years and engineering scale (1 MW) in 5 years. Good distribution of work effort between partners. Materials problems (as evidenced by the 25% cell degradation over a 1000 hr test) are questionable. Question proceeding with development of larger systems and demonstrations. Further examination of disassembled cell may uncover failure modes.
- Good overall plan for bench and integrated lab scale programs but additional detail on pilot scale and engineering scale program plans would be helpful.
- Approach to process R&D discussed at reasonable level of detail. Other laboratory and industry partner research contributing to potential process refinements. .
- A well-balanced R&D program was presented, including both experimental and computer simulation work. Good progression planning for the scale-up of proposed technology through different stages.
- Inclusion of CFD work should yield helpful results to guide R&D.

- Discussions for technical barriers could be expanded to include risks associated with the integrated operation of the sub-process blocks.
- The approach appears to be focused on addressing the barriers to commercialization. Very good focus on commercialization of the technology.
- There are synergies between this work and SECA program. Need to leverage SECA Program resources.

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

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

- This project has finalized engineering designs; solved sealing problems (or, so claimed); and, developed and demonstrated a 25 cell prototype producing 100 l/hr H_2. Project has also considered important operational parameters for the system (e.g., relative amounts of hydrogen and water needed in input and outlet streams).
- 1000 hr test was a good accomplishment but no explanation of 25% degradation is an issue and needs to be addressed soon before very much additional scale up using this design is done. Key milestones achieved since beginning of FY06. Believe that they have overcoming cell stack sealing issue identified last year. Discussed additional milestones planned for completion this year. Continuing work at small scales to investigate cell materials for improving process performance. Discussed details of small scale process experiments and measurement techniques. Discussed details of cell stack designs and testing results. Discuss progress against detailed testing objectives necessary to support longer term goals. Showed ILS 60 cell stack module and discussed plans for testing later this summer. Good improvement in cell stack performance since last year. Doing root -cause assessment of cell stack degradation experience - most important goal for this year.
- Good results were reported on the completion of the 1000-hour testing with 20-25 cell stacks.
- Pressure levels of the operating system and their implications were not adequately discussed.
- Excellent progress.

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

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

- Stronger collaboration with national laboratories and other solid oxide companies is recommended. Good collaborations with partners experience in solid state materials and electrochemical devices.
- Taking advantage of solid oxide fuel cell development work in design of electrolysis cell and stack design. Coordination with partners is evidenced by the successfully implementation of the button cell and cell stack tests completed.
- Good teaming arrangement with qualified collaborators.
- Broadening of participants to include nuclear power plant operating companies and more venders/developers of solid oxide electrolysis cells (SOEC). This should be beneficial to the project.
- Good collaborative effort – particularly with industry.
- Direct collaboration with DOE SECA program will enhance the chance for success.

Question 5: Approach to and relevance of proposed future research

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

- Planned activities and go-no-go milestones by accomplishment and schedule were not included in the presentation.
- Barriers need to be identified more sufficiently and demonstration of resolution with schedules presented as a precursor of moving into 2007.
- Concern that the system will not be commercialized.
- Good short-term plan but long term plan is perhaps too long and a more detailed intermediate plan is needed that addresses contingencies especially if degradation issues can not be resolved.
- Discussed long-term conceptual design of High Temperature Electrolysis plant coupled to a Nuclear plant and integrating process refinements being developed by other laboratory. Discussed design and plans for operation of the integrated laboratory scale test.

- No slides for future plan and milestone.
- The future work plan can include a system efficiency analysis to identify the focus areas which show most promise for improvements.
- Future plans represent logical progression of the effort.
- The future work was imbedded in the presentation but unclear about specific (tasks).
- Need to study thermal cycling impact to the seal and stack durability or borrow experience from SECA program.
- Need to better define the schedule to achieve bench scale, integrated lab and pilot test and eventually engineering demonstration by 2015.

Strengths and weaknesses

Strengths
- The concept has merit and some decent progress achieved.
- Good overall team to demonstrate high temperature steam electrolysis using nuclear heat. Designed and tested a 25 cell stack—a good accomplishment.
- Good progress was made, especially on 1000 hr test. Good and competent overall team.
- Well organized presentation with strong technical content. Well focused program concentrating on R&D efforts to overcome the technical barriers.
- Excellent Collaboration.
- Good systematic approach with clear vision of 5MW engineering demonstration by 2015.
- Use of CFD model to predict cell behavior is an excellent approach to speed up the development.

Weaknesses
- Question whether milestones were decided after the work was completed so success was embedded in the schedule and milestones.
- Project will not solve materials problems before proceeding on to a larger scale demonstration.
- Need better contingency plan if membrane performance degradation can not be resolved and if membrane area can not be sufficiently increased.
- Two of the key elements of the project are SOEC and ORNL membrane. The current status and development hurdles for these two elements were not adequately discussed.
- Use of the proposed HTE technology for applications in current nuclear power plants should be investigated and discussed.
- An economics analysis for the proposed technology should be added to provide R&D guidelines.

Specific recommendations and additions or deletions to the work scope

- Focus should be on materials issues for one year before moving on to a larger scale demonstration.
- Recommend more contacts with SECA and adding a contingency plan which may consider other SOE vendors or designs.
- Question why project is not moving forward as fast as technology development efforts (elsewhere).

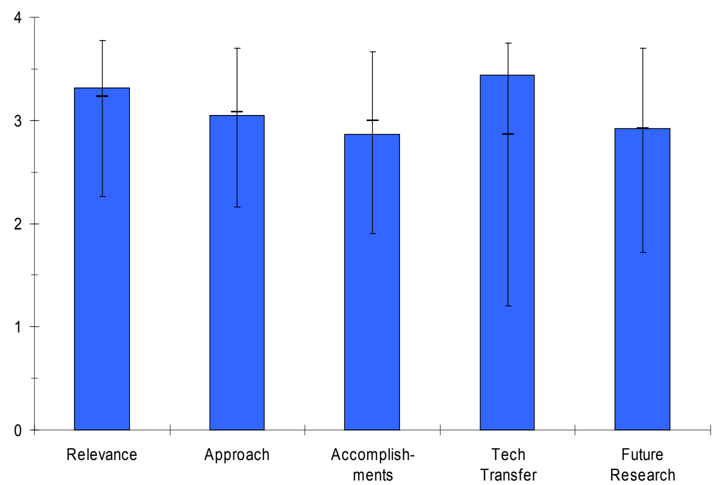
Project # PD-18: Nuclear Reactor/Hydrogen Process Interface

Steve Sherman; INL

Brief Summary of Project

The Interface with Nuclear Reactor project, otherwise known as the Systems Interface and Support Systems area within the DOE Nuclear Hydrogen Initiative, concerns the development of a thermal interface between a high temperature nuclear reactor and a hydrogen production plant; the definition and development of balance-of-plant components for a nuclear hydrogen production plant; and the definition of infrastructure and support systems requirements for a nuclear hydrogen production plant. All activities are directed towards developing the technology within the scope of the project to a sufficient level to support decisions concerning the pilot-scale and engineering-scale nuclear hydrogen production plants.

Overall Project Score: 3.1 (9 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The schedule for the high temperature nuclear reactor was not presented so there is almost no way to judge the relevance to the President's Hydrogen Fuel Initiative.
- This program does support the infrastructure and materials needs of the nuclear hydrogen program but needs better overall direction and coordination. For example, while a material test plan is still being developed material decisions on several thermochemical integrated designs have already been made.
- Materials issues and the interfacing of nuclear reactors to hydrogen thermochemical cycles are needed.
- If nuclear energy is to be used for the production of hydrogen, then the integration of nuclear safety and operational constraints with the processes is quite relevant to the overall Hydrogen Fuel Initiative.
- This program is tied to the Gen IV Nuclear Reactor and therefore is highly relevant in that context. However, it is at risk if the Gen IV program is put on hold or underfunded.
- Overall project objectives and FY06 objectives addressed and related to NHI overall objectives. Timeline, budget, barriers, and partners addressed. Although the R&D efforts support the major milestones, the deliverables for this project are not clear.
- The President has recently called for the increased use of nuclear energy to meet the Nation's energy needs, and this particular work provides a critical linkage for the utilization of nuclear energy in the production of hydrogen.
- This effort is absolutely critical if nuclear produced hydrogen is ever to become a reality.

Question 2: Approach to performing the research and development

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

- Clear approach and good integration of work between tasks, and between participants.
- Willingness to down select is a positive aspect of the project execution.
- Approach is still too vague and not specific enough even for a program that was described as a "catch all" program.

- There does not appear to be any fundamental basis upon which decisions are made regarding which materials are used where. It is doing a literature search, identifying materials, and testing them. It seems as if there should be a better method to determine what materials are needed and to possibly be ahead of the curve and to actually develop materials. In particular, emphasis should be put on coatings that can be placed on substrate materials for specific applications where there is an attempt to match thermal expansion of the substrate and where surface properties are suitable for process atmospheres.
- The approach contains both materials in the main heat exchanger and operational constraints in the separation of the nuclear plant and the hydrogen process.
- Sometimes the approach seems scattered because of the range of cross-cutting issues and the relatively low amount of funding.
- This program appears to be directed at over coming technical barriers while side stepping cost issues. If sufficient sophistication exists to link nuclear reactor codes with chemical process codes to assess steady state performance, then sufficient information exists to design a commercial scale plant and do a first cut on a cost estimate for construction.
- Partnered with laboratories, universities, industry and international organizations. Approach to materials, heat exchanger, and codes/safety R&D work discussed and tied into the long term project milestones coordinated with the thermochemical and high temperature electrolysis process development efforts. More detail is needed for the interfaces with the hydrogen production process development efforts.
- The approach outlined by the PI is solid and comprehensive. The long project timelines are the key to this; a broader view of the research and its approach is both possible and preferred in this case.
- The approach appears to be well thought out. It is encouraging to see collaboration with Framatome (AREVA) as mentioned in the presentation. It appears that several barriers to ultimate success are being addressed. There are additional barriers, however, that will need attention in the near future if NGNP schedules are to be met - e.g., the impact of transients (both ways – from the nuclear plant to the chemical plant and from chemical plant to nuclear plant). This transient analysis should involve input from commercial nuclear and industrial chemical facility personnel.
- It was not clear whether the secondary coolant has already been chosen. The choice certainly has impact on some of the research and design work being done on the heat exchanger.

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

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

- Down selecting has enabled focusing on fewer options and thus good success in advancing state-of-the-art.
- Some good individual accomplishments were reported but the overall results do not appear to be well integrated for such a large program.
- This project seems to be applied testing of available materials instead of development of new ones to solve specific issues. It seems as if there should be some protocol that is used to determine what is needed in terms of properties – i.e. certain temperatures, certain heating rates, certain thermal conductivity, certain chemical compatibility, certain thermal expansion, etc. Then some kind of scoring of available materials to determine what is suitable and what kinds of materials would need to be developed.
- The technical progress has been mostly in the modeling of heat exchangers, in filling gaps in the materials literature and in scattered international collaborations. There are several other areas that need to be addressed.
- Good technical progress has been made.
- Discussed results of material development tasks in support of the nuclear plant interface system and elements of the thermal chemical process heat transfer interfaces. Results from French CEA and university partner R&D testing efforts. Performance indicators supporting achievement of major milestones are not well defined. Not clear where the progress stands relative to achieving these major milestones.
- While important strides have been made in terms of the heat exchanger system design, most of the work for the first two years has been the study of corrosion processes that may impact the system. Of these, there has not been a strong indication by the PI that materials may be found that are readily available, inexpensive, and durable.
- It appears that good progress has been made in several areas. A caution – there seems to be a fixation on the heat exchanger. The heat exchanger is perhaps the biggest issue but it is not the only issue.

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

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

- International cooperation is a good strategy.
- Good focus on managing the many players and keeping each focused on project objectives.
- Good overall collaboration but more with industry would be helpful.
- Good interactive team.
- I assume that there is a direct connection between what is needed by SNL for the sulfuric acid decomposition and then what the PI does for this project. Also, since SNL is working on materials too, there needs to be an effort to make sure that none of the tasks overlap.
- There are a large number of collaborators, both domestic and international. However, the coordination among the collaborators is very difficult and the selection of activities seems to be driven by the ongoing research of the institutions involved.
- A large number of collaborators are useful only if their activities can be coordinated.
- Excellent team of universities and industry partners. There should be no problem transferring technology and bringing it to the market place.
- Partnered with laboratories, universities, industry and international organizations. Interfacing with high temperature reactor and GEN IV programs. Complex collaboration with partners and hydrogen production efforts appears well planned at a high level - some details of interfaces appear to require further development.
- The PI has shown that both communication and collaboration with the university and industrial partners has been strong and sustained. It is certain that this effort has had a great deal of positive collateral effect in education and building understanding of fundamental materials and design issues.
- Outstanding collaborative efforts are evident. Additional "team" members should be considered – if only in an advisory role.

Question 5: Approach to and relevance of proposed future research

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

- Very good focus.
- FY07 plan is more of a list than an integrated plan. Plan beyond FY07 is the overall NHI plan which is too vague.
- Plan appears limited. Why were only sulfur and Ca-Br chosen as the go forward cycles? At one time membranes for separations of gases in the cycles were part of the plan-have they been dropped?
- The safety issues of plant separation and of toxic material inventories need to be further addressed.
- The activities are all inherently tied to the Very High Temperature Reactor. Some consideration ought to be given to the use of other reactor types.
- Future plans primarily address technical issues associated with the sulfur family of thermochemical cycles. They need to address cost issues as well. Cost issues need to be factored into the decision points and are as important as overall efficiencies in determining the viability of this technology.
- Discussed plans for future materials development work. Discussed remaining work for FY06 and plans for FY07. Showed long-term plans for supporting pilot and engineering scale decisions for the thermochemical and high temperature electrolysis processes. More detailed plans are needed to ensure necessary support of these long term decision points, but their is sufficient time to establish these plans.
- The success of future work, especially work proposed for FY 2007, will depend strongly on the work to be completed in the remainder of FY 2006. The amount of overall project progress to date suggests that much of this work may run over into FY 2007, so the proposed work schedule suggested by the PI is likely over-ambitious.
- It appears that only minimal thought has gone into planning beyond the materials research required in the future. This may be a result of the project definition. There are other elements that must be addressed if a nuclear reactor is to be coupled to a hydrogen production plant. Licensing ramifications could be significant and some level of effort should be targeted for upcoming years.

Strengths and weaknesses

Strengths

- The PI appears to be a strong leader. He gets things focused, is open to new ideas, but appropriately down-selects concepts. Good emphasis on schedules with go/no-go milestones.
- PI has good technical qualifications. Some progress on a variety of areas was identified.
- Great interactive team!
- Knowledgeable of needs of SNL, etc.
- Includes several cross-cutting issues relevant to all nuclear hydrogen production processes and even to non-nuclear processes.
- This program has great strength in its partners.
- Complex collaboration with partners and hydrogen production efforts appears well planned at a high level - some details of interfaces appear to require further development.
- This project is critical for the eventual success of coupling nuclear heat to thermochemical hydrogen production. The team that the PI has organized is strong, and is working together well. Although progress has been slow, it is steady and will yield valuable results.
- Excellent collaborative efforts underway - build on it.

Weaknesses

- Additional planning and integration with the various NHI teams needs to be done. For example various leads working on SI, High Temperature Electrolysis, HyS should be collaborating with this project. Need to provide this team with a list of specific needs and agreed upon objectives with respect to materials, safety and other infrastructure studies.
- No scoring system is in place to help determine what materials are best for what needs. Also, there does not appear to be a program that is pro-active - i.e. developing new materials to solve identified problems.
- Several of the tasks are long-term university activities, which are themselves good work, but which are not really integrated into the overall design.
- The division between the reactor and the hydrogen production plant is sometimes unclear.
- Based upon the materials studies being undertaken at GA, UNLV, UC-Berkeley, and Ceramatec, this project appears to have a single minded focus on the S-I Cycle.
- Need some discussion on the plans for other balance of plant development efforts beyond heat exchangers and materials development. Provide more detail in the plans/deliverables to support the pilot scale and engineering scale decisions and beyond.
- Progress has likely been slow due to the materials issues that are bogging the team down. It may be helpful to seek the advice of outside experts that may assist with corrosion and materials stability studies.
- Project focuses on heat exchangers and materials - this is important but there is more to the nuclear/H2 plant interface that must be addressed. Perhaps not in the scope of this project, but consideration should be given to the licensing aspects -perhaps these might drive design considerations which may shift R&D priorities.

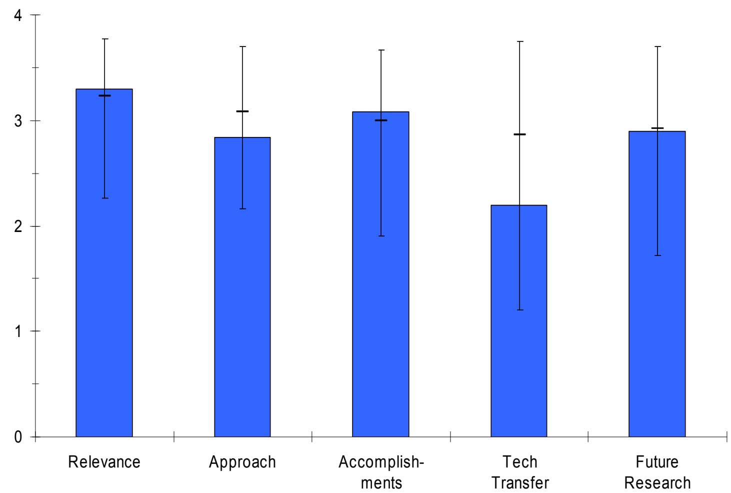
Specific recommendations and additions or deletions to the work scope

- Recommend better coordination and integration with various NHI process teams.
- Test protocol needs to be improved to look at gas stream compositions similar to coal gas.
- Atomic Layer Deposition of nearly perfect films is being developed to essentially design materials for compatibility. Adding expertise in ALD should be considered.
- This task needs to be more closely coordinated with the overall design of the Very High Temperature Reactor or of the other reactors that are being considered.
- More effort needs to be put into describing the heat transfer loop between the reactor and the thermochemical cycle. Will the loop be able to deliver the 900C heat needed for the S-I Cycle? More effort should be put into bringing along a lower temperature cycle as a back-up or contingency. Costs, in particular assessment of capitol costs for building a commercial-scale plant, need to be brought into this program as soon as possible.
- For the extensive project time frame, the scope of work is appropriate.
- If licensing is not in scope, consider at least some high level assessment of the licensing issues. Although project is somewhat embryonic, association with and personnel from the chemical industry and the commercial nuclear industry would be benefitial in an advisory role.

Project # PDP-02: Low-Cost, High-Pressure Hydrogen Generator

Cecelia Cropley; Giner Electrochemical

Overall Project Score: 3.0 (5 Reviews Received)



Brief Summary of Project

Giner Electrochemical Systems, LLC is developing and demonstrating a low-cost, high-pressure PEM water electrolyzer system with reduced capital costs, improved electrolyzer stack efficiency and increased hydrogen discharge pressure. This project will demonstrate a 3,300 scfd high-pressure electrolyzer operating on a renewable energy source. The objectives for the past year included developing lower-cost stack components, decreasing parts count per cell, and increasing operating current density.

Question 1: Relevance to overall DOE objectives

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

- Addresses the goals for hydrogen production from electrolysis of decreased costs and increased efficiency. Aligned with programs emphasis on reforming of natural gas and advanced electrolysis systems for short term.
- Project addresses critical issues related to electrolysis cost reduction.
- This project seeks to overcome the cost barriers for high pressure distributed hydrogen generation using PEM electrolysis. They will demonstrate a 3,300 scfd high pressure electrolyzer using a renewable source of electricity.

Question 2: Approach to performing the research and development

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

- The initial studies suggest that cost is minimized for this system at lower pressure H_2 production (1200-1500 psi). How pressure trades off with system simplification, performance, and costs needs to be explored further. Durability, especially with high H_2 pressures, has not been addressed and may have a large impact on H_2 cost. Cost study has catalysts as a minor part of system costs, and membrane substantially more than catalyst cost. From fuel cell studies would expect catalyst costs to be higher than membranes, even for substantially thicker membranes than are used in PEMFC.
- Reducing complexity of system through parts-count reduction.
- There is no discussion of trade- offs between membrane life / pressure / current density.
- Using a solid technical approach, this project seeks to reduce capital cost through reduction in cell parts count and examining mass production protocols. At present, there are 16 parts per cell; goal is 6 parts per cell. Worked with an automotive component supplier to design new thermoplastic cell frames and manufacturing methods. These and other efforts should reduce cell cost by 40%. Also looking for low cost cell separators. Biggest effort is on membranes to reduce thickness yet maintain mechanical strength. Project seeks to increase operating current density which will increase efficiency.

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

This project was rated **3.1** based on accomplishments.

- Have achieved a stack efficiency of 71%, however, still a ways to go to meet 76% stack efficiency target. Considerable progress has been made in lowering the parts count. Making progress in membrane.
- Demonstrated cell part count reduction from 40 parts in 2002 to 16 parts today. Good progress on membrane development.
- Making good progress in cost reduction and component minimization.

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

This project was rated **2.2** for technology transfer and collaboration.

- Collaborations not described. Not clear what collaborators are doing.
- No solid collaborations on this project; however, project is approaching vendors on mass fabrications issues, such as with the cell frames. Some interaction with GM.
- Should consider outside assistance for separator development and improved joining technologies.

Question 5: Approach to and relevance of proposed future research

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

- Development plans are good.
- Plans for demonstration are not firm.
- Solid plan going forward focusing on stack cost reduction and membranes. Will demonstrate low cost materials and fabrication in the development of a 10 cell stack. In FY2007 will fabricate a 3300 scfd system and will also conduct a field test using renewable (wind) energy.

Strengths and weaknesses

Strengths
- Logical evaluation of higher pressure operation.
- Good project – appears to be making progress in lowering cost.
- Solid approach to lowering cost of hydrogen production by decreasing system capital cost and increasing efficiency. Capital cost reduction will be achieved through reduction in cell parts count. Advanced membranes will allow operation at higher current density. Giner is a contractor with a solid technical track record.
- Good progress in reducing part count.

Weaknesses
- The PI should provide clearer presentations on cost reduction over time. Projection of stack life at high pressure and current densities should be included.
- Very difficult to see that they will be able to achieve hydrogen cost goals unless they use very inexpensive electricity (less than $0.035/kwh).
- Materials and/or manufacturing assistance may greatly reduce development time.

Specific recommendations and additions or deletions to the work scope

None provided.

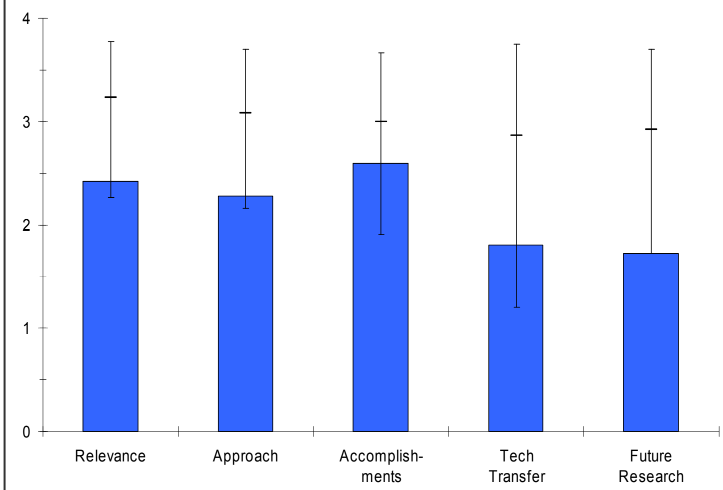
Project # PDP-06: Adapting Planar Solid Oxide Fuel Cells for Distributed Power

Andres Marquez; Ohio University

Brief Summary of Project

The goals of this Ohio Coal Research Center project are to develop more efficient and environmentally friendly methods of producing electricity using coal and to develop a distributed power generation source utilizing planar solid oxide fuel cell technology. The research in 2006 is focused on determining the carbon deposition resistance and sulfur tolerance of planar solid oxide fuel cell anodes.

Overall Project Score: 2.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.4** for its relevance to DOE objectives.

- The goal of this project is to develop solid oxide fuel cells for distributed power generation. It does not directly support the President's Hydrogen Fuel Initiative.
- This program does address looking at improvements to SOFC due to H_2S contamination but does little else to address the overall needs of the hydrogen program.
- The development of sulfur tolerant anodes are important for coal-derived syngas but the presentation didn't make an adequate case for how this work would be applied to developing sulfur tolerant psofcs.
- The project supports the generation of electricity via SOFCs and coal-derived syngas. While this is not in direct support of the President's Hydrogen Fuel Initiative, it does support the more efficient use of coal for electricity production.
- The technologies being developed are applicable to the production of hydrogen.

Question 2: Approach to performing the research and development

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

- The approach is to expand the triple phase boundary by using additives in the anode. This enhances reaction rate. Ceria is used as an additive to scavenge sulfur.
- Some work was done to improve the overall performance of SOFC membranes with respect to H_2S impurities but very little other progress or understanding on how these improvements fit in to the overall coal to hydrogen program.
- It was not clear how this work was would be moved forward to SOFC development.
- The approach seems more directed toward an Integrated Gasifier Combined Cycle coal-fired power plant than to distributed power.
- The approach is well thought out in terms of dealing with the sulfur content of the candidate coals.

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

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

- The funding for this project is not known. They showed that performance is stabilized after 500 hr, after sulfur is removed and reaction products formed. Approximately 20% degradation in cells after 1000 hr of testing in coal syngas. Very low power densities are reported-much lower than other groups.

- The PI has made good progress toward determining the composition of sulfur-tolerant fuel electrodes.
- It is unclear whether all ceramic interconnects containing platinum vias will be necessary for this cell configuration. If so, the Pt would make the SOFCs much more expensive.

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

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

- No collaborations reported or evidence of technology transfer activities.
- No other partners were identified.
- Not clear what the level of interaction with SOFCO is on the project.
- Very little technology transfer or collaboration is mentioned on the poster, other than an acknowledgement to sofco-EFS, presumably in connection with the cells.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.7** for proposed future work.

- None was discussed.
- None shown.
- No information provided about future work.
- The technology has promise, though in IGCC generation rather than in hydrogen production.

Strengths and weaknesses

Strengths
- Interesting work on anode additives to improve stability in presence of sulfur. Needs further investigation.
- Some improvement in SOFC membrane performance in the presence of H2S was shown.
- Good testing program which has demonstrated sulfur tolerant anodes.
- The PI has successfully developed SOFC anode materials for use with syngas containing hydrogen sulfide.
- Great Goals!

Weaknesses
- Project does not directly support the Hydrogen Fuel Initiative. No clear innovations over other efforts in this area (i.e., SECA).
- Did not follow review format. No funding listed. No future work identified. No comparison to other H_2S removal processes or turbine technology.
- An overall integrated program plan not apparent. What are the next steps? How will success in R&D lead to development phase?
- The connection with the hydrogen program is rather tenuous.
- Approach to reach goal; should be clarified.

Specific recommendations and additions or deletions to the work scope

- Discontinue funding for this project.
- Delete program or add to fuel cell program as a lesser activity.
- Need to develop an overall plan as to how this project if successful can be moved to the next step of development.
- Apparently this project has been completed, so additions/deletions are moot.

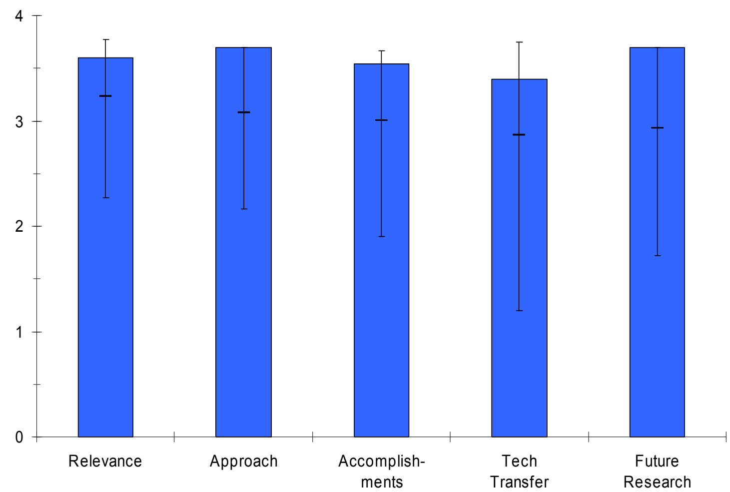
Project # PDP-08: Zeolite Membrane Reactor for Water-Gas-Shift Reaction for Hydrogen Production

Jerry Y.S. Lin; U of Cincinnati

Brief Summary of Project

The University of Cincinnati is studying fundamental issues related to synthesis of high quality, stable zeolite membranes and membrane reactor for water-gas-shift reaction and hydrogen separation. Activities include synthesis and characterization of chemically and thermally stable silicalite membranes, experimental and theoretical study on gas permeation and separation properties of the silicalite membranes, hydrothermal synthesis of tubular silicalite membranes and gas separation study, and experimental and modeling study of a membrane reactor for water-gas-shift reaction.

Overall Project Score: 3.6 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The technology could apply to a number of hydrogen production options. For example, it might be used in steam methane reforming, renewable liquids reforming, and gasification. Membranes likely resistance to S is an additional bonus.
- Single step high efficiency Water Gas Shift process is important to the development of low cost hydrogen production methods.
- Project addresses new processes that reduce cost and increase efficiency of hydrogen production from natural gas and renewable biomass resources.

Question 2: Approach to performing the research and development

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

- The program is a well integrated project addressing all key aspects of the development of the membrane reactors from materials and catalyst development to equipment construction.
- Strong combination of relevant experimental and modeling capabilities at four universities. Team clearly has the core capabilities to synthesize membranes, fabricate membrane reactors, carryout integrated reaction/separation evaluations, and compare results against engineering models of the system.
- Comprehensive approach with well balanced technical expertise.

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

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

- A bit too early to tell since no performance data has been generated yet.
- Good progress in the first year of the project.
- Progress in: synthesizing high quality silicaline layers and intermediate layers; pervaporation and gas permeation unit set up; synthesis and testing water gas shift catalysts; construction of facilities to produce tubular alumina reactors; and putting in place characterization tools for thin (membrane) layers.
- Multidisciplinary team making significant accomplishments.

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

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

Although there is outstanding collaboration among four academic institutions, should coordinate with potential industrial partners for future commercialization of the technology. An industrial collaboration may help replace outdated, inefficient, or costly purification technology used today with new separations technology under development.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.7** for proposed future work.

Strengths and weaknesses

Strengths
- Innovative technology with potential for breakthrough. Sulfur tolerance of the membrane. Versatility of the approach (can be used for separation only, WGSR, or reformer).
- A diversified team among multiple institutions bringing complementary skills to bear on the project. The level of coordination among the groups is impressive.

Weaknesses
- Potential mechanical issues with the material such as the ability of the final reactor to withstand vibrations.
- Difficulty with connecting ceramic membrane with the metal piping.
- No apparent industrial contacts as yet.
- The future work should have increased emphasis on durability testing including review of all types of contaminant issues. In addition, some work on corrosion of the membranes at elevated temperatures in the water gas shift environment should be considered to demonstrate robustness of zeolite membrane and catalysts.

Specific recommendations and additions or deletions to the work scope

- Consider adding an industrial partner, particularly in the areas where industry can provide existing solutions (such as in the catalyst area) as well as to help evaluate the materials under realistic industrial conditions.
- Consider building reformer from thermally stable materials thereby combining reformer-water gas shift reaction -separation in one step.
- The motivation the water gas shift membrane reactor is reducing overall cost of the water gas shift step in hydrogen production from hydrocarbons or alcohols by reducing the number of water gas shift reactor stages. The benefit cannot be realized if the membrane reactor system costs more that the multistage reactor system. This project should include an element that addresses the impact of the membrane reactor on overall system cost as a function of transmembrane flux and selectivity and membrane reactor costs. The model should include the cost of adding heat transfer functionality to the membrane water gas shift reactor (heat is usually removed between stages of Water gas shift reaction). This analysis would help the researchers set specific flux, selectivity, and cost targets that would define project success.

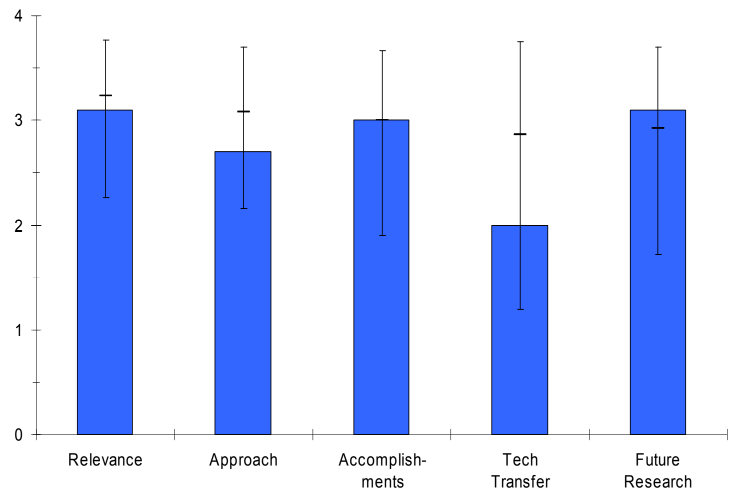
Project # PDP-10: Startech Hydrogen Production

David Lynch; Startech Environmental

Brief Summary of Project

Startech Environmental Corp. will field test integrated hydrogen production on a pilot scale using plasma gasification and ceramic supported carbon molecular sieve membrane hydrogen separation, and evaluate commercial viability and scalability through extended operation under representative conditions. The performance of the integrated Plasma Converter and StarCell Systems for hydrogen production and purification from abundant and inexpensive feedstocks will be characterized.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Conversion of municipal solid waste (MSW) to hydrogen is not in-line with DOE's emphasis on natural gas reforming, advanced electrolysis, and renewable technologies. Destruction of MSW and conversion to hydrogen is beneficial from two standpoints, reduce waste volumes and create a useful product. Economics will depend on electricity costs and credit taken for waste disposal.
- Ceramic membrane for purification could be substitute for conventional PSA if system can be developed.
- This project seeks to facilitate hydrogen production from abundant, low-cost feedstocks utilizing an innovative, unique plasma generation system and membrane hydrogen purification.
- Project seeks to demonstrate an integrated hydrogen production system based on plasma gasification and ceramic supported carbon molecular sieve membrane separator. This system can utilize inexpensive feedstocks like MSW.
- The plasma conversion approach should lend itself to high a degree of feedstock flexibility. The approach can utilize low value feedstocks.

Question 2: Approach to performing the research and development

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

- Impressive demonstration of hydrogen production from municipal waste on a fairly large scale. Discussion of the impurity composition coming out of the membrane purifiers would be useful. Purity needs to be substantially higher than what was demonstrated here. Need information on efficiency of conversion and H_2 yield per unit of electricity used. Comparison with thermal conversion technologies would be helpful.
- A bit scattered - inclusion of plasma work dilutes the focus on ceramic membrane separation system.
- The approach is focused and addresses established barriers to hydrogen production.
- The electrical power input requirement remains a concern.
- Potential hydrogen cost based on this approach was not adequately addressed.
- System feeds mixed gases from a plasma converter into carbon molecular sieve tubes. The tubes separate hydrogen product and residual gas streams. Membrane systems are supplied by Media and Process Technology. Economics of this process are highly suspect, as is the purity of hydrogen obtained.
- The program could benefit with better integration with other ongoing hydrogen purification and membrane reactor programs.

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

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

- Demonstration shows some potential but there is a lot to do, including efficiency measurements, etc.
- Ceramic membrane testing should continue.
- Significant technical progress was made this year – the production parameters, including hydrogen purity, have been experimentally determined.
- Project demonstrated that hydrogen rich gas could be produced and purified to 96% on a reasonable volume scale. Hydrogen recovery rates of greater than 80% are reported. Membrane performance was tested in the laboratory and potential poisoning effects from gases such as hydrogen sulfide, ammonia, and others in coal and MSW evaluated following more than 2 months of exposure.
- The current effort showed good conversion of simulated MSW and coal to syngas. Limited details were presented on the details of the operating conditions of the plasma converter.

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

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

- Industry-led.
- A collaboration was mentioned, but no detail was given.
- No obvious collaborations.
- The prime works with a partner on the membrane separation but other sharing of results or collaboration with other investigators was not evident from the poster.

Question 5: Approach to and relevance of proposed future research

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

- Focus on ceramic membrane system is appropriate.
- Proposed future activities appear to be reasonable and appropriate.
- Detailed tasks were identified on a realistic schedule.
- Preliminary hydrogen cost estimates should be included.
- Project will complete system enhancements such as increasing the operating temperature and implementing counter current gas flows. This would enhance hydrogen yield. Phase II testing will be completed and a final report issued.
- The proposed future would build one current results, but additional work could be done to investigate further process optimization of the plasma converter, and to investigate incorporation of other operations to boost hydrogen yields, e.g. Additional of water gas shift reactors to convert CO.

Strengths and weaknesses

Strengths
- A potential alternate approach for hydrogen production using coal and/or waste feedstocks is useful.
- System can use inexpensive feedstocks, like MSW.
- The poster addressed concerns from the prior review. The product gas had very low concentrations of the contaminants that were examined. The plasma conversion process should enable the use of a variety of feedstocks for hydrogen production, potentially many very low value streams.

Weaknesses
- Need to provide much cleaner hydrogen.
- Electrical input requirements would tend to reduce total system energy efficiency when the produced hydrogen is used as an energy-producing fuel in transportation or power applications.
- The hardware may have high capital costs resulting in high hydrogen costs.

- Lifetimes of plasma generator and membrane purification subsystems may be limited.
- They have not addressed economics and cannot make a projection on cost of hydrogen produced. Even with 2 stage purification system can only get to 96% hydrogen gas purity.
- It does not appear that adequate efforts were made to share the results of the work. It does not appear that the investigators have collaborated with other hydrogen projects, it could potentially benefit from collaboration with others working on separations and separative reactors.

Specific recommendations and additions or deletions to the work scope

- Focus more on the ceramic membrane system, including engineering and operation.
- Add meaningful preliminary cost estimate as soon as possible.
- Minimize electrical input requirements.
- Address economics of hydrogen production and purity issues.

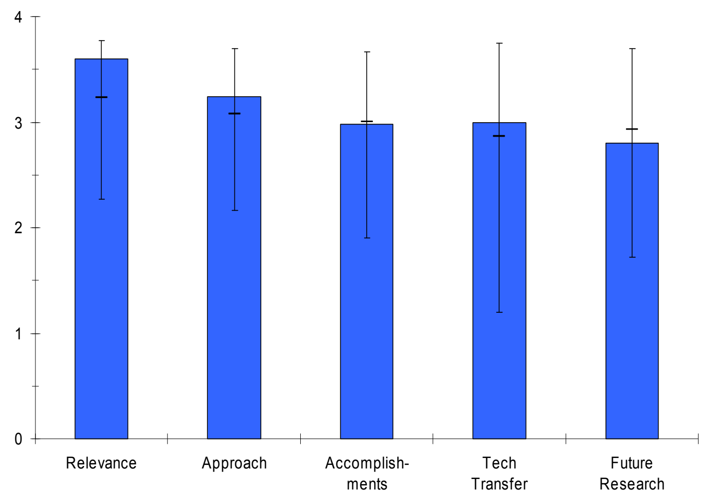
Project # PDP-12: Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen

Liwei Xu; Midwest Optoelectronics

Brief Summary of Project

By the end of this 3-year project, Midwest Optoelectronics LLC will develop and demonstrate tf-Si based photoelectrochemical systems with 9% solar-to-hydrogen efficiency with a lifetime of 10,000 hours and with a potential hydrogen cost below $22/kg. Two approaches are taken for the development of efficient and durable photoelectrochemical cells: an immersion-type PEC cell and a substrate-type PEC cell.

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Renewable hydrogen production is an important long-term goal of the program.
- The project involving the design and fabrication of photoelectrochemical cells for production of hydrogen is directly in line with the HFI.

Question 2: Approach to performing the research and development

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

- Multiple approaches and configurations are being developed.
- In general they focused on a single technical barrier in the design of a photochemical cell that can operate in an electrolyte solution with limited corrosion. The approach could be improved by establishing defined goals and targets with respect to durability and developing a basis for the overall device cost.

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

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

- Progress has been slow with 2 of 3 years of program completed. The project has been under funded according to award.
- Only a very small portion of the funding has been received, limiting the progress to plan.
- Good progress.
- The project appears to be well thought out and progress is being made. The STH efficiency value reported for the technology is reasonable at this stage, however the presentation lacked clear performance standards with regard to durability and this could be analyzed on the basis of number of cycles the system has sustained. It would be very useful to develop a comparative basis for this technology. In addition, the basis for cost needs to be developed.

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

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

- Multi-junction cells using (primarily) inexpensive materials for solar collection.
- The project did not appear to coordinate with other efforts. Efforts toward this may be beneficial in developing a comparative basis for performance assessment.

Question 5: Approach to and relevance of proposed future research

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

- Additional funding to the expected level is needed to move this project forward as expected.
- The future work plan was vague and focused on the improvement of coatings to limit corrosion of the surface of the photoelectrochemical cell. The goals for future work were very vague and could be strengthened by developing clear goals that in some manner relate to DOE targets for hydrogen production and durability. Goals for device fabrication could be addressed in terms of cost and performance.
- May want to assess some of the process issues for low cost production.
- Need to focus more on durability and efficiency to down-select the configuration options.

Strengths and weaknesses

Strengths
- The project is focused on innovative materials and approaches.
- A working photoelectrochemical system is in place and the general goals of the project to develop an effective working hydrogen producing device are sound.
- Good Project.
- Excellent partnership with NREL and United Solar.
- Solid experimental work based on the fundamental understanding of basic science.

Weaknesses
- The focus is on materials and not enough work on the system or durability testing.
- Severely-reduced funding (compared to contractual plan) is a serious detriment to project.
- The project lacks clear goals and targets in future work which does not allow the practical merit of the project to be realized.

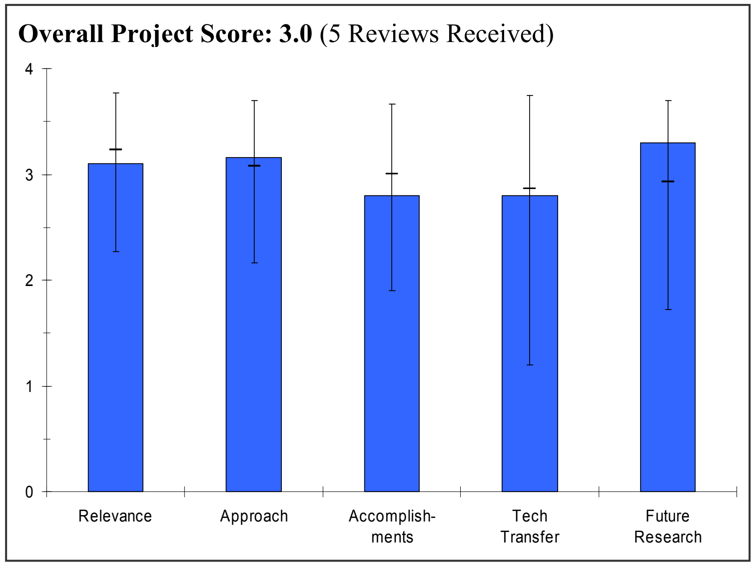
Specific recommendations and additions or deletions to the work scope

- Develop plan for increased focus on system and durability testing.
- Develop clear goals and targets for durability, performance, and costs that allow the developing technology to be evaluated in the context of DOE goals and competing technology.

Project # PDP-13: Development of Water Splitting Catalysts Using a Novel Molecular Evolution Approach

Neal Woodbury; Arizona State U

Brief Summary of Project



Arizona State University and the Biodesign Institute are developing a library-based solid-phase synthetic method for molecular evolution of a catalyst for electrolysis. Such a catalyst will be evolved using metal binding peptide libraries based on photosynthetic complexes and will then be optimized for minimum overpotential.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Very preliminary study of catalysts for peptide synthesis.
- Relevant to long-term objectives.
- The project is very fundamental and its benefits in application and implementation will have to be realized in the long term. Within the context of the development of water splitting, technologies could be applicable.
- The project goals to develop a high throughput method for screening improved hydrogen catalysts are completely aligned to the President's HFI objectives.

Question 2: Approach to performing the research and development

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

- Too early in this project to assess probability of viable hydrogen production.
- Interesting approach for screening many potential catalysts.
- The project is well conceived and on an incremental basis the approach is sound. The basis for design of metal/Mn binding peptides is unclear in that it is not clear if the peptides are designed on the basis of the biological water splitting complex or single Mn binding sites. It is unclear whether sites capable of a concerted 4e oxidation can spontaneously assemble. It seems unlikely that metal sites with one or two Mn ions could accomplish this.
- The project is designed to specifically address improvements in hydrogen production system efficiency, using advanced combinatorial technologies and molecular modeling. The project presents an outstanding approach for designing an addressable solid-phase, screenable platform for improved metal-bound catalysts, and the key component of photosynthetic function. The success of this approach will result in significantly improved catalytic efficiency.

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

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

- It appears that at this stage in the project more questions than technical answers have been identified.
- Variability in electrode performance may mask role and performance of catalyst in the synthesis process.
- Project is just underway.
- This area of the project is difficult to review and perhaps is not applicable. There is no basis at this stage of the project to put the developing technology in the context of any of these standards. The approach is sound but at

this stage it is not clear if an effective technology that can be implemented will be developed. The accomplishments presented thus far are significant and far reaching. The ability to screen peptide arrays in a high throughput manner for various activities could have broad applications.
- The project has been active for almost a year, and the investigators have made good progress towards developing the correct addressable platform for arraying the metal-binding peptides. The modeling of specific metal-binding peptides to predict catalytic properties and iterate improved versions of the chip is appropriate. Technical barriers in peptide elongation and masking have been addressed, although no evidence was presented that cross-talk has been eliminated.

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

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

- Some collaboration was reported, but the role of the partner in supporting hydrogen production was not clear.
- Excellent incorporation of students and post-docs.
- Very limited collaboration in the project and this could be strengthened perhaps by adding a component that investigates the implementation of the resulting technology.
- The university investigators have selected good collaborations with the appropriate industrial partner for the electrode chip design, as well as with the other university researchers performing the molecular dynamic modeling.

Question 5: Approach to and relevance of proposed future research

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

- Proposed future work seems appropriate, given the very early stages of the research.
- Good approach to moving beyond screening.
- The future plan seems sound although it could be strengthened by more detailed thought to peptide design and diversification of the metal complement to encompass complexes that would be more likely to accomplish the redox chemistry (water splitting). Goals are not specific and contingencies and optional paths were not presented.
- The future work plan is clearly laid out, with goals and objectives logically planned. The few technical barriers remaining have been presented with proposed experimental solutions and contingencies where appropriate.

Strengths and weaknesses

Strengths
- Combinatorial approach to catalyst selection is useful given the idea that catalyst performance can be separated from electrode effects.
- Creative approach to fast screening.
- A nice project with significant fundamental merit. The peptide array technology being developed, if successful, could have broad and far reaching application.
- The strengths of this project are the combination of expertise of the research team, and the clear, logical work plan. It is very clear who is responsible for which work component, and the timeline is very well-defined. Dr. Woodbury's extensive experience in working with photosynthetic complexes and modeling metal cofactors is clearly the driving force of this work, and he displays significant enthusiasm for the project. The approach of combining cutting-edge combinatorial peptide evolution with electrochemical detection is creative, and the data presented indicate that it is feasible. The investigator clearly understands where improvements are necessary in the research plan, especially regarding sensitivity of the signal and efficiency and fidelity of the peptide elongation. The choice of the cyanobacterial system is sensible, given that the majority of the crystallographic evidence used to populate the molecular modeling program comes from cyanobacterial proteins, and there is also flexibility in the experimental regimen.

Weaknesses

- Ultimate scale-up of the process to significant levels of hydrogen production may be difficult.
- The specific context of being able to generate water splitting with this approach is uncertain. Limited collaborations with groups outside of the university.
- The only weakness in this project is the lack of description in the interpretation of the voltammetry data. It is not clear from the information presented exactly how the investigator will extrapolate from and synthesize the information from changes in metal-binding titration curves to generate predictive models for catalytic function.

Specific recommendations and additions or deletions to the work scope

- There appears to be many unknowns related to this approach - an assessment of probable success including scale of hydrogen production and associated costs would be helpful prior to continued investment in this process. Is this simply a photo synthesis process looking for a practical application?
- Develop a more concrete plan for peptide design and modifications. Develop collaboration with synthetic model chemist(s) to explore the binding of synthetic clusters into peptide fragments.

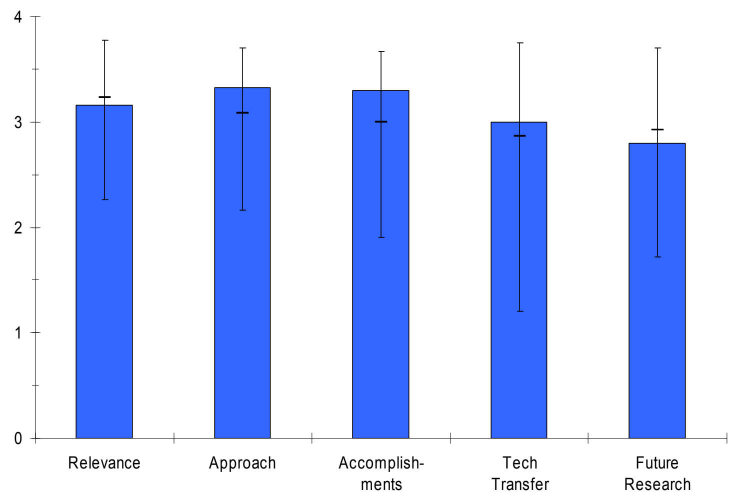
Project # PDP-15: Fundamentals of a Solar-thermal Mn_2O_3/MnO Thermochemical Cycle to Split Water

Alan Weimer; U of Colorado

Brief Summary of Project

The University of Colorado is researching and developing a cost effective Mn_2O_3/MnO solar-thermal thermochemical cycle through theoretical and experimental investigation. A preliminary process flow diagram has been developed based on literature information, and the economics of the process is being studied. Experiments are underway to validate each step in the cycle. The process flow sheet and economic evaluations study will be updated continually as process understanding is developed.

Overall Project Score: 3.2 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The research, if successful, would be quite relevant to the production of hydrogen using solar energy.
- The research is in a very preliminary stage.
- This represents a long-term renewable process for hydrogen production.
- This program does address the need to investigate and develop new hydrogen generation processes.
- There are some questions on whether this is the best approach as compared to other thermal chemical processes being pursued by the NE program.
- This project addresses four barriers in the DOE's multiyear research plan. Those barriers are: high-temperature thermochemical technology, high-temperature robust materials, and coupling of solar energy and thermochemical cycles.
- Program directly addresses a fundamental goal of the DOE initiative to produce hydrogen from renewable resources.

Question 2: Approach to performing the research and development

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

- The cycle has the advantage of operating at a somewhat lower temperature than other cycles and at about one atmosphere. The cycle is nevertheless, fairly complicated.
- The cycle requires the handling of solids and the recovery of NaOH.
- Good academic approach.
- Operation at reduced pressure may introduce additional safety, economic and operability issues.
- The PI has identified many of the technical barriers and appears to have a systematic plan to address these barriers.

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

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

- Only literature searches and preliminary experimental tests completed to date.
- Several challenges have been identified.

- Good progress was made within the past year despite limited funding.
- Preliminary process flow-sheet developed. Identified the research needs. Experimental work is underway for major research areas.
- Well thought out exploration of an alternative thermal cycle that operates at relatively low temperatures, thereby broadening ranges of materials that can be used to house the solar thermal reactor.
- Good combination of modeling and experiment to identify key technical hurdles in this approach (corrosive character of NaOH; efficient separation of NaOH/Mn_2O_3).
- Process simulation used to generate screening level economics and identify areas to reduce cost.

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

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

- Collaboration with ETH-Z builds on expertise with solar thermochemical cycles.
- Academic collaboration with Swiss institute brings additional resources to the project.
- Additional collaboration with National Labs doing nuclear-based thermochemical water splitting would be helpful.
- I am not clear what the Swiss collaborator has contributed to this project at this stage.
- Collaboration with Swiss Federal Research Institute. Would be beneficial to identify some private sector collaborators to help validate flow sheet and economic analysis and assist in materials development.

Question 5: Approach to and relevance of proposed future research

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

- Identified technical challenges need to be solved.
- Material compatibility and system integration and operation issues will need to be addressed.
- Future issues and challenges have been identified and possible solutions have been mentioned but a detailed plan or methodology for overcoming these challenges is not obvious. A more specific future work plan is needed.
- Required mandatory slide on future plans is missing. PI mentioned that water removal is a problem and wanted to focus work on how to handle the water issue.
- Given limited amount of funds available, the program plans appropriately focus on a few potential showstoppers, i.e. optimizing hydrogen generation step and looking at mixed metal oxide alternatives to Mn2O3.

Strengths and weaknesses

Strengths
- Project may lead to a solar thermochemical cycle operating at somewhat lower temperatures and at atmospheric pressures (or below).
- PI has good chemical processing experience and expertise. Good progress was made despite limited funding.
- Good chemistry. Good work for the small amount of money. Very good return on investment.

Weaknesses
- The process is relatively complicated and may require the recovery and/or conversion of NaOH.
- At some point in the development of this cycle, an evaluation should be made as to whether the benefits of somewhat lower temperatures and pressures are worth the complexity challenges.
- Needs better coordination with NE Thermochemical Water Splitting Programs.
- How to concentrate NaOH in this process? No plans given to address this issue.

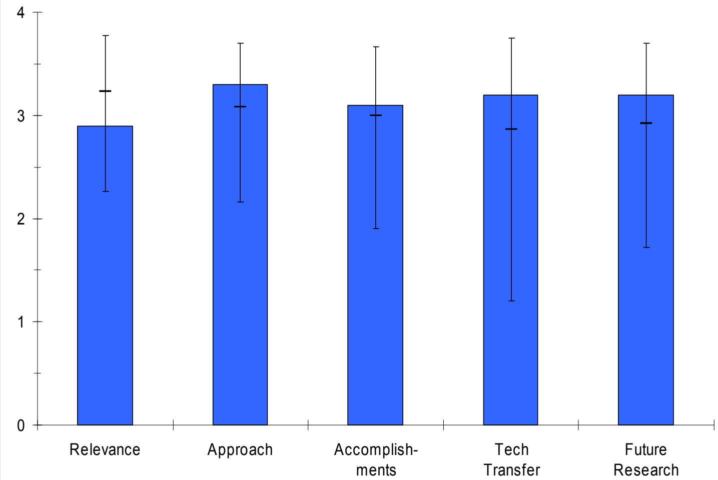
Specific recommendations and additions or deletions to the work scope

- For the next year, continue as planned.
- Recommend that the program look at lower temperature thermochemical cycles that are being looked at in the NE program. Even though this program has focused on somewhat lower temperature cycles (<1300C) cycles less than 1000C are needed to lower the cost of materials needed for future deployment of these systems. Recommend linking program with NE programs.
- Put emphasis on concentrating NaOH. Solve NO_x mitigation problem.

Project # PDP-16: Hydrogen Embrittlement of Pipeline Steels: Causes & Remediation

Petros Sofronis; U of Illinois

Overall Project Score: 3.1 (5 Reviews Received)



Brief Summary of Project

The goal of this University of Illinois project is to develop and verify a lifetime prediction methodology for failure of materials used in pipeline systems and welds exposed to high-pressure gaseous environments. The approach integrates mechanical property testing at the microscale, microstructural analyses, transmission electron microscopy observations of the deformation processes of materials at the micro- and nano-scale, first principle calculations of interfacial cohesion at the atomic scale, and finite element modeling and simulation at the micro- and macro-level.

Question 1: Relevance to overall DOE objectives

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

- Hydrogen embrittlement has been studied for decades and the failure mechanisms are well understood.
- Pipeline systems, including hydrogen pipelines, have been employed in industrial applications for many years.
- Aligned with DOE goals and objectives. Addresses barriers to hydrogen delivery.
- This project addresses barriers such as hydrogen embrittlement of pipelines, assessment of hydrogen compatibility of the existing natural gas pipeline system, and selection of suitable steels, and/or coatings to provide a safe and reliable hydrogen transport system. This is a very important project.
- H_2 delivery by pipeline will play a very important role in the H_2 fuel initiative. This study is aimed to help improving the H_2 pipeline safety and reduce the H_2 transport cost.

Question 2: Approach to performing the research and development

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

- A systematic approach, both computational and experimental, to studying this problem has been developed. The question is does this duplicate many previous studies having the same goals and objectives?
- Project was sharply focused on modeling hydrogen embrittlement and potential effects of trapping sites.
- The work is focused and includes modeling and supporting experimentation including SEM analysis.
- Uses finite element modeling and actual experimental work to determine the root cause for the embrittlement problems in pipeline materials.
- A sound approach was proposed to include both experimental and simulation work, which complement each other.
- The study will also include new materials to be used for future H_2 pipelines.

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

This project was rated **3.1** based on accomplishments.

- Progress has been made during the past year – however progress has been constrained due to lack of adequate funding.

- Modeling and parallel experiments are progressing and already elucidating the boundaries.
- Four out of five original tasks were completed. Reduction in funding forced the PI to drop the fifth milestone from their original plan. Considerable amount of work is done for a small amount of funding. Results are very useful for people dealing with hydrogen infrastructure development.
- Good start has been made in both experimental and computer simulation areas.

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

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

- Meaningful collaborations have been established.
- Collaborations are broad and strong.
- Industrial gas companies and two national labs are listed as collaborators but their roles are not defined clearly.
- The role of industrial participants in the project was not adequately discussed.
- How would the results benefit the industry? This question was not addressed adequately.

Question 5: Approach to and relevance of proposed future research

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

- The future work plans are dependent on substantial increase in project funding.
- Future work plans address the needs pointed out by the modeling effort.
- Future work will provide valuable physical data on candidate pipeline materials and new insights through ongoing modeling.
- Excellent future plans and objectives for long term research.
- A breakdown between the efforts on experimental and computer simulation work was not provided.

Strengths and weaknesses

Strengths
- Good modeling and experimental capability. Easy access to good students to do experiments and modeling studies. High rate of return for small investment of funds.
- Excellent Principal investigator (PI) qualifications and program planning.

Weaknesses
- Funding for this project appears to be below the critical minimum - as currently funded the probability of this project producing useful results is low. Existing expertise in industry (and the National Labs), who have worked on understanding this problem for years, should be used to address this barrier.
- It appears that the PI's work is not linked to other activities going on at DOE.
- No specific arrangements were proposed to develop a closer working relationship between PI and industry which can speed up the transfer of know-how from this work. to address:
    - Can the safety of the existing pipelines be improved with improved monitoring (e.g., improved sensors developed based on the results of this work)?
    - Can future pipelines be designed in a way which is less conservative than the current system and can lead to a reduction in capital cost?

Specific recommendations and additions or deletions to the work scope

- Any available funding for this project should be redirected to allow the project team to make a meaningful contribution to the resolution of other material-related barriers to production and delivery.
- Full funding should be restored to carry out the planned activities. Great potential to get some very valuable information from this proposed research.

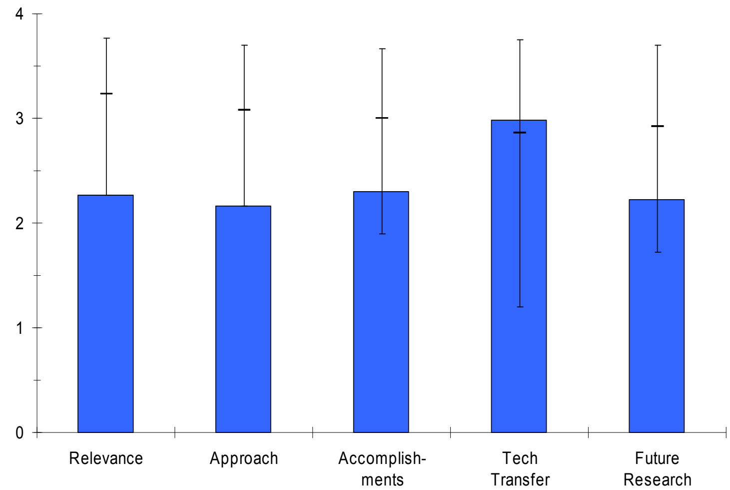
Project # PDP-17: Hydrogen Regional Infrastructure Program in Pennsylvania

Linda Eslin; Concurrent Tech. Corp

Brief Summary of Project

The Concurrent Technology Corporation program is undertaking R&D in hydrogen delivery, hydrogen storage, and hydrogen sensing. Specifically, these R&D areas include new and advanced materials for use in hydrogen pipelines and off-board compressed gas storage tanks, including testing and modeling of their lifetime performance; modeling of flow stratification in pipelines; gas separation technologies to separate hydrogen from its mixtures with natural gas; and hydrogen sensor development for leak and bulk concentration determination. Additionally, a hydrogen delivery tradeoff study is being conducted to determine the most attractive approach(es) for delivering hydrogen fuel, using Pennsylvania as a case study.

Overall Project Score: 2.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.3** for its relevance to DOE objectives.

- Methodology may be applicable to other states or regions.
- Investigators are looking at some relevant questions.
- More focused on the needs of Pennsylvania than national interests which is really the focus of the President and DOE?

Question 2: Approach to performing the research and development

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

- Significant amount of the work duplicates other projects in delivery program.
- Redundant with several other DOE projects. Unclear besides the Penn focus what the project adds.

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

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

- Collection and evaluation of information is reasonably thorough.
- Some progress since last year.
- Pipeline separation work was well done.
- Would be helpful to see existing and legacy piping material actually undergo rigorous accelerated pressure/heat testing in the presence of high pressure hydrogen. Paper study is good start but does not justify the level of government support.

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

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

- Industry partners are active in hydrogen business.

Question 5: Approach to and relevance of proposed future research

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

- Goals do not seem to be clearly spelled out.

Strengths and weaknesses

Strengths
- Strong effort to make program relevant.
- Delivery scenarios for a real geographic region provide good check for more idealized scenarios.
- The project covers a lot of interesting topics.

Weaknesses
- Not a good value. Funding is breath-takingly high for what has been accomplished.
- Analysis supporting objectives "Quantify tradeoffs between alternative hydrogen (H_2) production and delivery approaches" and "Determine most economic delivery scenarios. . ." was limited in scope: study was of H_2/CH_4 gas mixtures without reference to alternative carriers such as ammonia, methanol, various other hydrocarbons, and so on. – Project's breadth makes it difficult to achieve significant discovery in any individual technical area (e.g., separations, pipeline metallurgy, and sensor development).
- The progress against the goal is unclear – many topics – unique work should be identified.

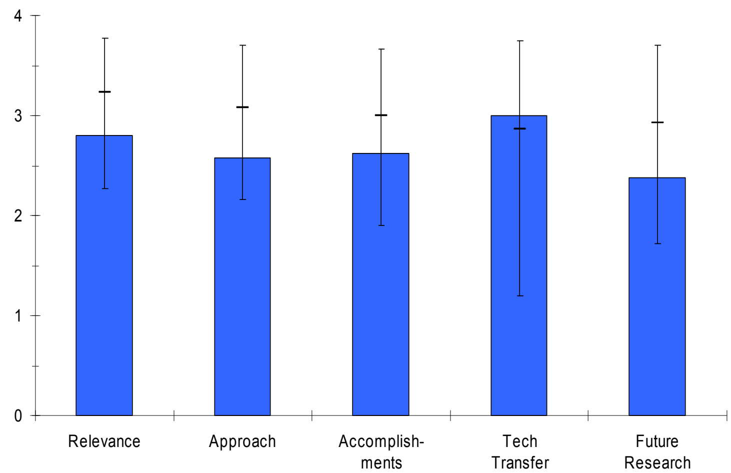
Specific recommendations and additions or deletions to the work scope

- Suggest changing approach to more different energy/fuel clearly define project sources may require a change to H_2 production.

Project # PDP-18: Developing Improved Materials to Support the Hydrogen Economy

Michael Martin; Edison Materials Tech Center

Overall Project Score: 2.7 (4 Reviews Received)



Brief Summary of Project

The Edison Materials Technology Center solicits and funds hydrogen infrastructure related projects that have a near term potential for commercialization. The subject technology must be related to the DOE hydrogen economy goals as outlined in the multi-year plan titled, "Hydrogen, Fuel Cells and Infrastructure Technologies Program Multi-Year Research, Development and Demonstration Plan." Preference is given to cross cutting materials development projects that lead to the establishment of manufacturing capability and job creation.

Question 1: Relevance to overall DOE objectives

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

- The relevance is difficult to assess because of the breadth of the program. Some of the projects funded by EMTEC are relevant to DOE Hydrogen Program objectives, and others are less so. The EMTEC portfolio of projects leverages multiple funding sources and addresses multiple stakeholder objectives, of which DOE is one.
- This project contains a number of small tasks in general support of the Hydrogen Economy. The individual tasks do not appear to be coordinated.
- Some of the tasks support the needs for instrumentation and for manufacturing technologies, area that have received relatively little emphasis in the overall DOE program.
- This program focuses on funding projects near commercial readiness. It fits well into the H_2 fuel initiative.

Question 2: Approach to performing the research and development

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

- Simply running a solicitation is a duplication of DOE efforts.
- Mixture of projects is a hit-and-miss group of unrelated projects.
- Cross cutting materials approach is unique to this project. Commercialization/manufacturing focus is also fairly unique to this project and is a valuable addition. Leveraging multiple funding sources is a plus. The breadth of technologies suggests that there is not a strong focus area for the project. It is not clear if resources are too diluted to have a strong impact.
- The poster presentation did not go into enough detail on any individual task to judge its approach. (Eight of twenty tasks were highlighted; the remainder were listed by title only.) However the summaries and oral explanations indicated that the general concept.
- The individual tasks were instituted or chosen for funding in a coordinated manner or in a way to specifically address particular DOE targets or barriers.
- The project is serving as an incubator for preliminary concepts in general support of the Hydrogen Economy.
- Some discussions were presented for the technical barriers to be overcome, but not with great details. This can be due to the large number of projects included in this poster.

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

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

- Little obvious connection to program goals.
- Very little technical information on each funded project was presented, so the technical accomplishments are difficult to assess individually. As a program, it appears that technical progress is being made, but it is difficult to assess its relevance to meeting the DOE targets based on the limited nature of the poster. I believe progress is being made and that innovation is fostered through this project.
- Not enough information was presented on the individual tasks to judge progress toward specific DOE goals.
- Highlighted accomplishments were presented for selected projects. The emphasis on near term commercial deployment was well addressed.

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

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

- Numerous companies engaged.
- No work with others in DOE program.
- It is difficult to assess collaborations for individual projects funded by EMTEC. However, the focus of the program is on technology transfer and leveraging multiple resources.
- The project fosters wide-ranging collaborations and technology transfer.
- The technology transfer/collaboration was well cultivated in the program.

Question 5: Approach to and relevance of proposed future research

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

- See little that will contribute here.
- Not much detail was given here.
- Each of the individual tasks described in the poster presentation is of short duration (1-2 years). Therefore the plans for future research are to commercial the products and move the funding to other ideas.
- A brief summary of future plan was presented, but without adequate details. Again, this is due to the large number of projects included in the poster.

Strengths and weaknesses

Strengths
- Product development and near-term commercialization approach. Much enthusiasm for project shown by PI. Leverages multiple funding resources.
- The project encourages the development of many, small-scale concepts to a point where they might apply for more substantial funding or be commercially viable.

Weaknesses
- Little relevance to program.
- These grants would have been better done by DOE—not contractor.
- Management of program is not transparent. Technical accomplishments are difficult to assess individually based on limited data available on poster.
- The individual tasks do not appear to be coordinated with one another.
- The selection process for the individual tasks is not clear.

Specific recommendations and additions or deletions to the work scope

- Project application and selection process should be coordinated with overall DOE goals.

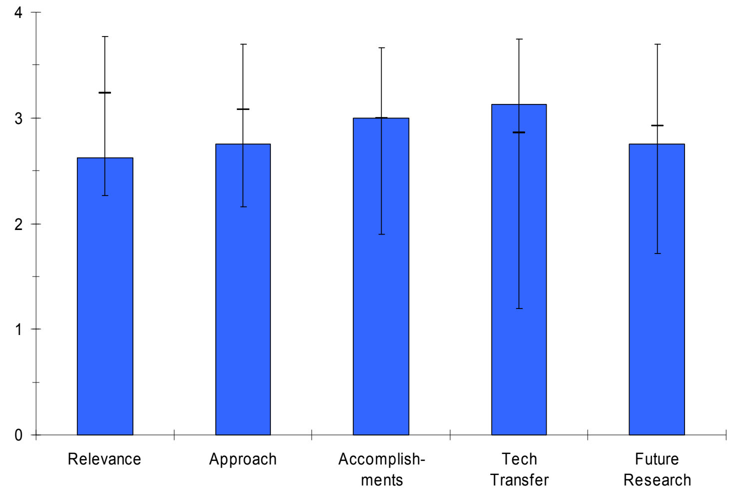
Project # PDP-31: EVermont Renewable Hydrogen Fueling System

Tom Maloney; Northern Power Sys.

Brief Summary of Project

EVermont, along with Proton Energy Systems and Northern Power Systems, is assisting DOE in the development of hydrogen production technologies by building and testing a validation system. Objectives of the project are to develop an advanced PEM electrolysis fueling station that utilizes renewable electricity sources; reduce cost of hydrogen production; improve electrolyzer efficiency; improve fueling station integration and controls; utilize hydrogen fueled vehicles for testing and validation; show viability of distributed production pathway; and gain experience and document performance of the use of the fuel in a vehicle.

Overall Project Score: 2.9 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- This is mainly a demo project and does not appear to be in line with H_2 Production R&D goals and objectives. There is relatively little novelty in the project.
- Demonstrates distributed hydrogen production, storage and delivery for vehicle refueling.
- System operation under cold-climate conditions is envisioned.
- Similar facilities exist and are in operation.
- Project will develop and test an advanced PEM cell stack and integrate with wind power electricity source. Will use this system to demonstrate along with a hydrogen fueled hybrid (ICE) vehicle.
- Electrolysis based on wind power provides hydrogen from renewable energy.

Question 2: Approach to performing the research and development

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

- Not a lot of new elements in this project.
- This project is nearly complete and will be finished by the end of fiscal year.
- Hydrogen production by electrolysis, using grid electricity, has been demonstrated on-site storage and dispensing will allow hydrogen vehicle refueling.
- This project is using a real-world demonstration to define operation characteristics of PEM electrolyzer systems. This information will be utilized in electrolyzer refinement, resulting in lower cost of hydrogen produced. Key to reducing hydrogen cost is reduction in capital cost and increasing efficiency. In the demonstration, the project uses three, thirty four cell stacks. The demonstration will provide important information about cold weather operation. Demonstration will provide 12 kg H_2/day.
- The approach is straightforward—design, build, and test an advanced electrolysis system operating on renewable electricity.

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

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

- The only progress shown is some improvement in electrolyzer efficiency, and it is not clear whether the improvement is related to the project or is a consequence of non-related work by the contractor.
- In-house testing of system components has been completed as well as site preparation.
- A 12 kg/day advanced PEM electrolyzer has been built, tested, and is ready to be shipped to the demonstration site. Cold weather efficiency was evaluated. Project has achieved an increase of 8-10% cell/stack efficiency, along with 20-30% cell stack cost reduction. Project has utilized a new power supply that has decreased cost of this component from 1/3 to 1/5 of system cost. System advancements could potentially reduce hydrogen cost up to $0.50/kg. PEM system will be operational within a month.
- Electrolysis system is ready for shipment to the wind turbine site.

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

This project was rated **3.1** for technology transfer and collaboration.

- Good collaboration with local authorities, permitting bodies, etc.
- Limited collaboration with subcontractors, suppliers and permitting agencies.
- Good job of working with Northern Power, EVermont, and local authorities to obtain operating permits.
- Collaborating with a power company.

Question 5: Approach to and relevance of proposed future research

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

- Work almost complete. It is not clear how testing an modified ICE hybrid will contribute to the DOE goals.
- Future work will complete the facility this Fiscal Year.
- Project will initiate testing and analysis of PEM system at the demonstration site in near future.
- Future work is straightforward.

Strengths and weaknesses

Strengths
- Joint effort with state and local authorities to site and permit a station.
- Component testing completed and installation in progress.
- Project will provide important real-world experience on electrolyzer refueling stations in a cold weather environment. Good results in reducing electrolyzer capital cost.

Weaknesses
- Station is not publicly accessible; little contribution to DOE R&D goals shown. Little novelty in purchasing REC credits to make electrolyzer appear 'green'.
- Apparently no provision has been made for operational and maintenance costs for either the facility or the hydrogen-fueled vehicle.
- While this is billed as a renewable project, there is no renewable power generation on-site. The electrolysis system is actually powered by grid electricity which includes remotely generated wind power.
- With only one vehicle to be refueled by this facility, hydrogen demand will be minimal. Collection of operational data will be slow.
- No obvious consideration of mass manufacturing that will greatly reduce capital costs. No proposed pathway (other than information presented in project review) to share information gained from cold weather operation experience. Not much fundamental materials work (if any) involved in the project.

Specific recommendations and additions or deletions to the work scope

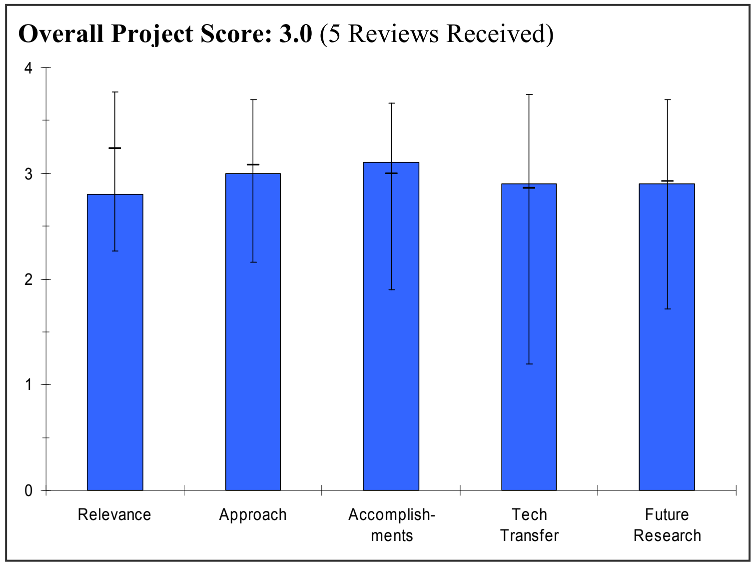
- If there is a possibility, recommend focusing on the electrolyzer improvements; particularly on enabling electrolyzer to run directly off the intermittent DC power supply (to mimic renewable power characteristics).
- Additional hydrogen utilization is desirable - additional vehicles are needed to use the facility.
- Consider how to mass manufacture electrolyzer parts. Integrate some more materials work into project, such as membrane development. Work towards delivery of hydrogen at higher pressures, perhaps eliminating one stage of compression. Share information openly on cold weather operating experience.

Project # PDP-33: A Reversible Planar Solid Oxide Fuel-Fed Electrolysis Cell and Solid Oxide Fuel Cell

Greg Tao; Materials and Systems Research

Brief Summary of Project

This industry-academic joint project is developing a planar 1 kW stack for generating clean hydrogen for onsite application and electricity for power parks, directly from either distributed natural gas or biogas fuel. It is based on the novel concept of composite/hybrid solid oxide fuel-fed electrolysis cell (SOFEC) and solid oxide fuel cell (SOFC) technologies. This project focuses on material research, stack design and fabrication, and experimental verification.



Question 1: Relevance to overall DOE objectives

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

- Project proposes to develop a planar SOFC/SOE stack to generate both electricity and hydrogen directly from natural gas or biogas. Project addresses the cost of hydrogen by improving efficiency through lowered electricity use. Project is centered on materials development and stack design; it is a long way from any consideration of commercialization. If the project wanted to make a more important contribution to the Hydrogen Initiative, it would concentrate on the development of the electrolyzer, but this operating concept has already been explored.
- Project supports objectives and is in-line with DOEs emphasis on natural gas reforming and advanced electrolysis technologies.
- It would be good transitional technology to have dual mode systems.
- Objective of this proposal is to develop a high temperature electrolysis system to produce water. Here, electrolysis and electric power production are carried out in adjacent sections. The power produced in the SOFC part will be utilized for electrolysis. This approach increases the efficiency of electrolysis process.

Question 2: Approach to performing the research and development

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

- Project is exploring new or optimized material structures to improve overall system performance. Use natural gas to depolarize the anode of the SOE (electrolysis cell). This same concept was employed earlier in a project funded by the Hydrogen Program at LLNL. Project claims to be able to achieve 80% efficiency. With regard to hydrogen production, it is not clear that this approach offers any clear advantages, perhaps just thermal management. The system basically sticks a fuel cell and electrolyzer together. It is not reversible in the sense that either the fuel cell or electrolysis cell can operate in the reverse direction.
- Project is focused on key barriers of developing electrolytes and materials for SOFC and SOFCE. Should focus on increasing materials performance before progressing to larger cells. Approach of combining SOFC and electrolyzer functions in one does not allow each to be optimized for its particular function.
- Systematic approach from materials development to cells to stacks. Is the I - RW stack self - heated?
- In this approach, pure hydrogen and electric power are generated from fuel, steam, and air. Electrolyzer part produces pure hydrogen and the fuel cell part produces electricity and increases hydrogen production rate. System efficiency is improved by thermal integration.

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

This project was rated **3.1** based on accomplishments.

- Project is 60% complete. High power densities shown for SOFC, but the power density is normalized to the smaller area electrode. New cathode materials and interlayer work useful (although considered proprietary). Also, materials performance enhanced through use of nanocrystalline materials and proprietary deposition process. Improved stack performance shown in SOFC mode. Cell testing under constant load for long term shows promising results, less 1.5% degradation per 1000 hrs after initial 2500 testing. Proof of principle offered for a 2 cell SOFC/SOE stack.
- Good progress to date.
- Considerable data shown but with very small cells (2cm2). For RW - scale stack objective, need data with larger cells. Need to shown efficiency calculations. Need more information on economic analysis - seems optimistic.
- Plenty of work done in cathode materials development. Very good (stable) cathode material is developed. Performance of 2-cell stack looks good.

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

This project was rated **2.9** for technology transfer and collaboration.

- Appropriate distribution of responsibilities between partners, Aker Industries and University of Missouri.
- Collaborations with the University of Missouri-Rolla and Arer Industries.
- University of Missouri-Rolla and Aker Industries are involved in this project. UMR is developing the cathode materials. Role of Aker Industries is not clear.

Question 5: Approach to and relevance of proposed future research

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

- Team will do short stack testing and optimization, then 1 kWe hybrid stack evaluation. Need to provide a better economic analysis. No clear timeline of when will complete this work and commercialization process.
- Good identified tasks: - short stack testing - demonstrations of H_2 productions - economic analysis.
- Future plan is to demonstrate a 1 kWe hybrid stack for hydrogen generation. They have already built the facility to test this 1 kWe stack.

Strengths and weaknesses

Strengths

- Good materials effort leading to improvements in cathodes and interlayers. Will benefit other solid oxide efforts (SECA). Good team, but not a lot of real commercialization experience or knowledge of mass manufacturing protocols.
- Good R & D team.
- Really an innovative concept.
- Strong cathode materials development team. Good team to fabricate/characterize the performance of stacks.

Weaknesses

- Unclear advantage of this approach—it is not a true reversible system. Economics not transparent. Not clear that coking of electrodes will not be a problem.
- H_2 refueling station would require MW- state equipment. The R & D team needs to address scale-up issues. Cost analysis may need input by organizations experienced in such analysis.
- Who will commercialize this technology? Lack of clear analysis on amount of hydrogen production vs. electric power generation.

Specific recommendations and additions or deletions to the work scope

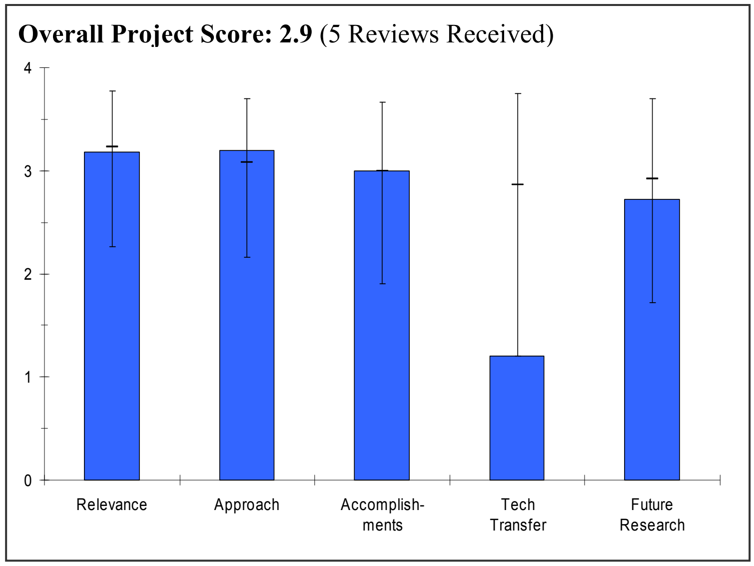
- Address sealing issues in stack development. Finish materials effort before proceeding to large stack demonstration. Enough funding should be allotted only to fund materials development and short stack (multiple cells, say three) long term demonstration. Project should be given one more year of adjusted funding (certainly not higher than FY2006) to more clearly enunciate the benefits of this approach. If they cannot, project should be terminated. Project must define when this technology could be commercialized and provide timeline for this.
- The research program appears to be well structured.
- Should stay the course and demonstrate the 1 kWe stack. Perform detailed energy and economic analysis. Since a stable cathode material has already been developed this effort may be reduced (slightly). Evaluate the mechanical properties of the cells.

Project # PDP-34: High Performance Flexible Reversible Solid Oxide Fuel Cell

Nguyen Minh; GE HPGS

Brief Summary of Project

GE Global Research is developing a single modular stack that can be operated under dual modes: fuel cell mode to generate electricity from a variety of fuels, and electrolysis mode to produce hydrogen from steam. This project will provide the materials set and desired electrode microstructure for reversible operation, and technology gap assessment for future work. Technical focuses of this project are reversible electrode modeling, electrode compositions and microstructure engineering, stack operation demonstration, and hydrogen production cost estimates.



Question 1: Relevance to overall DOE objectives

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

- Project attempts to combine in a single device the ability to produce hydrogen fuel and electrical power.
- This approach would allow electrical power production during high demand periods and then produce hydrogen during low electrical demand periods for power production at other time. The approach would maximize value of the device by allowing quick response to various demands for continuous operation.
- Improved materials are needed to increase efficiency and, hopefully, reduce cost. Dual mode capability can increase flexibility in application.
- The research is directly connected to the production of hydrogen as a fuel.
- Use of the cells in both the electrolysis and fuel cell modes would allow greater utilization.
- Fuel flexible electricity and electrolysis are both DOE goals.

Question 2: Approach to performing the research and development

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

- The approach is structured to develop a device that trades-off the best device performance for both modes of operation. This approach is different from conventional approaches that would optimize performance in either the fuel cell mode or the electrolysis mode. A compromise is required in this instance.
- Good mix of modeling and experimental work. Systematic progression from materials to cells to stacks.
- Experimenters have developed techniques for the production of larger area cells.
- Researcher has addressed critical issues including degradation of cell performance.
- The project is narrowly focused on developing designs and materials for a reversible fuel cell/electrolyzer system.

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

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

- Significant progress has been made in optimizing dual-mode operation.
- Identifications of LSCF as the best performers consistent with work elsewhere. Showed good match between model results and experimental data.

- Researchers have succeeded in fabricating and operating cells in both fuel cell and electrolysis modes and in cycling between modes.
- Project has demonstrated stable and predictable long-term operation.
- Accomplishments are good but the implications are not clear. How much more technical advancement is required?
- GE has demonstrated solid accomplishments in solid state electrolyzer development.

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

This project was rated **1.2** for technology transfer and collaboration.

- There are no apparent collaborations, outside of GE, for this project.
- No evidence of technology transfer or collaboration was presented.
- There is little indication of collaboration or technology transfer.
- The poster had sufficient technical information for an evaluation of project progress.
- No apparent technical collaborations.
- No partners or collaborators.

Question 5: Approach to and relevance of proposed future research

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

- Future plans appear to be reasonable and appropriate.
- Productions cost estimations would be good. No investigation of failure mechanisms (e.g., delamination) was proposed in future work.
- Plans for future stack experiments are sketchy but reasonable.
- Straightforward completion of experimental phase by demonstration of system. An estimate of hydrogen cost will be useful.
- Future work is not well planned.

Strengths and weaknesses

Strengths
- Project builds on extensive experience and expertise at GE.
- Well defined performance target (e.g., ASR < 0.5 ohm cm2). Includes model development to guide further research.
- Builds on established technologies.
- Demonstrated progressed in larger area cells.
- GE internal team is solid with a long history of making significant contributions to solid state fuel cells and electrolyzers.

Weaknesses
- It would have been useful to have a collaboration with a potential user for validation the merit of this sort of device.
- No clear application in mind. - without a target application, difficult to know if you have achieved something meaningful.
- Need to find causes for performance degradation in long-term tests.
- The future work should have increased emphasis on durability testing, cycling and degradation.

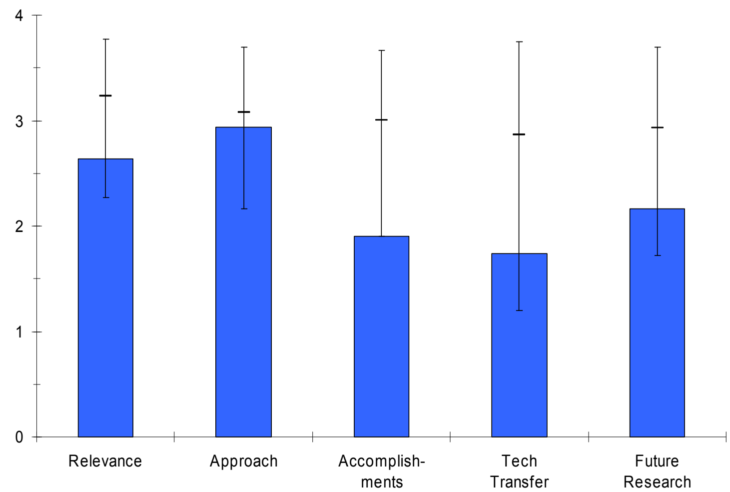
Specific recommendations and additions or deletions to the work scope

- Complete the project in a timely fashion and assess commercialization potential. Determining the cost of produced hydrogen will be an important result of this work.
- In the reversible fuel cell / electrolyzer being developed, it would be helpful to define how systems cost should be apportioned between power production (c/ KWH) and H2 production ($ / Kg).
- There should be more communication between this project and other similar electrolysis work within DOE.

Project # PDP-36: Solid Oxide Fuel Cell Carbon Sequestration

Daniel Graves; Nisource Energy Tech.

Overall Project Score: 2.3 (5 Reviews Received)



<u>**Brief Summary of Project**</u>

NiSource Energy Technologies, Inc., a subsidiary of NiSource Inc., is engaged in research and development of a solid oxide fuel cell (SOFC)/carbon sequestration technology whereby separate exhaust pathways are provided for spent fuel and air. This allows the SOFC the flexibility to either direct the exhaust back to the fuel input to provide steam for reforming hydrocarbon fuels or further oxidize the exhaust gas stream into carbon dioxide and water vapor. The remaining water vapor can be condensed, leaving a pure carbon dioxide stream, which could be used for chemical processing, oil/gas recovery enhancement, or simply re-injection into the ground to avoid carbon emissions. If successful, this development would support the ability of SOFCs to run virtually free of such emissions.

<u>**Question 1: Relevance to overall DOE objectives**</u>

This project earned a score of **2.6** for its relevance to DOE objectives.

- An approach far better aligned with the DOE objectives would convert the waste CO to useful heat or power.
- This alternative approach simply oxidizes the CO to CO_2 without trying to capture the valuable heat of combustion.
- Extremely relevant topic – although not clear how distributed SOFCs can help with sequestration considering the small quantities of CO_2 produced that have to be gathered and transported.
- The project objective is to capture all of carbon monoxide and carbon dioxides thereby enabling a natural gas driven SOFC without carbon emissions.
- Not directly a hydrogen production, storage and delivery objective.
- However, a purpose of hydrogen energy is carbon management and the development of fuel cell technologies, and thus the project supports the President's Hydrogen Fuel Initiative in this respect.
- Solid oxide fuel cell (SOFC) carbon sequestration is a unique idea which can advance the use of SOFC technology which is an important part of the H_2 fuel initiative.

<u>**Question 2: Approach to performing the research and development**</u>

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

- Given the poor progress to date, alternatives to the project approach should be considered and judged relative to that being explored.
- Clever engineering approach with the fuel recycle scheme.
- The approach involves the use of oxygen conductors to burn spent fuel so the exhaust is composed of only CO_2 and H_2O.
- CO_2 is separated from H_2O by condensation.
- The approach is not new or novel. Use of oxygen transport membranes to burn residual fuel has been reported in literature.
- The project reports advanced manifold design. Progress to higher Wattage tubes in stackable design in one year.

- Various approaches to achieve the oxygen transport membrane (OTM) goal have been proposed. Careful considerations were given on these approaches.
- The interface integration between OTM and the rest of SOFC was not adequately discussed, e.g., temperature compatibility, and the rates of oxygen ion transport vs. the combustion.
- Some considerations should be given to the handling of pure CO2 stream from the SOFC. This issue could be site specific.

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

This project was rated **1.9** based on accomplishments.

- This project made very little progress towards achieving its technical objectives in the last year. Most of the results I saw on the poster were also shown on last year's poster. Given the amount of money spent, this is a VERY disappointing project. Roughly 75% into the project schedule, only 20% of the project objectives have been completed according to the Poster page 2 (overview chart).
- Technical accomplishments are extremely limited considering that the project should be almost complete. No actual data has been demonstrated.
- Progress made consistent with intended technology R&D objectives.
- Mechanical design and fabrication improvements/advancements for collecting spent fuel and injecting via orifice to the fuel cell are accomplishments towards a successful project outcome.
- Experiments on oxygen conducting formulations and coating of the dense layers resulted in Oxygen Transport Membrane (OTM) II with high oxygen flux.
- No specific accomplishments reported on stack design, material composition, etc., that will be utilized for building the test rig and endurance tests.
- The project aims to achieve results in two significant technology areas; oxygen conductor materials R&D and SOFC design improvement for spent fuel capture and combustion.
- The expertise for OTM materials R&D and improvement of stack designs are not necessarily complimentary.
- It is not clear if the project PI is adequately resourced to execute the scope of work on the oxygen conductor materials R&D and fabrication in desired geometries.
- The project is in early stage of development. Progress was made in the material development area.

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

This project was rated **1.7** for technology transfer and collaboration.

- None evident other than Accumentrics and NiSource.
- No partners or collaborators identified in the presentation.
- Most of the work done at the PI lab.
- No list of industrial participants was presented.

Question 5: Approach to and relevance of proposed future research

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

- Not clear that any of the proposed work will overcome the barriers identified in prior work. Only five months remain in the project schedule, and I have little confidence that they will be able to select and validate an electron ion conductor material for the CO oxidation step.
- Proposed work sounds appropriate and well thought out, however, lack of progress to date makes one wonder if the plan is realistic.
- Legit future work is consistent with the proposed objectives.
- The PI has not discussed any failure modes and back up plans; one scenario is what if the oxygen transport membrane cannot deliver stoichiometric quantities of oxidant for complete combustion of the residual fuel.
- If the OTM fails, the project may fail because oxygen conductors are not commercially available.
- No project milestones were presented. Will a contract extension be needed?
- A future work plan was presented including details on advancing the proposed technology.

Strengths and weaknesses

Strengths

- Good overall approach.
- Improvements in fuel cell stack design for capturing the spent fuel and fuel delivery to the oxidation chamber.
- Reaction of oxygen anions with spent fuel for complete combustion.
- The PI has developed expertise in OTM development tailored for SOFC applications.

Weaknesses

- Not clear why consider CO_2 capture on small scale. Progress to date seems to be disappointing. Unclear how the work can be completed on-time.
- Substantial work remains during the next four months to achieve project objectives.
- Expertise in OTM materials R&D and fabrication in desired configuration is unclear.
- Since OTM is not available commercially, lack of availability of appropriate material with expected operating performance and life may pose a road block to project success.
- A cost-benefit analysis would be helpful for the future pursuit of this project.

Specific recommendations and additions or deletions to the work scope

- At this point would sharply limit focus on getting performance data with the system and components available.

Hydrogen Storage

Summary of Annual Merit Review Hydrogen Storage Subprogram

Summary of Reviewer Comments on the Hydrogen Storage Subprogram:

Reviewers consider hydrogen storage to be a critical enabling technology to the hydrogen economy and it remains a significant technical challenge. Overall, the R&D portfolio was judged to be robust, emphasizing key areas and well focused on the DOE targets. According to reviewers, the subprogram "is getting more refined and is constantly evolving in the right direction." Reviewers commented that DOE's "strategy is appropriate, to continue emphasizing a wide net of higher risk-high reward research." Some reviewers commented that the projects have shifted closer towards more basic science and need to keep sight of system and engineering issues. It was also stated that the use of the Centers of Excellence (CoE) "is an appropriate method to manage this diverse portfolio and promote synergy and enhance innovation." However, it was also clear that the flexibility provided by independent projects outside the CoEs is critical as well and that "maintaining independent projects ensures agility." Reviewers emphasized that "storage requires breakthrough discoveries" and that DOE has done an effective job in "developing the proper background and foundation for achieving these discoveries."

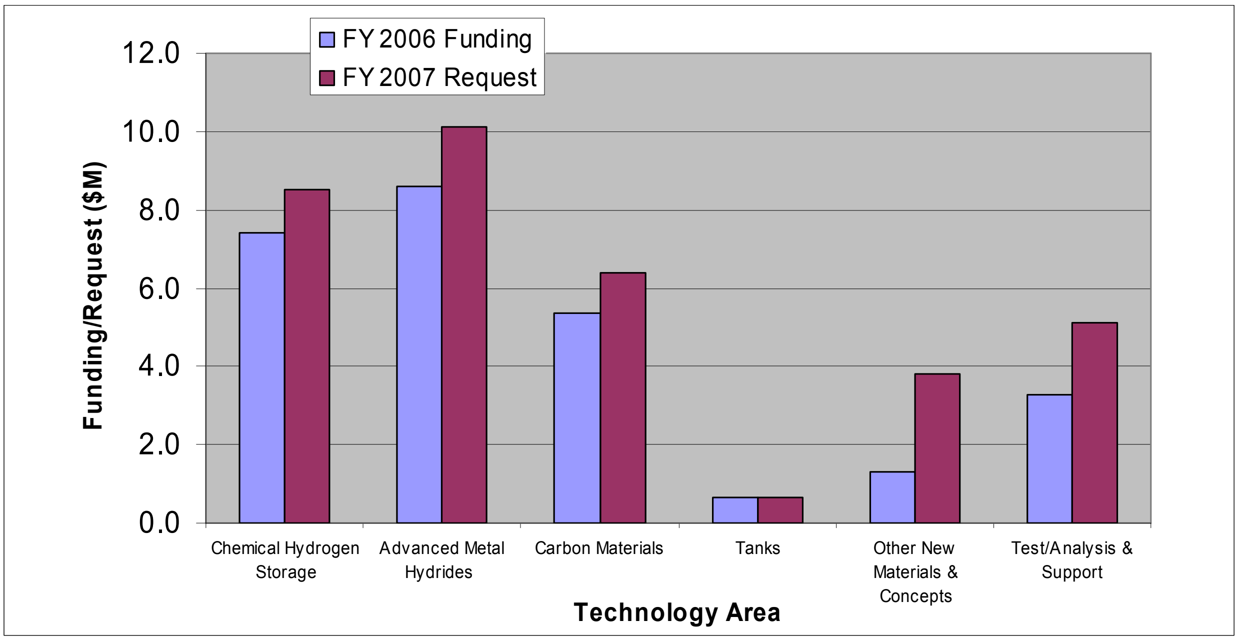
Note that although the basic science hydrogen storage projects (funded by the DOE Office of Science, Basic Energy Sciences) were not formally reviewed, FY2006 was the first time both basic science and applied research projects were presented together. This facilitated more interaction between both research communities and increased coordination since the inception of the efforts in FY2005. A "Theory Focus Session" on hydrogen storage materials was also conducted, co-organized by the Office of Science (Basic Energy Sciences) and Office of Energy Efficiency and Renewable Energy (Hydrogen, Fuel cells and Infrastructure Technologies), which included experts from around the world to further help define theoretical research needs in hydrogen storage
(see: http://www1.eere.energy.gov/hydrogenandfuelcells/wkshp_theory_focus.html).

Suggestions and general comments for DOE to address include:

- Provide more information on how the CoEs operate (meetings, openness, duplication of efforts, etc.)
- Ensure that lessons learned are shared among the CoEs. Interaction mechanisms among the CoEs may be valuable (e.g., issues with metal hydrides that are not reversible on-board as part of the Metal Hydride CoE may benefit from progress in the Chemical Hydrogen Storage CoE).
- Continue to remind researchers that targets are system, not material-based targets.
- Expand the discussion of engineering issues including thermal management, system integration, reactor design, and safety/hazard aspects of the materials investigated.
- One area that needs more emphasis is tank systems, which will be used for most solid state/material approaches as well. Research is still needed for cost reduction of high pressure tanks, which will most likely be used in early mobile and stationary applications.
- A recurring concern was the redirection of funds to congressionally-directed projects and the reduction of competitively selected, merit-based projects within the portfolio.

Hydrogen Storage Funding by Technology:

The funding portfolio for hydrogen storage addresses primarily long-term materials R&D to meet 2010 and 2015 targets for on-board applications. The requested EERE FY2007 funding profile (subject to congressional appropriations), which includes the CoEs and independent projects, continues to address the National Academies' report recommendations to "shift…away from some development areas towards more exploratory work" and that "the probability of success is greatly increased by partnering with a broader range of academic and industrial organizations…" Continued support, at a low level, for compressed hydrogen/cryogenic tanks emphasizes cost reduction and novel conformable designs. In addition, it is recognized that materials-based solutions will require low-cost, conformable tanks and would benefit from current R&D in this area. Key milestones for FY2007 will be (1) a system that meets the targets of 1.5 kWh/kg and 1.2 kWh/L and (2) a go/no-go decision on sodium borohydride-based systems ($NaBH_4$). Subject to congressional appropriations, the storage program will also have approximately 3 to 6 new awards from the FY2006 solicitation that will start in FY2007. The chart below illustrates the funding in FY2006 for each major activity along with planned funding in FY2007 based on the Program's budget request.



Majority of Reviewer Comments and Recommendations:

Chemical Hydrogen Storage: The Chemical Hydrogen Storage CoE was commended as well integrated and with excellent collaborations among multi-disciplinary partners. It was felt that the CoE has made good technical progress and produced promising results and that the synergy of the center has enhanced its accomplishments as a whole. However, several critical issues remain, including viability of off-board regeneration, slow discharge kinetics and limitations of systems that require water for on-board hydrolysis. Chemical hydrogen storage researchers were encouraged to focus on regeneration of the spent fuel and continue to pursue high capacity storage materials to enable a viable storage system. Additional recommendations are to start developing down-select criteria to narrow down options and to include cost as a factor in the search for effective catalysts.

Advanced Metal Hydrides: In general, the reviewers agreed that the full set of capabilities established in the Metal Hydride CoE represents an impressive array of tools for the study of hydrogen in metals. It was acknowledged that PIs are demonstrating flexibility and adjusting their materials research, based on early results and viability to meet DOE targets. Both in the Metal Hydride CoE and in independent projects, the utility of theoretical modeling in guiding materials discovery has been demonstrated and is in the process of being expanded. The use of experimental high-throughput synthesis and testing capabilities is accelerating materials discovery and is encouraged by the reviewers. As materials with suitable thermodynamics are identified, the hurdle of improving hydrogen discharge/up-take kinetics is being addressed by utilizing catalysis and reaction engineering. The reviewers acknowledged that the metal hydride work is beginning to address issues common to the Chemical Hydrogen and Carbon CoEs.

The issues of thermal integration for hydrogen re-fill/discharge and material regeneration will benefit from collaborations with the Chemical Hydrogen CoE. Similarly, the work to address kinetics by both catalysis and studying size effects will contribute to the Carbon and Chemical CoEs. The reviewers agreed that going forward with material down-selections and go/no-go decision points will be critical and that these processes should be quantitative and transparent.

Carbon Materials: The reviewers credited the Carbon CoE for expanding its focus beyond single walled nanotubes (SWNTs) to a diverse set of high surface area adsorbent materials. It was also stressed that adsorbent materials have the potential to offer alternatives to metal hydride approaches with reduced re-fill heat rejection requirements and improved hydrogen kinetics. The approach taken by the CoE was found largely sound: theoretical modeling to material synthesis to development. The reviewers were concerned that the CoE was stressing modeling at the expense of "proving" these experiments through synthesis and testing and recommended increased investment in the latter activities. For all adsorbent work in the program, most reviewers stressed that for the vehicular application, adsorption must be addressed to occur at close to ambient temperature rather than solely at cryogenic temperatures (e.g. 77K). As hydrogen binding energies are increased, the reviewers also noted that the issues of hydrogen uptake/discharge kinetics as well as thermal management will need to be addressed. This work will benefit from increased collaborations among the three centers, as noted above. Finally, volumetric capacity is a greater hurdle with these materials compared with gravimetric capacity and should receive priority.

Tanks: Tank projects were not reviewed in FY2006 due to the limited number of projects and the reduced effort on tanks. In lieu, DOE is conducting an independent assessment of cryocompressed tank technologies, with results to be made available at www.hydrogen.energy.gov by early FY2007.

Testing and Analysis: The analysis projects (Argonne National Laboratory (ANL) and TIAX LLC) were considered critical to the program in providing independent assessments of all storage options. Further refinement of assumptions and validation of models are essential. Strong coordination between TIAX and ANL, as well as with relevant storage system developers, was recommended. The independent testing project (SwRI) was not formally reviewed in FY2006; the materials test facility was completed and is available for testing external samples.

Note on Storage Report Structure:

Chemical Hydrogen Storage.
ST-01 to ST-08 and STPs 25, 26 and 27 are partners of the Chemical Hydrogen CoE.
ST-09 is an independent project.

Metal Hydrides.
ST-13 to ST-18 and STPs 03, 04, 05, 6, 7, 8, 9, 10, 12 are partners of the Metal Hydride CoE.
ST-10, ST-11 and STP-02 are independent projects

Carbon.
ST-23 to 28 and STPs 15, 16, 17, 18, 19, and 21 are partners of the Carbon CoE.
ST-21 and ST-22 are independent projects

Analysis.
ST-19 and ST-20 are analysis projects

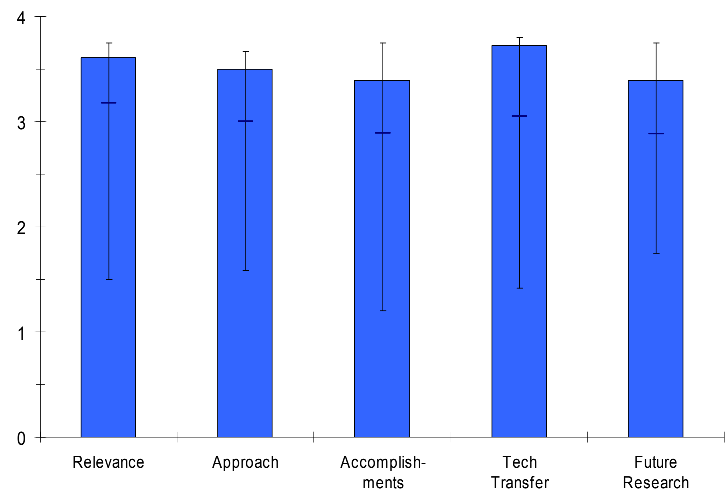
Cross-Cutting,
ST-12, STP-37, and STP-43 are cross cutting projects covering hydrogen storage and fuel cell technologies.

Project # ST-01: DOE Chemical Hydrogen Storage Center of Excellence: Center Overview & Los Alamos National Laboratory Contributions

Bill Tumas; Los Alamos National Laboratory

Brief Summary of Project

Overall Project Score: 3.5 (9 Reviews Received)



[NOTE: This review is on LANL's contributions, not on the entire DOE Chemical Hydrogen Center of Excellence. Each of the partners is evaluated separately.]

The chemical hydrogen storage CoE involves two national laboratories, seven universities, and four industrial companies. The objectives of the CoE are to identify, research, develop and validate advanced on-board chemical hydrogen storage systems to overcome technical barriers and meet 2010 DOE system goals with the potential to meet 2015 goals:

1. Develop materials, catalysts and new concepts to control thermochemistry and reaction pathways
2. Assess concepts and systems using engineering analysis and studies
3. Select most promising chemical systems for engineering development
4. Develop life cycle inventory and demonstrate a 1 kg storage system
5. More efficient borate-to-borohydride (B-OH to B-H) regeneration
6. Alternative boron chemistry to avoid thermodynamic sinks using polyhedral boranes (B_xH_y) or amine-boranes
7. Concepts using coupled endo/exothermic reactions, nanomaterials, heteroatom substitution for thermodynamic control

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Relevance has increased with the diversification into more chemistries. Plan to trim non-productive research – if implemented with vigor – will further improve this criterion.
- Well aligned.
- Need to identify materials with potential to meet DOE targets.
- The CoE is clearly relevant to the President's Hydrogen Fuel initiative; hydrogen storage is identified as the critical need for transportation. Off-board regeneration is the critical step to determine if the chemical hydrides are viable on a round trip basis. Off-board regeneration should have significant on-board advantages (weight or volume efficiency) to make up for the round trip efficiency losses.
- Chemical hydrides are highly relevant towards achieving the DOE goals. This CoE seems to have the best overall appreciation of these goals, and how they guide the research direction.
- Good! Quoted volumetric densities all the time!
- This program strongly supports the Hydrogen initiative and objectives.
- Hydrogen storage is the key enabling technology critical for the success of hydrogen as a transportation fuel. Any work which can further understanding of the basic science of hydrogen storage is vitally relevant to DOE objectives, the President's Hydrogen Initiative and the objectives of the Multi Year R&D Plan. The chemical hydrogen storage CoE is a key research activity which supports all these objectives.
- This research shows very high potential to meet the DOE system goals.
- Work in the CoE is central to hydrogen program storage development. Must have vigorous effort in all approaches to storage, including chemical hydrides.

- LANL work essentially covers full spectrum of work included in the CoE.
- Ammonia-borane holds a clear promise for meeting the DOE targets for hydrogen storage materials. However, it remains questionable whether the serious regeneration and cost issues about this compound and related compounds can be overcome that will allow its practical application to onboard hydrogen storage.

Question 2: Approach to performing the research and development

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

- Good efforts on CoE management.
- Program is well integrated internally. In general the plans are feasible at least in theory. Some questions on the case of electrochemical reversal of BO_2 to BH_4, but new programs are well poised.
- Good understanding of DOE targets; focused on correct technical barriers.
- Good mix of experiment/theory; Good use of computational chemistry.
- The technical barriers are correctly understood. The overall project is designed well. Need further diversification to consider candidates beyond boron-chemistry.
- The CoE is taking a logical, methodical, and thorough results-focused approach to addressing the barriers to a viable hydrogen storage system.
- Good CoE structure; good approach for downselect; CoE has defined down-select procedures and some criteria; flexibility in shifting work
- Good approach.
- CoE appropriately splits efforts in parallel regarding, regeneration, kinetics, etc.
- Regeneration is the key issue with all chemical hydrides. Efforts are being made toward identifying new methods for the electrochemical reduction of $[B(OH)_4]^-$ and regeneration of dehydrogenated ammonia borane. However, the project is too weighted towards improving dehydrogenation kinetics rather than solving the key issue of developing a practical regeneration processes. The organic based systems do not seem to be a very promising approach.
- Would like to see more carbon-based hydrides (not just N & B--assuming no conflict with Carbon CoE).
- Hydride regeneration needs to be considered from day-one on new materials.
- Put "experimental demo of regeneration" above "engineering assessment of H_2 release" in priorities on "funnel" diagram.
- CoE goal of 50% efficiency of recycling seems to be set low; encourage to aim higher.
- Good to expand CoE interest to non-boron chemistries. Need some clearer definition of where to look and what systems seem most promising. Need to articulate some of the downselection criteria reasonably soon as 2007 is drawing close.
- It's good to keep numerous parallel approaches in the first two years of CoE operation but some thought needs to be given for down selection criteria to narrow down the options from among many choices.
- Concentration is appropriately shifting from $NaBH_4/H_20$ systems to amino borane and other liquid carrier systems. Work on B-O regeneration from R&H should be final point for $NaBH_4$ work.
- CoE is demonstrating appropriate balance between modeling and experiment to down select possible materials
- LANL work well integrated with CoE partners

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

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

- The CoE as a whole has made good progress; possibly the best progress of any of the CoEs. Several of the partners have produced results that hold real promise of being able to meet the long term storage targets. LANL's own projects are also promising with positive results from the ammonia borane work. Although lab results have been promising, a more practical solution is needed if ammonia borane is ever to be used in a production vehicular storage system.
- This CoE is providing the most novel ideas and approaches of all CoEs and is actively trying out new materials with some success

- Evidence of good progress from inception of the CoE. It is still a long time to go until the go/no-go decision on sodium borohydride. Last year the CoE was just forming, and this year a number of chemistries for the rehydrogenation step have been identified. It is not clear which one(s) have the most promise, but at least they have been identified.
- I expect this to improve in future, as there is good work underway, but the actual demonstrated progress in the last year is probably best termed between fair and good.
- Many high density materials have been demonstrated although no one material has demonstrated good kinetics, regeneration etc all at once.
- Kinetics still an issue and economical regeneration is unclear at this point.
- Program shows good progress for 1st year.
- LANL made important progress in all areas
- Some promising areas identified for future work
- Need to focus on identifying most promising systems and moving forward.
- The development of a method for the "B-O to B-H" conversion that involves a readily re-generable alkaline hydride rather than the irreversible oxidation of a metal hydride is a truly significant achievement. The development of catalysts for hydrogen release from ammonia borane is nice technical achievement but represents progress on a very secondary front.
- Avoidance of BO bond formation is a good idea.
- Very good that there is significant work on ammonia borane regeneration (w/Penn). This is really the key obstacle for this (and generally for chemical hydride) systems. Have developed a process, albeit a complex one.
- B-OH to B-H work doesn't look terribly inspiring. We will have to wait until next year to see whether any of these pathways actually prove promising. Have a good exit strategy to get out of this game, if things don't prove promising soon.
- Encouraging results on H_2 release.
- Reformation of hydride using formic acid is very significant development.
- Some interesting work on organic hydrides - how does this couple/differ from Air Products work? Why are there so much lower densities than the similar approach from Air Products? It seems like as a base-line, you should be able to build on their ideas? There is a projected theoretical number of >8wt.%, but this is greater than 1 H/C; what is the idea here?
- 5 patents filed; very prolific generation of ideas.

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

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

- LANL is doing an excellent job of CoE coordination. The projects at LANL are well interfaced with multiple collaborations of CoE partners.
- Reasonable mix of partners.
- May need to work to promote interaction between scientists and engineers (chemistry and process design).
- Lots of collaboration and excellent IP arrangement to enable more. Tech transfer is built in with appropriate and strong industry involvement.
- Collaboration between the CoE members appears to be excellent. The university contributions seem to be well integrated into the CoE's portfolio. It appears that the universities can provide some of the basic information on reaction chemistries of interest. It appears that the accomplishments of the CoE as a whole are greater than the accomplishments of the individual members would have been without the benefits of the synergy between the members.
- General comment about dehydriding of AB: there are many separate strategies about this, and new catalysts proposed: what is the coordination between these efforts?
- The CoE has assembled a world class group of partners to work toward the goals of hydrogen storage.
- CoE is collaborating with all the appropriate members and institutions. CoE must be careful about relying too heavily on systems design and assumptions from one partner, other partners in system design should also be pursued.
- Interactions within CoE clear, but not clear with respect to outside of the CoE.

Question 5: Approach to and relevance of proposed future research

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

- Future research plans for both the CoE and LANL appear to be well conceived, thorough and are working toward the go/no go decision points as well as the goals of the program.
- The plans are interesting, innovative and on target. Costs are probably not very well considered, but that is not inappropriate when other goals are far from being met.
- Reasonable goals for next year.
- Go/no-go needs to go forward--regeneration should be prime consideration.
- The CoE has laid out a plan to acquire the knowledge necessary to define the criteria by which to make a go/no-go decision on the borohydride system. However, this decision is not very far away and it is not too early to start laying out the criteria for evaluating the process that will be used to arrive at the decision.
- Will $NaBH_4$ decision rule out other boron routes that go to borate? Since regeneration is critical, that should be part of go/no-go criteria.
- Studies focused on the re-generation of ammonia borane are of key importance and well designed. Work on "non-boron-based 6-7 material wt.%" should be de-emphasized as it seems very unlikely that system based on these materials will meet the DOE goals.
- Need to consider further contingencies in case ammonia-borane does not work out. Further consideration of other regenerative carbon-based hydrides.
- Direct more effort toward regeneration processes on promising candidates since this is a critical aspect of any chemical hydride approach
- Some of the catalyst research for improving aminoborane kinetics seems uncoordinated and/or duplicated. Some more coordination from the CoE is required to fully maximize resources on this issue
- Lean away more from borohydride, concentrate more on the coupled reactions

Strengths and weaknesses

Strengths
- Well organized CoE with strong partners and focused research ideas.
- Strongest CoE.
- Probably have the clearest view of a total system, not just material.
- Good understanding of the fundamental chemistries involved in the boron-nitrogen system. The CoE appears to be up and functioning with excellent collaboration between the partners in evidence. The CoE rightly has cast a wide net in examining various B-N systems because it is too early to downselect to specific paths to regeneration.
- Positive progress with storage technologies in both CoE partners and at LANL.
- Quality of team in this area.
- Technical Expertise.
- Strong leadership from PI. Of the three CoEs, the Chemical CoE has the best grasp on "SYSTEM" and regeneration issues and are either addressing or keeping in consideration all aspects of the system design in their materials discovery path. Amineboranes are demonstrating very high storage densities in terms of volume and gravimetric.
- Strong program management. Excellent collaboration among the CoE members. High degree of technical competence across the entire program. Innovative approach. Using an effective approach on go/no-go decisions. The PIs and management understand the barriers well. Significant accomplishments in the short period.
- Leadership by LANL has been very good. The science has been good too.
- Management Program.
- Progress toward the re-generation of ammonia borane.
- Number of options theoretically meeting 2015 goals.
- Number of new programs.

Weaknesses

- The electrochemical "B-O to B-H" project seems to be approaching a dead end.
- Not sure of capabilities of team for process design/simulation and economic assessments. These efforts need to be started early in concert with experimental work.
- Recycle efficiency goal seems low.
- The overall CoE presentation was a little hard to follow. This probably was a function of the amount of material and information that needed to be presented. That said, however, some fundamental background information to provide a better perspective on how chemical hydrides fit in the overall picture on hydrogen storage would have been helpful. The following talks actually clarified some points from the first presentation.
- It is hard to assess the likelihood of success for $NaBH_4$ regeneration. Data mining did not reveal any favorable pathways, and electrolytic regeneration attempts have not met with success. The only hope appears to be some pathway based on proprietary Rohm and Haas data that was alluded to in the presentation.
- Would like to see more information about the B-O to B-H work at LANL; PI should try to find a way to share more details without compromising IP.
- Recommend that summaries of status be shown in terms of system targets to make it easier to compare progress to the system goals and not to material capabilities.
- One of the initial and key strengths of the CoE is that liquid carriers would allow for cheap and conformable tank systems. The recent shift in material research to solid amino boranes has potentially jeopardizing this strength. Strong efforts should be made to solubilize these materials into free flowing liquids in order to maintain system simplicity. If a compromise in storage density is required to liquefy the materials, It is better to cut the storage density of the fluid in half rather than double the amount of ancillary equipment required to handle the solids. From an OEM perspective, material handling of solid fuels is not preferred.
- The CoE is overly dependent on one partner for system design of liquid based carrier fuels. The CoE should seek additional partners to balance and provide alternative approaches and provide checks and measures to current partner's assumptions and claims.

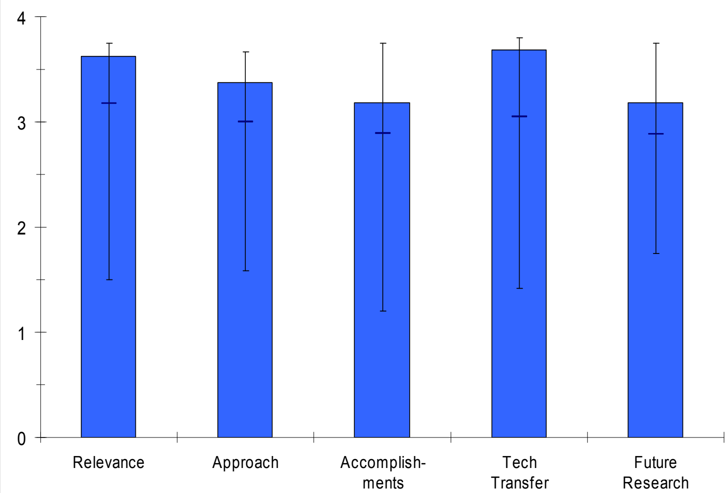
Specific recommendations and additions or deletions to the work scope

- Stay on all participants to generate data. The main lack is hard data to pour into the evaluation "funnel".
- The CoE should elaborate on their plans for assessing progress on individual projects and the means by which new ideas will be incorporated into the CoE.
- All members of the CoE need to keep the basic requirements for a viable on-board system in mind at all stages of the research. Any concept involving solids handling on vehicles is not desirable.
- The go/no-go decisions and criteria need to be developed.
- The down selection process mechanics need to be articulated.
- Conceptual process design for on-board system using solids.
- As soon as possible, the CoE needs to determine if there is any real value in $NaBH_4$ as a vehicular storage media, and if not, discontinue work on it to focus on more promising developments. There are plenty of chemical alternatives that bear further study to continue work on $NaBH_4$ if it is proven to be of minimal feasibility.
- The potential of the "organic hydrides" is rather marginal and work on these compounds should be deleted.
- Probably should use the lower heating value of hydrogen in burn ratio, HHV [higher heating value] is unlikely to be achieved.
- Go no-go for borate regeneration should be heavily based on outcome of the Rohm and Haas project on identifying economical regeneration routes.

Project # ST-02: DOE Center of Excellence for Chemical Hydrogen Storage: PNNL Progress
Chris Aardahl; Pacific Northwest National Laboratory
[Partner of the DOE Chemical Hydrogen Center of Excellence]

Brief Summary of Project

Overall Project Score: 3.4 (8 Reviews Received)



[NOTE: This project covers the R&D activities being conducted by PNNL as part of the DOE Center of Excellence for Chemical Hydrogen Storage.]

The objectives for PNNL include identification and investigation of chemical compounds that promise to meet DOE goals for storage density (gravimetric and volumetric), hydrogen release rate, and fuel cost. The approach includes assisting in evaluation of improved regeneration strategies for sodium borohydride (SBH), examination of other boron systems such as the ammonia boranes, and discovery and development of new chemical systems beyond boron. Viable bench-scale chemistry from the CoE will be developed into engineered approaches and demonstrated as a viable storage system.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Hydrogen storage is the key enabling technology critical for the success of hydrogen as a transportation fuel. Any work which can further understanding of hydrogen storage is vitally relevant to DOE objectives, the President's Hydrogen Initiative and the objectives of the Multi Year R&D Plan. PNNL in this project supports the chemical hydrogen storage CoE, which is a key research activity supporting all these objectives.
- PNNL's contribution to the chemical hydride CoE is important in two areas related to sodium borohydride: developing models to investigate processing options for higher concentrations of NaBH and extensive literature review of NaBH regeneration schemes. In addition the work on ammonia borane scaffold material is very important to the study of ammonia boron systems.
- Certainly relevant programs.
- Work is completely in line with hydrogen program storage development
- Amino borane materials are showing good promise towards achieving some DOE targets.
- Ammonia-borane holds a clear promise for meeting the DOE targets for hydrogen storage materials. However, it remains questionable whether the serious regeneration and cost issues about this compound and related compounds can be overcome that will allow its practical application to onboard hydrogen storage

Question 2: Approach to performing the research and development

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

- PNNL is taking a logical, methodical, and thorough approach to addressing the barriers to a viable hydrogen storage system.
- Similar to LANL/CoE, the approach here is very good. A nice mix of experiment/theory.
- Combined theory and experiments a good approach.
- PNNL efforts complement the other CoE partner's work, use of engineering tools to direct research activities is particularly noteworthy

- Good understanding of targets, but didn't mention volumetric density (and regeneration) too often; these are some of the most critical issues.
- Accurate determination of the thermochemical parameters of the ammonia borane system is the correct first step in this effort. However, regeneration is the key issue with all chemical hydrides. The project is far too weighted towards improving dehydrogenation kinetics rather than solving the key issue of developing a practical regeneration processes.
- PNNL intends to measure reaction rates for the seeded and unseeded material to determine the status of this technology versus the DOE targets.
- Interesting and simple approach to improve kinetics with seeds but if seeds are simply an intermediate component of the reaction or partially spent fuel-- how stable will they be through several cycles. Will that intermediate sustain an equilibrium concentration?
- Not quite sure how would the system be implemented, seeds are simply an intermediate, scaffolds may be expensive and difficult to regenerate but should be investigated anyway.
- PNNL recognizes the need to minimize the weight and volume of the scaffold material if the concept is to have a chance of success. Unsure if handling solid ammonia borane on board a vehicle is feasible. Probably better to look at solubilizing the AB.
- Active cooling of system (even minimal) may be a concern for system design in order to avoid the thermal runaway situation.
- Don't see value in developing the SBH reactor modeling- avoids the main issues of SBH.

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

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

- Scaffolding approach first reported by PNNL was a significant accomplishment and is being looked at for potential application in the other CoEs
- Seeding with spent ammonia borane to eliminate the delay for the onset of hydrogen release and increasing the dehydrogenation reaction rate appears to be a significant accomplishment.
- Significant progress on ammonia borane programs, however they need to be cautious on setting the induction time such that the material in the tank reacts rapidly after a 600 hr induction time.
- Interesting work on DFT of solid vs. gas-phase ammonia borane dehydrogenation (showed example for cyclic products). How well do these energetics compare with experiment?
- Heat management slide - showed near thermal runaway of pressure at very modest pressure (~50°C)? Isn't this a big concern? This could essentially become a dormancy issue similar to the concern over liquid H_2 - exothermic reaction pressure buildup could cause significant amount of fuel to vent during dormancy? To some extent, this is dependent on the design of the catalyst (i.e., seeding the material with ammonia borane as a catalyst vs. Using a separate heterogeneous catalyst), so this design needs to be more clearly specified.
- Technical accomplishments seem reasonable commensurate with the amount of time and money spent to date. Most of the accomplishments are valuable; however, there is some concern that the accomplishments in heat management (slide 13) indicates there is an exothermic reaction which would require external cooling to avoid run away and the loss of hydrogen after prolonged storage.
- Good progress on seeding- this is a simple and novel approach. More work needs to be done on seeding to determine the cyclic stability of the seeds and the correlation of it's relation to the thermal runaway issues observed.
- Key thermochemical parameters have been established. Very little progress towards development of a method for regeneration.
- Made excellent progress in the areas considered, identified some mechanistic issues, such as induction period in H_2 release from AB, and completed multi-scale model

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

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

- Multiple collaborations with CoE partners and others.
- Well integrated, appropriate transfer to relevant business.

- Development and transfer of engineering reactor model to Millennium Cell is a good sign that collaboration among the members of the CoE is fairly strong. This was evident in some of the following talks as well.
- PNNL is working with the chemical hydrogen storage CoE and with world class partners in the CoE.
- Partnered with CoE.
- good example of collaboration outside of CoE.
- good interactions within CoE.

Question 5: Approach to and relevance of proposed future research

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

- The plan forward is logical and will help resolve the issues in controlling the dehydrogenation process as well as identifying viable pathways to regeneration of the spent AB.
- Future plans are well thought out and logical developments of work conducted so far.
- Builds on this years accomplishments
- Excellent IPHE project proposal on $AB/LiNH_2/MgH_2$ - this seems like a very good idea, and novel results are likely. This seems like an excellent candidate for combinatorial/high-throughput work? Can theory help out here?
- The international collaboration with IPHE could prove to be very promising considering the caliber of partners.
- Future studies should have greater focus on the re-generation of ammonia borane.
- Engineering and regeneration work appropriate.
- Need to work on thermal cooling or ensure that it won't break down in the absence of seeds or catalyst chamber.
- Should really evaluate the tie to reach 10-15 bar as opposed to 500 bar and evaluate if time to thermal runaway becomes increasingly concerning.
- Plans to solve the material handling problems are not clear.
- The prediction of novel B-C-N molecules with good thermodynamics is somewhat less interesting, since their synthesis researchers have also advised that they are likely to be unstable.
- Scaffold work is very intriguing but of questionable value to the development of ammonia borane as a practical hydrogen storage material.

Strengths and weaknesses

Strengths
- The thermochemical parameters of the system have been established.
- High capacity.
- PNNL was able to eliminate the induction period before the onset of hydrogen release from the ammonia borane scaffold material.
- Strong collaboration. High degree of relevancy. High degree of technical competency.
- Strongly leverages basic science.
- Manageable number of projects.
- Very good headway on kinetics - Work on scaffolds to improve H_2 purity is important however cost effective techniques must be stressed in early design of scaffolds. Would aerogels be appropriate for scaffold use?
- Good discussion of "critical issues"

Weaknesses
- Far too little emphasis on development of methods for regeneration.
- The scaffold ammonia borane material has relatively low gravimetric capacity as shown in the table on slide 21. Even the neat solid capacity is not likely to meet DOE targets.
- Handling solids on board a vehicle should be considered only as a last resort if the material capacities and energetics of the reaction are much more favorable than for other materials. Liquid based systems are strongly favored for vehicles.
- Multiscale modeling of reactor systems (transferred to MC) work on SBH with Millennium Cell seems to be of questionable value. Might just be diverting attention from real problem associated with SBH. No real surprising result here, and at least for SBH, this might just be diverting attention from the real problem

associated with SBH. However, this tool could be useful to other chemical hydride systems, and might serve as a model for these other materials when it comes time to build a system out of them. So, this work, if continued, should stay quite general so as to be applicable to a wide variety of chemical hydrides, and not too narrowly specific to SBH.
- The future work does not address the hybridized solutions to take advantage of the seeding versus scaffolding.
- Glad to see that multiscale modeling tool has been handed off to industry.
- Thermal runaway is by far the most troubling aspect of this project. A key strength of these materials is that they operate at lower pressures and could permit the use of conformable tank concepts. Thus the time to reach 10-15 bar should be evaluated to determine if this will become a problem. They should not report time to 500 bar since the plan does not include putting these materials in compressed vessels. This may not change the total time significantly since the induction time seems to dominate the curve.
- Have they addressed H_2 purity?

Specific recommendations and additions or deletions to the work scope

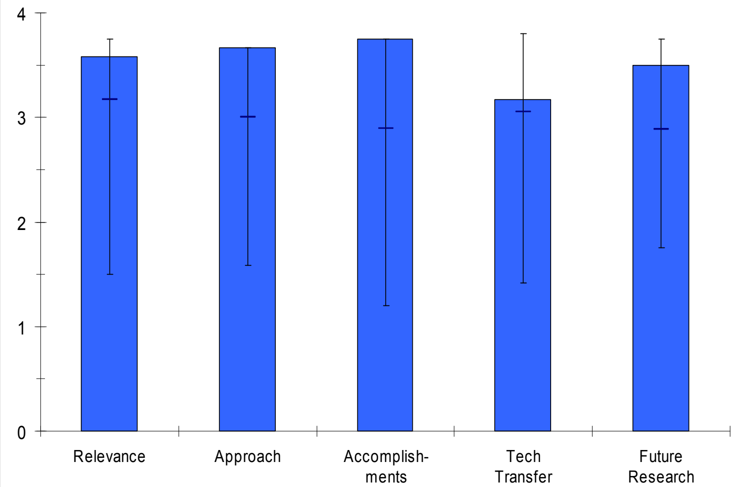
- Add more work on the development of methods for regeneration.
- Need to begin to develop the criteria for selection from the various alternative pathways.
- Heat management slide - showed near thermal runaway of pressure at very modest pressure (~50°C)? Isn't this a big concern? This could essentially become a dormancy issue similar to the concern over liquid H_2 - exothermic reaction pressure buildup could cause significant amount of fuel to vent during dormancy? To some extent, this is dependent on the design of the catalyst (i.e., seeding the material with ammonia borane as a catalyst vs. using a separate heterogeneous catalyst), so this design needs to be more clearly specified.
- The work on alternate scaffolding materials and structure is very important. Also need to address the spontaneous hydrogen release and runaway exothermic.
- Work on SBH multiscale reactor modeling should stay quite general so as to be applicable to a wide variety of chemical hydrides and not too narrowly specific to SBH.

Project # ST-03: Amineborane Hydrogen Storage: New Methods for Promoting Amineborane Dehydrogenation/Regeneration Reactions

Larry Sneddon; University of Pennsylvania

[Partner of the DOE Chemical Hydrogen Center of Excellence]

Overall Project Score: 3.6 (6 Reviews Received)



Brief Summary of Project

The objectives of this project are to:
1. Develop methods for on-demand, low temperature hydrogen release from chemical hydrides that can achieve DOE targets
2. Develop high conversion off-board methods for chemical hydride regeneration

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Materials show promising capacities, and hydrogen release is interesting.
- Hydrolysis work focuses on H_2 release, but regeneration is the critical question.
- Because of the high hydrogen capacities of the amine boranes, this work is very relevant.
- Storage is the key to the hydrogen initiative and amineboranes have potential for some of the highest capacities of all approaches.
- The material which they have studied has potential to achieve the DOE target.
- Making a stable and highly water soluble material that won't react without the presence of catalysts dramatically simplifies system design and potentially allows for a membrane single volume tank design.

Question 2: Approach to performing the research and development

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

- The combined approach of hydrolysis and thermolysis of the amine borane materials is excellent.
- They choose two contrasting processes; hydrolytic process which shows better kinetics but relatively lower hydrogen capacity, and thermolytic decomposition which show higher hydrogen capacity but worse kinetics. Those processes are considered to be the most important options for utilization of ammonia-borane as hydrogen storage material.
- Good approach to dissolve $B_3H_7NH_3$ in water and produce a stable liquid fuel. Like the use of ionic liquids as catalysts, solvents etc. would always want some reaction chamber with a catalyst for safety reasons.
- The approaches seem to be appropriate given the volume of results in the span of only one year.
- Really an incredible amount of work and clever ideas for such a small amount of money! This may be the highest return on investment in the entire portfolio!
- Good progress on H_2 release chemistry.
- To be relevant, hydrolysis processes must include release.
- Need to partner with chemical engineering and look at process for regeneration to determine first order economic estimates based on known regeneration chemistry.
- CF_3COOH is fairly exotic for large scale operations.
- Should seek regeneration routes that don't involve electricity to carry out reaction (think that AlH_3 requires electricity).

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

This project was rated **3.8** based on accomplishments.

- The research seems to have made considerable progress in one year.
- Good progress on new NB candidates for storage.
- This project has achieved some excellent and potentially quite important results, both with the ammonia triborohydride and with the ammonia borane. The catalyst results look very good.
- They characterized $B_3H_7NH_3$ as a hydrogen storage material by hydrolytic and thermolytic process, and reported large hydrogen capacity in a hydrogen evolution process and good kinetics in hydrolytic process. This result shows potential of ammonia-borane as an energy carrier.
- Interesting work on $H_3NB_3H_7$: the simpler, safer synthesis process is a nice discovery.
- Additions of $LiNH_2$ to AB: showed up to 9.6 wt.% at 85°C! This is really very exciting, and should be more fully investigated in the coming year. What are the decomposition products? What is the mechanism by which the second equivalent of hydrogen can be extracted from ammonia borane by adding $LiNH_2$?
- Regeneration of NB products (non-oxides) is encouraging, but need energy and material balance.
- Be careful with reactions which proceed to the formation of B-O bonds and to an energetic well that it will be difficult to get out of. For these systems, really need to prove that regeneration is feasible, or else consider moving onto more interesting activities.
- Need to comment on the solubility of the waste fuel in water. Is it better than $NaBO_2$? If so then, there is a chance to develop a membrane based fuel tank.

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

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

- There appears to be excellent interactions with the other members of the chemical hydrogen storage CoE.
- The PI is affiliated with the appropriate partners. This has become the best coordinated of the three CoEs.
- Improvement of hydrogen evolution kinetics and development of prototype systems with their partners will be expected.
- Should work with engineers to do early stage process design/economics.
- Outside collaborations are not apparent. Perhaps it is too early for many.

Question 5: Approach to and relevance of proposed future research

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

- The proposed future research paths are excellent. A key aspect will probably be which of the systems, ammonia borane or ammonia triborohydride, has the best potential for regeneration.
- Logical progression from first year's work.
- Good program going forward.
- Research and development of effective technology for production and regeneration for ammonia-borane system is strongly required.
- Some more direction and integration from the CoE is required to ensure that the catalyst work is coordinated well between all partners to avoid duplication.
- Focus on $AB/LiNH_2$ (and related systems): understanding mechanism and regeneration possibilities.
- For reactions involving B-O: try to quickly assess whether regeneration will be feasible; if not, consider dropping these activities in favor of more promising routes.
- Information that needs to be presented next time in order to gauge suitability for simple system design are: mixtures, pH, waste solubility.
- Some mention of regeneration pathways and energy should be made. B-O bonds are formed which could be troubling.

Strengths and weaknesses

Strengths
- Good work on Hydrogen release.
- This project is investigating hydrogen storage materials with high gravimetric and volumetric capacities. A new material system, ammonia triborohydride has been discovered, and this suggests the possibility of discovery of other new hydrogen storage materials.
- Knowledge of the subject and apparent willingness to work hard.
- Experience on ammonia-borane chemistry.
- Ionic liquids serve several beneficial functions, suppress borazaine, serve as catalyst and solvent and could potentially be cheap to make.

Weaknesses
- Too little emphasis on regeneration.
- The incorporation of ammonia-borane into an ionic liquid may reduce the gravimetric and volumetric capacities of the storage system.
- None obvious at this time, but outside collaborations are not apparent. Perhaps it is too early for much outside collaboration yet.
- Knowledge on hydrogen evolution system may be required when they improve performance of their materials. Intimate information exchange with LANL, PNNL, and private companies is expected.
- Ionic liquids are large bulky molecules. This could possiblly reduce the storage capacities of the system depending on how much liquid is required to for solubility, catalytic activity, etc.
- B-O bonds are still a waste product of this system. There may be similar regenerations to $NaBO_2$ - If not, then the PI needs to point this out as an advantage.

Specific recommendations and additions or deletions to the work scope

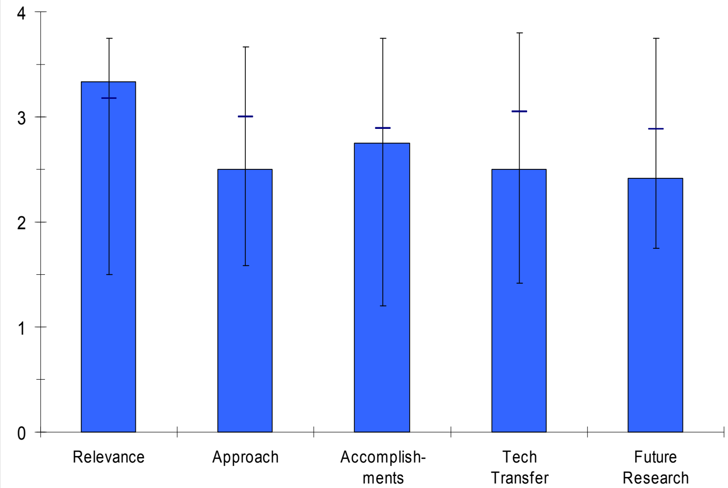
- Need to define optimum endpoint for dehydrogenation. Is it BN, BHNH, BH_2NH_2?
- For hydrolysis, ammonia poisoning of fuel cell needs to be addressed.
- None.
- None. Look at in six months to a year.
- Please include waste solubility, enthalpy of reaction and system pH values in next presentation.

Project # ST-04: Solutions for Chemical Hydrogen Storage: Hydrogenation/ Dehydrogenation of B-N Bonds

Michael Heinekey; University of Washington

[Partner of the DOE Chemical Hydrogen Center of Excellence]

Overall Project Score: 2.7 (6 Reviews Received)



Brief Summary of Project

The objectives of this project are to:
1. Understand the interaction of BN compounds with transition metals
2. Develop platinum group metal (PGM) based catalysts for dehydrogenation and rehydrogenation of BN compounds
3. Determine thermodynamic parameters for hydrogenation/dehydrogenation
4. Develop non PGM catalysts

Question 1: Relevance to overall DOE objectives

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

- As part of the chemical hydrogen storage CoE. U. Washington is studying ammonia borane chemistry, specifically thermal and catalytic dehydrogenation of ammonia borane and the reverse, hydrogenation. The work aligns well with the DOE program objectives, and is an important part of the CoE activities.
- Storage is key to the hydrogen initiative and amineboranes have potential for some of the highest capacities of all approaches.
- Effective catalysts for amino borane are required.
- Project has only addressed one of the three barriers claimed in presentation.
- Need to move on to efficiency and regeneration.
- Ammonia-borane holds a clear promise for meeting the DOE targets for hydrogen storage materials. However, it remains questionable whether the serious regeneration and cost issues about this compound and related compounds can be overcome that will allow its practical application to onboard hydrogen storage.

Question 2: Approach to performing the research and development

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

- Good approach on dehydrogenation.
- Thermally induced dehydrogenation of ammonia borane is known to be very slow at temperatures of interest. Therefore, the approach focused on discovering a catalyst(s) to increase the hydrogen release to appreciable rates at temperatures of interest. PGM catalysts were developed. This approach aligns closely with the DOE objectives as well as the CoE's program plan.
- Good thought, but Ir [iridium] is too expensive.
- Focus needs to shift away from Ir based catalysts since world supply of Ir will be a problem.
- Need work on regeneration.
- Not clear how extensive the understanding is of the targets, or hydrogen storage, generally, as a technology.
- There doesn't seem to be a clear strategy to move this project forward.
- Regeneration is the key issue with all chemical hydrides. The project is far too weighted towards improving dehydrogenation kinetics rather than solving the key issue of developing a practical regeneration processes. The practicality of systems involving ammonia borane dissolved in tetrahydrofuran solutions that contain a soluble homogeneous catalyst is questionable as it would seem to be precluded from practicality on the basis of weight, safety, and complication of tank design.

- Program lacks focus, trying too many approaches at once.
- There doesn't seem to be a clear strategy to move this project forward.

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

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

- A very active catalyst for the dehydrogenation of ammonia borane has been identified.
- Effective catalysts containing Ir were found to increase the dehydrogenation reaction rate two orders of magnitude over previously reported data. Further increases are necessary to approach transportation requirements. One hydrogen equivalent of hydrogen is released with this catalyst, and the reaction product is a solid, single cyclic pentamer.
- Good work to isolate unique dehydrogenation product. How long does catalyst last? Need to determine if deactivation is unique to this catalyst or if it's generic.
- They did create an effective catalyst that is reasonably reliable and reactivated under pressure (50 psi may be the limit for non pressurized systems though). However strong emphasis is required to reduce / eliminate precious metals. (multiple)
- If the only accomplishment is an Ir-bearing catalyst; this is not sufficient.
- At the outset, it seemed that a large effort would be focused on regeneration. Somewhat disappointed to see that nearly all the efforts involved trying to improve dehydrogenation kinetics with catalysts, as many others are also doing. The Ir catalyst work shows a remarkable improvement, but how practical is an Ir catalyst towards supporting the DOE goals (i.e., cost)?

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

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

- Good collaborations with PNNL and other CoE partners.
- There is evidence of good collaboration between University of Washington and LANL. It is unclear how much interaction there is between U. Washington and the rest of the CoE members.
- Clarify objective of work with PNNL on the Ir-bearing catalysts?
- This effort needs strong management form the CoE to coordinate catalyst work. This project is at risk for spinning in place and not working toward cheap catalyst production. More collaboration with the CoE partners would be useful.
- Need engineering partner to begin conceptual process design and economics for regeneration. Also need process design for dehydrogenation. What would on-board dehydrogenation reactor look like? How would solid be handled on a vehicle?

Question 5: Approach to and relevance of proposed future research

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

- Future work is laid out but is a little sketchy; e.g., the approach to look at rehydrogenation reactions. It was not clear what role the U. Washington will have in the downselection process being developed by the CoE. A positive aspect of the future work is the investigation of non-PGM catalysts.
- Need to concentrate heavily on identifying less expensive metals.
- Continued work on the Ir-bearing catalyst makes no sense, unless it is used as a surrogate leading to others.
- The practicality of development of solvent based systems is questionable. Studies of the re-generation of ammonia borane should be the highest priority.
- Doesn't appear to be a clear plan or direction for this part of the chemical hydride work.
- Interesting that one of the main future activities is to validate computational work, since the PI appeared to indicate skepticism towards computational work.
- Regeneration is key for this material. It should form the focus of future activities, but currently looks a bit like an afterthought (and not clear that there are any promising ideas here).
- Plans to study regeneration reactions, but no details. Need more info.

Strengths and weaknesses

Strengths
- Good work on dehydrogenation and characterization of catalyst and products.
- Strong background in homogenous catalysis.
- The presentation was clear and well focused. The Ir catalyst was shown to be fairly durable; 50 psi overpressure is enough to maintain catalyst activity indefinitely according to the presentation.
- Knowledge.

Weaknesses
- Less expensive catalysts are needed to demonstrate the viability of the process.
- Solid products of reaction are very difficult to handle on board a vehicle.
- If only one equivalent of hydrogen can be released, the maximum material capacity is only 6.5 wt. % which will fall below even the near-term system level targets.
- Far too little emphasis on development of methods for regeneration (multiple).
- No partners to look into engineering.
- Fixation on a dead-end approach, namely, the Ir-bearing catalysts.
- This presentation was difficult to follow, need to improve explanation of slides.
- Presenter needs to state in one simple bullet point the significance of each slide.

Specific recommendations and additions or deletions to the work scope

- Some effort should be focused on solubilizing the ammonia borane without excessive penalty on capacity or without introducing/producing toxic materials into the process. Investigate means to increase hydrogen equivalents. Has to be at pressure to prevent catalyst deactivation.
- The toxicity of the reactants and products needs to be assessed if there is serious interest in these materials. Also the presence of any volatile nitrogen-containing species needs to be determined. These species will be harmful to fuel cells and very possibly to humans if they are released.
- More work on the development of methods for regeneration. Studies of solvent based systems should be deleted.
- Give regeneration work more priority.
- Partner with engineering expertise.
- Re-orient or discontinue.

Project # ST-05: Chemical Hydrogen Storage Using Polyhedral Borane Anion Salts

Fred Hawthorne; UCLA

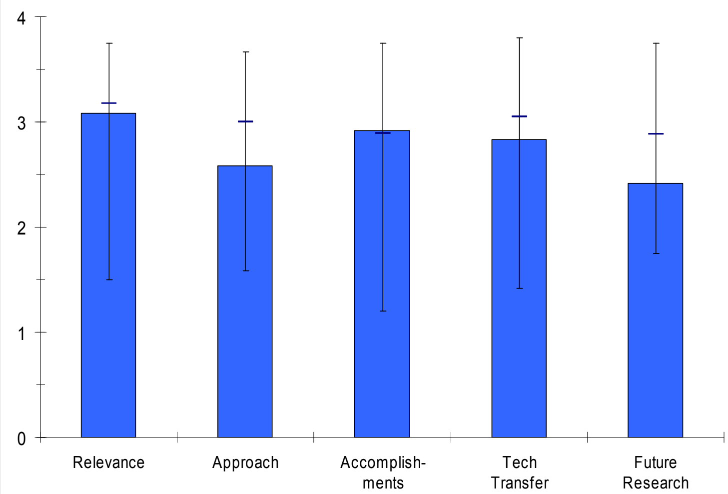
[Partner of the DOE Chemical Hydrogen Center of Excellence]

Brief Summary of Project

The objectives of this project are to:

1. Develop heterogeneous catalysts for the controlled release of hydrogen from the hydrolysis of salts of $B_{12}H_{12}^{2-}$, $B_{10}H_{10}^{2-}$ and $B_{11}H_{14}^{-}$ ions.
2. Determine the kinetics and mechanisms of these catalyzed polyhedral borane anion hydrolysis reactions to provide design data for large-scale hydrogen storage devices.
3. Optimize existing processes for the conversion of diverse >BH sources to $B_{12}H_{12}^{2-}$ and $B_{10}H_{10}^{2-}$ salts for direct use in hydrogen storage without extensive purification.

Overall Project Score: 2.8 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Generally, the area of chemical hydrides is of high relevance to the Hydrogen Fuel Initiative (HFI). Main issues for all chemical hydride approaches are density, kinetics, and (perhaps most importantly) regeneration. Need to keep regeneration at the forefront of the activities, and not treat as an afterthought.
- Mainly relevant to stationary applications. At 5 to 6 wt.% hydrogen based on materials alone, Polyhedral borane (PHB) anions probably won't make it for vehicles. Advantage is that the PHB anions are much more stable in H_2O than BH_4^-. The project is mainly focused on catalyst optimization and on H_2 release kinetics.
- Working to stabilize borohydride based materials.
- Storage is key to the hydrogen initiative and polyhedral boranes have potential for some of the highest capacities of all approaches; however, slide 7 indicates they can only approach the 2010 target.
- Polyhedral borane anions are a nice alternative to sodium borohydride and ammonia borane for boron based chemical storage of hydrogen. However, it remains questionable whether the serious regeneration and cost issues about these compounds can be overcome that will allow their practical application to onboard hydrogen storage.
- Hard to see how these low rates would work in a once through reactor that would be needed in a vehicle.
- Hydrolysis of complex boron hydrides appears to be an expensive option for H_2 storage.
- This seems like a good project, but the presentation was too slowl at first and didn't do justice to the results and future plans slides at the end.

Question 2: Approach to performing the research and development

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

- The PI has extensive knowledge of and experience in PHB anion chemistry. The research plan is well thought out, The results so far are encouraging, and the probability of a successful technical development in terms of a demonstrated H delivery capability for perhaps 2010 targets seems high.
- The progress to date seems to indicate approaches are appropriate.
- Good approach to find materials more stable than SBH however the materials identified are almost half the storage density of SBH. Taking a hit on the storage density.

- This approach seems to have many of the same weaknesses of the SBH approach, and the densities are not too high (~6 wt.%). Really need to show why this approach has some advantages over not only sodium borohydride, but the other chemical hydride ideas in the CoE.
- Regeneration is the key issue with all chemical hydrides. The project addresses improving dehydrogenation kinetics rather than solving the key issue of developing a practical regeneration processes.
- A B-O bond is still formed in the waste products and no mention of economical regeneration was presented.
- Needs to look at regeneration as well as H_2 release.
- Until process is demonstrated that doesn't use aqueous solution, state hydrogen capacity including water needed to solvate product.
- The overall economics were not discussed and may in the long run be a show stopper.

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

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

- Interesting compounds that are much more stable than SBH in water. This makes system design simple and avoids the use of excessive NaOH that creates a system of pH 14 and hence makes for difficult material selections. Formation of boric acid is concerning.
- Seemed to have good quality storage and kinetics data. It seems that a significant number of measurements have been made in the first year. Unfortunately the presentation rushed through the most important part of the results.
- A highly effective rhodium hydrolysis catalyst has been developed.
- Good results on H_2 release, Rh catalyst appears to work well for release.
- The research looks like good chemistry, but its unclear how guided this is by the DOE targets, or what the direction of the research is.
- O.K. But, only one catalyst is a bit disappointing, and the 90°C temperature is worrisome.

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

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

- This project is part of a larger integrated program involving Penn State, LANL, and PNNL, overall program planning and integration appears to be proceeding in an orderly fashion.
- The collaborator from Penn State is an excellent electrochemist. That bodes well for a successful outcome with respect to regeneration.
- Partnered with appropriate partners, good collaborations with CoE partners. (multiple reviewer comments)
- Need engineering expertise to consider how chemistry might be implemented.
- Slide 6 indicates that the partners have been identified, but collaborations have not yet begun.

Question 5: Approach to and relevance of proposed future research

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

- Reduction of catalyst cost is a key issue; thus the planned investigations of alternative transition metal catalysts are essential. Future plans should include efforts to develop regeneration process.
- Need regeneration component.
- Seems logical.
- Presentation of the future plans was rushed. They needed more amplification. As presented in the summary slide, all but the last one appear to build on experience to date.
- Future actions should concentrate more heavily on solubility, regeneration schemes, volumetric densities and enthalpy of reactions.
- Need to demonstrate reaction in a plug flow reactor.
- Should calculate target reaction rates.
- Not clear really where the research is going. Doesn't seem to be guided by DOE targets or driving by technological concerns?

Strengths and weaknesses

Strengths
- Good knowledge of BH chemistry.
- Hydrolysis of polyhedral borane anions salts at effective rates has been demonstrated.
- Knowledge of subject.
- The PI is a well recognized inorganic chemist with considerable experience in the field of study of this project.
- The class of chemicals under study has many favorable traits for a safe reliable H delivery system.
- Overall, the project team seems to be a strong one.
- This is a more stable material than SBH at lower pH which should significantly reduce overall system complexity.

Weaknesses
- This approach seems to have many weaknesses of the SBH approach (i.e., placing yourself in a strong exothermic energy well). If this is true, the work likely should be stopped; but if it is not true, need to demonstrate why this approach has (many, significant) positive attributes compared to SBH and other chemical hydride approaches.
- The full system economics associated with borane-based hydrogen storage are still questionable. This talk did not address either economics or the conceptual aspects of the proposed regeneration approach.
- No effort on development of methods for regeneration (multiple reviewer comments).
- Half the weight percentage of SBH/H_20 system.
- None discernible from the presentation.
- The presentation itself could have been much better if time was balanced to cover the information in the back end of the presentation (which happened to include some of the key results).

Specific recommendations and additions or deletions to the work scope

- Focus on non-noble-metal catalysts.
- Other spectroscopies, besides NMR, might add characterization depth to the project. Studies of molecular vibrations should be informative.
- Add solubility and regeneration schemes, volumetric densities and enthalpy of reactions for next presentation.
- Addition of studies aimed at the development of methods for regeneration.
- Continue on in a balanced fashion.
- Include engineering component to make sure work is relevant to onboard storage needs.

Project # ST-06: Novel Approaches to Hydrogen Storage: Conversion of Borates to Boron Hydrides

Suzanne Linehan; Rohm and Haas, Inc.

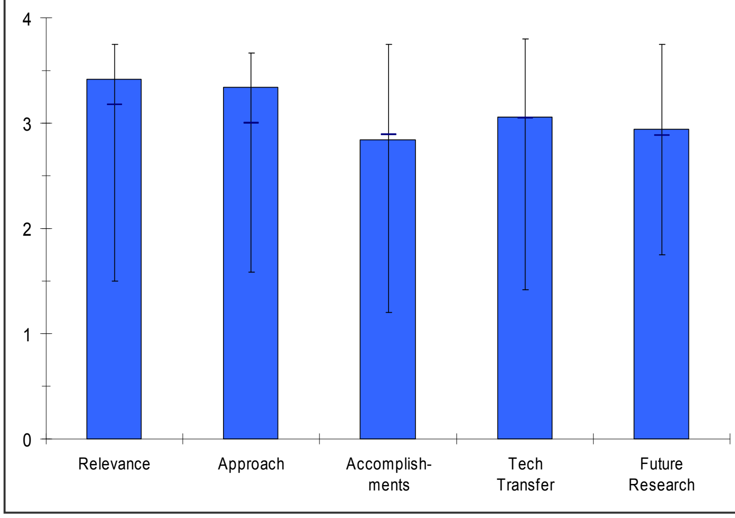
[Partner of the DOE Chemical Hydrogen Center of Excellence]

Brief Summary of Project

The overall objective of this project is to define and evaluate novel chemistries and processes to produce chemical hydrogen storage materials that meet DOE 2010 targets, and that have the potential to meet 2015 targets.

1. The primary focus is to identify energy efficient and cost-effective options for B-OH to B-H conversion.
2. A secondary objective is to leverage Rohm and Haas' expertise and experience across the entire CoE, assessing engineering requirements, economics, and life cycle inventory of hydrogen storage materials other than borohydride.
3. A third objective is to support DOE's Hydrogen Storage Systems Analysis Working Group in the area of chemical hydrogen storage analysis.

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- General area of B-OH to B-H, is an important item for regeneration of B-containing hydrolysis reactions. Regeneration is probably the most significant challenge for these systems, yet appears to receive comparatively little attention.
- The high practical potential of chemical storage of hydrogen as borohydrides has been long recognized. However, it remains questionable whether serious regeneration and cost issues can be overcome.
- This project has to succeed if borohydride materials have any chance as H_2 storage media.
- Performance based metrics are a good approach towards identifying best processes. A good support for H_2A tool.
- Storage is key to the hydrogen initiative and regeneration is key to many of the strategies.

Question 2: Approach to performing the research and development

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

- The approach seems like a reasonable one to take; simple modeling to screen out viable regeneration pathways.
- Regeneration is the key issue with all chemical hydrides. This project addresses this issue head-on. The three most promising approaches to conversion of B-O to B-H have been identified and are pursued.
- Engineering approach to metal reductions very good!
- Fast fail concept needs to be applied to more projects when appropriate.
- Need to verify properties of systems researched prior to making decisions or eliminating undesirable options, i.e. identification of leading metals for regeneration. More metrics need to be included prior to screening.
- Starting with an assessment of what is known is a logical approach. But, it is difficult to judge this criteria as so much is proprietary.
- However, this seems like work that should have been done earlier ago before even proposing the SBH idea? Cannot fault this project for this, of course, but it is quite surprising that this is just being done now.

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

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

- Promising new electrolytic processes have been identified.
- Promising results for electrochemistry, but current efficiencies will need to improve greatly.
- Use of metals to achieve reduction would be a very significant development.
- More information is needed on the chemical regeneration routes as little information was provided. No measurable metrics on ongoing regeneration routes discussed or mentioned.
- Seems like a very small amount of progress for the funding amount. The work seems competent and correct, but could have been done with much less time/money expended.
- Significant work to be done.

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

This project was rated **3.1** for technology transfer and collaboration.

- Good collaborations with CoE partners.
- Good work with partners within CoE.
- Slide 10 indicates active contact with partners for information. But, Slide 22 suggests that IP issues may be impeding.

Question 5: Approach to and relevance of proposed future research

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

- If anything is found theoretically, need to concentrate (urgently!) on demonstration of a process. If nothing is found theoretically soon, need to stop.
- The most important aspect of this work is that it could demonstrate clearly that hydrolysis/SBH technologies are not viable.
- Efforts seem well directed but timetable appears to be very tight.
- Ambitious goal for lab evaluation.
- Ammonia borane work looks good.
- Verification of metrics values obtained from literature. More focus on ammonia borane manufacturing routes needed.
- Seems logical.

Strengths and weaknesses

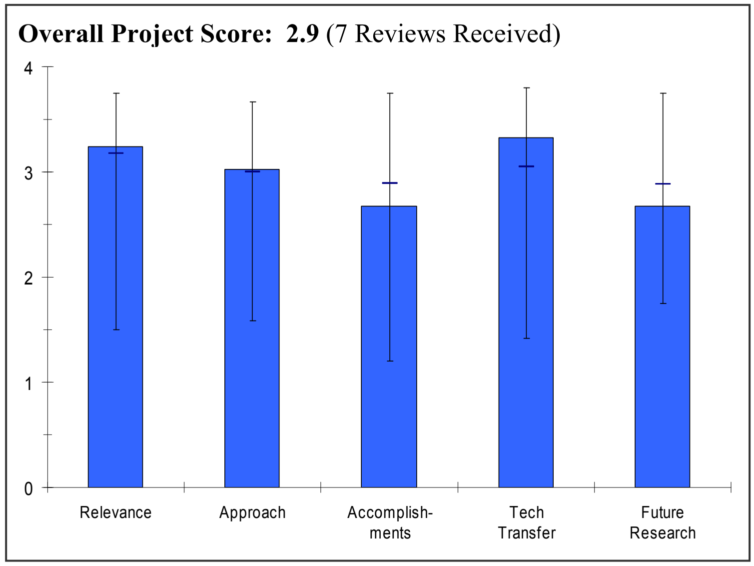
Strengths
- This is a key project as the practical viability of all boron based chemical hydride, hydrogen storage systems would seem to depend on the development of an economical method for production of borohydrides from borates.
- Good fundamental approach. Only project in CoE to be looking hard at life cycle issues.
- Collaboration with different members.
- In-depth knowledge of field with industrial perspective as to what is viable.

Weaknesses
- Economically viable methods for the conversion of borates to borohydride have yet to be identified.
- Data verification.
- None apparent, except maybe the extent of the study and potential IP issues.

Specific recommendations and additions or deletions to the work scope

- Coordinate with delivery or production teams since regeneration costs will be relevant to these teams.
- Use grid mix rather than hydroelectricity when doing life cycle analysis.
- Verification of results reported in literature prior to making any final judgments is needed for the project results to be of use and value.
- Continue, but look to see if anything can or needs to be done about the IP issues impeding the collaborations and information exchanges.

Project # ST-07: Development of Advanced Chemical Hydrogen Storage and Generation System
Ying Wu; Millennium Cell, Inc.
[Partner of the DOE Chemical Hydrogen Center of Excellence]



Brief Summary of Project

The objectives of this project are to:
1. Improve capability to store and release H_2 from chemical hydride
2. Meet the DOE 2007 target and beyond: 1.2 kWh/L (36 g H_2/L) and 1.5 kWh/kg (45 g H_2/kg)
3. Leverage Millennium Cell engineering expertise and guide chemical hydrogen storage CoE research

Question 1: Relevance to overall DOE objectives

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

- Borohydride represents the most viable, near-term chemical hydrogen storage system; however it may be difficult to meet longer-term DOE targets.
- Control of hydrogen evolution rate and stable operation for a commercial system is one of key issues for practical use.
- Storage is key to the hydrogen initiative.
- Good project to bring hydrogen goals to fruition.
- This project appears to be the only partner in the CoE that has demonstrated systems experience. Assumptions and system projections must be scrutinized heavily by reviewers or users.

Question 2: Approach to performing the research and development

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

- The approach is reasonable and appropriate.
- The use of modeling capabilities to identify areas for performance improvements is very useful.
- The approach takes advantage of Millennium Cell's extensive experience in borohydride storage technology.
- Development of the simulation software suitable for the $NaBH_4$-hydrolysis system is the key issue to design an effective construction for hydrogen evolution.
- Someone has to do the system work, and they seem to be doing it properly.
- Even though the number of project variables provide a challenge, the project team has done a good job in addressing them.
- Millennium Cell appears to not focus on the true issues of regeneration efficiency and is creating a simple compact system. The reactor chamber represents less than 5% of the weight of the system and currently works well (has worked well since 2003) it does not require as much attention as the other areas.
- Need to address the materials properties issues instead of focusing on engineering optimization, for example enhance wt.% of borohydrides in solution. What about wetting solid borohydrides with water studies?

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

This project was rated **2.7** based on accomplishments.

- Progress has been based primarily on use of modeling to identify areas for potential performance improvement.

- Model validation experimental activities are important - however presentation of validation results were confusing and not clear.
- Future work is strongly tied to the accuracy of the model and parametric inputs. Apparently validation measurements are made only at the exit of the reactor - data at other locations would be very useful in the validation process.
- Enhancement of borohydride wt.% in solution is a key item to improve H_2 wt.% efficiency, reaction mechanisms and kinetics studies need to be addressed and thoroughly studied prior to any engineering optimization studies.
- Development of the simulation software suitable for the $NaBH_4$-hydrolysis system has been completed, and effectiveness of the software has been shown.
- They are almost to the DOE 2007 target now. But, they have been at it for a long time.
- Good job so far.
- Need to support with data the claim for a system capacity over the previous state of the art of 1.8%.
- It appears unlikely that they truly has a system that has exceed the previous state of the art of 1.8%. They do not have a bladder system. According to previous additional studies, the bladder system failed due to material compatibility with high pH (14) and temp (160°C) of waste fluids. Heat transfer from waste to fuel causing premature evolution and crystallized fuel would tear any membrane. A bladder system (if it could be built) would not nearly double the system weight density (it would only reduce by 30-40 lbs the 200 lbs necessary), 30% fuel will create a semi crystallized waste fuel in the system and waste tanks.

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

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

- Collaboration with PNNL in the modeling activities are very significant.
- Additional collaborations with Rohm and Haas, PSU [Penn State] and LANL were mentioned but the nature of these collaborations was not clear.
- Seem to be working well with others especially PNNL.
- The project team has functioned well and has been productive.
- Partnered with the right partners as part of the CoE.

Question 5: Approach to and relevance of proposed future research

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

- Future plans were not discussed in oral presentation because time ran out. Future work should focus on ability of approach to meet 6 wt-% and other DOE performance targets.
- Plan needs modification to be able to meet DOE targets.
- This developed software is expected to be useful to design a hydrogen evolution system.
- Logical. Close to 2007 targets now.
- The successful completion of this project, when it occurs, will contribute to the commercialization of hydrogen.
- Future work on the catalyst chamber is not the issue. Regeneration and finding alternative ways to stabilize SBH without NaOH should be their primary goals; increasing solubility in a liquid medium is also important. [DOE note: Regeneration is investigated under a separate DOE project with Millennium Cell]

Strengths and weaknesses

Strengths
- Overall view is very clear.
- Strong background capability in this approach based on prior Millennium Cell experience and accomplishments.
- PNNL modeling contribution to this approach is very significant and future performance improvement will likely result from the utility of the PNNL model.
- System engineering know-how.

- Knowledge of design, modeling, and integration of hydrogen evolution systems with knowledge of hydrolysis of $NaBH_4$.
- Extensive knowledge with industrial perspective.
- Good team, solid performance.
- Only system supplier in the CoE.

Weaknesses
- System image for the evaluation in not clear.
- Apparent, inherent limitations of this approach to meet long-term system performance targets.
- It is not clear that proposed future work (reducing fuel and BOP volume and increasing fuel concentration) has a reasonable probability of meeting 6.0 wt.% system target.
- Need more focus on reaction kinetics studies prior to any reactor optimization efforts.
- None apparent form presentation.
- None noted.
- Regeneration of fuel and NaOH causing a pH of 14 makes economical material selection very difficult.

Specific recommendations and additions or deletions to the work scope

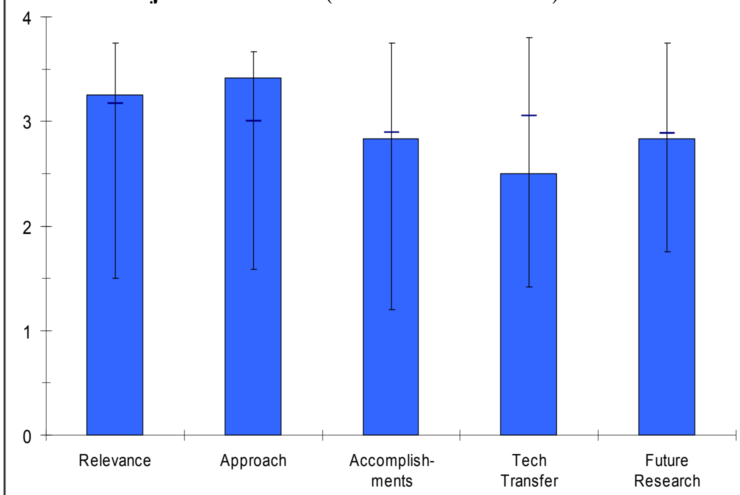
- System image is a little bit difficult to reach to the researcher. It is recommended that the PI explain the basic mechanisms, at the least. For the evaluation of the system image, it is recommended that the PI listen to CRA OEM's system engineer's opinions. For example, use of water for the hydrogenation shall include the antifreeze and condenser.
- The fourth quarter, 2007, go/no-go decision point is appropriate for this borohydride approach - all effort in the interim should be strongly focused on demonstrating the ability of the approach to meet required performance targets that will be the basis for the decision determination.
- What about wetting of borohydrides with water kinetics studies, basically avoid solutions? Reactor optimization is not needed. Using systems likely not to meet targets; modification of the project's focus is recommended.
- Continue as planned.
- None at this time.

Project # ST-08: Combinatorial Synthesis and High Throughput Screening of Effective Catalysts for Chemical Hydrides

Xiao-Dong Xiang; Intematix Corporation

[Partner of the DOE Chemical Hydrogen Center of Excellence]

Overall Project Score: 3.0 (6 Reviews Received)



Brief Summary of Project

The objectives of this project are to discover cost-effective catalysts for release of hydrogen from chemical hydrogen storage systems to enable deployment of on-board automotive hydrogen systems; and discover cost-effective catalysts for the regeneration of spent chemical hydrogen storage materials. The specific objectives for 2006 include:

1. Validate scale-up of catalyst from microgram to gram scale
2. Screen catalyst libraries for H_2 release from ammonia borane
3. Screen catalyst libraries for H_2 release from polyhedral boranes

Question 1: Relevance to overall DOE objectives

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

- Any fast throughput technique for catalyst discovery is welcome because catalysis is not an exact science. Fast synthesis is the only method to truly investigate a reasonable amount of possible candidates.
- Development of technique to search catalysts widely and quickly is important to develop hydrogen evolution system with higher rate for chemical hydrides.
- Should accelerate developments.
- Storage is key to the hydrogen initiative and catalysts are key to both hydrogenation and dehydrogenation at acceptable and cost-effective rates.
- Scope of the catalyst search is limited to only well known chemical hydride systems with well known limitations that limits overall probability of success of the project.
- PI doesn't seem to be too concerned with hydrogen storage and targets. The approach seems to be to just take materials and systems from their collaborators, and search for catalysts. I don't think that this is necessarily bad, but just need to ensure that their collaborators are always giving them the right things to work on.

Question 2: Approach to performing the research and development

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

- Any fast throughput technique for catalyst discovery is welcome because catalysis is not an exact science. Fast synthesis is the only method to truly investigate a reasonable amount of possible candidates.
- Very interesting approach to catalyst discovery (largely a "black art"). High-throughput seems like the perfect tool to apply to this problem.
- How high is the throughput? Are these "continuous" libraries of samples, or physically-separate samples? It would be appreciated if Intematix could be a bit more open about their experimental approach, and describe in a bit more detail.
- Should focus a bit more strongly on regeneration catalysts.
- They contribute to this chemical hydride storage CoE based on their high potential for combinatorial technology.

- Solid combinatorial approach.
- Screens appear to work.
- Proven. Seems to work.
- Not sure what "proprietary screening of libraries for catalytic activities" means on slide 10-- Are these libraries that are being developed from this testing or from their own previous work? Should some of this info be accessible to the researchers sending samples?
- Experimental design is good within the scope.

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

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

- Most of their targets have been achieved.
- Good discoveries of cheaper catalysts.
- Lots done in one year.
- Not completely clear how catalyst results are scaled from high-throughput scale, to larger scale? Are the ones identified via high-throughput the same ones that show good behavior in the larger-scale bulk testing?
- Showed data for screening of catalysts, but it's unclear exactly did they find anything that was really good? This is something they need to be demonstrating very soon (or the chemical CoE needs to demand this soon)!
- Minimal results were presented and even then they were difficult to understand. No attempt to quantify or explain the results were made. PI should take a more active role in developing the catalysts rather than just providing a service to the CoE.
- The CoE is not working on hydrogen release from $NaBH_4$. Not clear why the high throughput experimentation is being done in this area.

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

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

- They collaborate well with PNNL and LANL on reaction mechanism.
- Effectiveness depends on CoE arrangements to share IP coming out of combinatorial work.
- Working through PNNL and LANL to get to Rohm & Haas and Millennium Cell; probably because of IP concerns.
- They are sufficiently collaborating with the appropriate partners due to their association with the CoE however they need to produce more results and take a more active role in the development and understanding of these catalysts.
- Only three collaborators within the CoE have been mentioned. Not a great deal on collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Future work should focus more strongly on regeneration catalysts. Right now, it seems like an afterthought. Not sure that the SBH work is really adding much value.
- Extension to regeneration is a good direction if screens can be developed.
- More of same, but logical.
- Work should be dropped on $NaBH_4$ catalysts unless they will be useful for other materials.

Strengths and weaknesses

Strengths
- Technique to search catalysts widely and quickly. Their contribution is very effective for the CoE.
- Combinatorial approach.
- Knowledge of their approach.

- Fast throughput method is key; appropriate equipment for the job.
- Applications of heterogeneous catalysts to improve dehydrogenation of chemical hydrides as well as attempts to prepare nano-particles to improve kinetics are promising.

Weaknesses
- None really apparent from presentation, but concerned about transferability of results due to proprietary nature of approach.
- Lack of understanding of the catalyst and poor presentation and explanation of the results obtained.
- Not clear why $NaBH_4$ and NH_3BH_3 were selected. Not clear now heterogeneous catalyst can be separated from spent system that may contain solid products. Catalyst regeneration options are not addressed. In case of ammonia borane, product composition issues (hydrogen purity) were not addressed.

Specific recommendations and additions or deletions to the work scope

- Future work should focus more strongly on regeneration catalysts.
- Create mechanism for other CoE partners to send early leads to combinatorial effort so that combinatorial efforts could take leads and fully explore them.
- Continue, but watch lack of interaction with R&H and Millennium Cell to see if anything can or needs to be done to foster more interactions.
- Shift focus towards ammonia borane and other more promising chemical systems. Suggest abandoning sodium borohydride dehydrogenation work as irrelevant to the chemical hydrogen storage CoE.

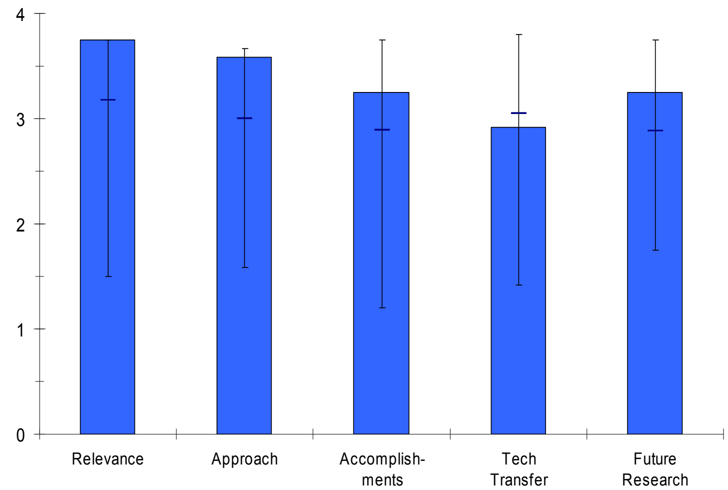
Project # ST-09: Hydrogen Storage by Reversible Hydrogenation of Liquid-phase Hydrogen Carriers

Alan Cooper; Air Products & Chemicals, Inc.

Brief Summary of Project

This project is dedicated to the development of reversible organic liquid-phase hydrogen carriers for the delivery and storage of hydrogen. These liquid-phase carriers can be used to transport hydrogen from production sources, using the existing liquid fuels infrastructure, to sites where they can release hydrogen by dehydrogenation for stationary power applications or be dispensed to H_2-powered vehicles. The overall objective is the development of liquid-phase hydrogen storage materials with capacities of >7 wt.% and >60 g H_2/L and associated dehydrogenation and hydrogenation catalysts, and scale-up of liquid carriers for use in systems engineering activities as part of an associated DOE production/delivery project.

Overall Project Score: 3.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Focused on exploration of heterocyclic chemicals that can give >7 wt.% and >60 g/L reversibly.
- A total system approach is being taken.
- The materials are expected to meet the 2010 target.
- Very promising approach
- No materials that meet DOE targets yet
- Liquid approach has definite benefits that no other approach has. Don't necessarily need high-pressure storage, so it is possible that tank could be conformable.
- Only proposed endothermic release reaction that is proposed to be regenerated (exothermic) off-board. This gives some energy efficiency advantage (energy released during recharge is partially recovered).(but couldn't all of the endothermic reactions adopt this approach? Should some of the concepts in the metal hydride CoE explore this idea?)
- This project is perhaps one of the most promising of Chemical hydride approaches. This concept potentially may achieve the best well to wheel efficiency and onboard safety and cost.

Question 2: Approach to performing the research and development

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

- One of the best examples of how computational modeling has led experimentalists to a proved-out promising hydrogen storage reaction. Serves as the model for this type of approach.
- Excellent use of theory to guide experiment
- Their approach is considered to be appropriate to develop organic chemical hydrides.
- The approach that Air Products is taking seems to be very effective. Solutions to key technical barriers are being emphasized in the research.
- The testing is sufficiently combinatorial in nature that over 100 candidate molecules were tested in the past year.

- This project is perhaps one of the most promising of chemical hydride approaches. This concept potentially may achieve the best well to weal efficiency and onboard safety and cost.
- Could perhaps some small amount of ionic fluids be used to catalyze the reaction and keep it liquefied at all times?

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

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

- Conformer results are interesting and suggest potential new routes to lower temperatures
- The finding that molecular conformation is important provides seminal insight about the types of molecular structures to most beneficially explore.
- New materials from last year have better capacity
- Lower temperature for initial step
- Incremental but necessary improvements were made this year ~ 1% g.d but no mention of volumetric or viscosity improvements were made.
- They have developed promising organic hydrides for hydrogen storage. Some of them exceed 6 wt.% and release hydrogen around 200^{o}C. Especially, N-ethylcarbazole releases hydrogen about 100^{o}C, although the hydrogen capacity of the compound is slightly lower than 6 wt.%. Active research and development on chemical hydrides are outstanding.
- Significant progress has been made in the past year. New, higher capacity compounds have been identified.
- Showed some new results on novel molecules, catalysts, etc. The results are good, but this is a large project, and so expectations are quite high; A stunning amount of progress in last year's meeting, but this year, the progress wasn't as substantial(?) Hopefully, this is not standard.

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

This project was rated **2.9** for technology transfer and collaboration.

- In response to prior year comments about lack of coordination with other institutions, four new collaborations are either underway or being formulated.
- Not as significant since this is independent project and APCI has skill sets needed for project
- They have submitted some papers important in the field of organic hydrides, although interaction with other institutes and projects is not described.
- Not really applicable in this case since this program and material is unlike any other in the CoE. Air Products has all the expertise that they need but could perhaps benefit from organometallic chemists and ionic fluid chemists to investigate further options.
- Perhaps Air Products can team up with some fuel systems suppliers or polymer suppliers to work on single volume exchange tank design.

Question 5: Approach to and relevance of proposed future research

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

- Main two things that this project needs to focus on are (a) bringing the temperature of desorption down even further, and (b) trying to figure out a way to exceed the ratio of 1 H:C. So, their focus for next year is very good, but with the funding amount, substantial progress will be expected.
- A bit more clarity on what the ideas are to exceed 1 H:C would be valuable. What are the theoretical upper bounds, and what temperatures are really thought to be practical for extracting a second mole of hydrogen from these carriers?
- It seems as though the options of materials to choose to fit the appropriate window are becoming more limited which may be disturbing. Would be interesting next year to investigate options such as blending other H_2 storage fluids that are exothermic in release to tune the enthalpy of the system. Or to try ionic fluids for viscosity / catalytic effects.
- Needs to explain how they will improve on more than 1 H per carbon/molecule and also explain a bit about how to liquefy the system.

- Future plans look good
- Challenge of going to higher degrees of unsaturation is a good stretch target
- Need to address melting point issues
- Development of regeneration technology and accumulation of toxicity of organic hydrides should also be emphasized.
- The future work plan is indeed focused on resolving technical barriers in a relatively short time to maintain pace with project schedule.

Strengths and weaknesses

Strengths
- Strong approach
- Good continuing accomplishments
- Approach can use existing infrastructure
- Ability on organic synthesis and knowledge on organic compounds and catalysts are outstanding.
- The PI did an excellent job of presenting the program. That helps shape opinion but it also lends confidence that the planning and execution are being done thoughtfully and thoroughly.
- The class of molecules being investigated is one that could at some level provide adequate on-board storage, delivery, and regeneration capabilities suitable for implementation in a prototype vehicle in the not too distant future.
- The fact that this material will not react without a catalyst greatly improves their chances to build a safe system and incorporate a bladder tank system that won't evolve H_2 prematurely due to heat conductivity form waste fuel across membrane to the fresh fuel. The material is not corrosive or abrasive as compared to SBH which also greatly improves their chances; good understanding of overall system.
- The approach of liquid hydrogen carriers is advantageous to most of the other approached since it covers transportation, delivery as well as on-board and off-board storage.

Weaknesses
- System integration for hydrogen evolution.
- Achieving 2015 wt.% H and g H/L targets might be out of the question for these materials.
- Future Material selection may be limited. Most materials selected are limited to the 7% range which may be too low to build an adequate system around. This is why it is crucial that they explain how they can break this barrier in order for this program to remain relevant and promising.
- Useable hydrogen capacity is not considered (that is measured capacity minus hydrogen equivalent of the energy requirement to maintain the system at required operating conditions).

Specific recommendations and additions or deletions to the work scope

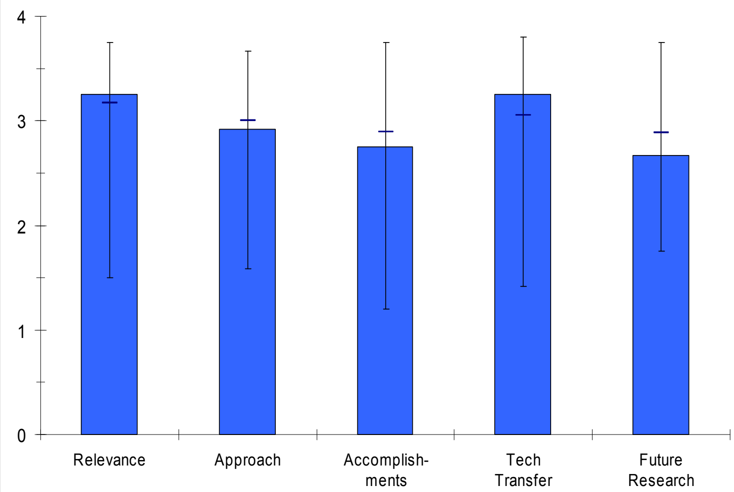
- Discuss/address effect of catalyst on confomer mix. Probably have already, but would be interesting to hear.
- Address what calculations say about hydrogenolysis.
- The pace of collaboration development needs to be stepped up to start getting benefit from other institutions in selected problem areas. Stronger collaboration with appropriate members of the chemical hydrogen storage CoE is recommended.
- Continue efforts to reduce or eliminate platinum.
- Would be interesting next year to investigate options such as blending other H_2 storage fluids that are exothermic in release to tune the enthalpy of the system. Or to try ionic fluids for viscosity / catalytic effects.
- Needs to explain how they will improve on more than 1 H per carbon/molecule and also explain a bit about how to liquefy the system.
- Conduct systematic research on catalyst development in conjunction with the liquid carrier development to include both the factors simultaneously in the experimental design (statistical, combinatorial). Pay more attention to regeneration of hydrogen carriers (hydrogenation step) as hydrotreating reactions may occur. In addition, a broader collaboration in the area of catalyst development is highly recommended.
- The system development for the Air Products' materials is currently unfunded. The two Air Products' projects depend on each other and it would represent the only other true system work being done aside from Millennium Cell.

Project # ST-10: Complex Hydride Compounds with Enhanced Hydrogen Storage Capacity

Dan Moser (PI); Susanne Opalka (co-PI, presenting); United Technologies Research Center

Brief Summary of Project

Overall Project Score: 2.9 (6 Reviews Received)



United Technologies Research Center (UTRC) and the project team are developing new complex hydride compounds capable of achieving hydrogen capacity greater than 7.5 wt.% H_2 and 0.05 kg H_2/L, and discharge rates greater than 0.02 g/s/kW. The focus is on fully hydrided phases with the formula: $Ak_wAe_xM_{+iy}(P_{\pm j}H_z)(w+2x+iy)$ where z =| 1±j |, formed in the multi-dimensional phase spaces between alkali (Ak) and/or alkaline earth (Ae) hydrides, d-metals (M), p-metal hydrides (alane, borane, or ammonia) (P), and molecular hydrogen (H_2). The project team is using first principles modeling to guide and accelerate the discovery of new complex hydride compounds with solid-state, molten-state, and solution-based processing methods. The project activities include: conducting performance evaluations to select compositions for further development; optimizing dehydrogenation and hydrogenation catalysis with spectroscopic mechanistic studies and first-principles screening simulations; evaluating safety and compatibility; and outlining plans for synthesis scale-up and business case development for commercialization of hydrogen storage systems integrated with fuel cell power plants.

Question 1: Relevance to overall DOE objectives

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

- The project is fully focused on hydrogen storage, a critically important component of achieving the President's H_2 Initiative and RD&D plan.
- In principle, this project has the features of relevance that support the goals of the HFCIT Program. The problem is that they have only a few months left to make the project come to fruition and they have several challenging technical barriers to circumvent.
- The project mainly concentrates on hydrogen storage capacity with no attention to operability.
- The project supports the gravimetric and volumetric density according to the DOE RD&D plan, but does not support the discharging rate.
- Addresses 2007 targets, but not later targets.

Question 2: Approach to performing the research and development

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

- This project has set up a very nice approach combining computational approaches to screen out viable candidates and pass them along to experimentalists. However, though the approach looks nice in principle, the current year's results show only three exothermic reactions (i.e., reactions with precisely the wrong thermodynamics). It looks like the approach broke down somehow? Molten state processing is one of the few things this project has that others do not. (Others have first-principles modeling; others have solid-state and solution-based processing.) CALPHAD modeling is an interesting tool (and perhaps unique), but what is it used for in terms of materials discovery?
- The project is well focused on the key technical barrier of hydrogen density. The approach is well organized and balanced to integrate theoretical and experimental activities. The technical feasibility is experimentally

supported. The project has limited time to consolidate the results and make a conscious choice, because the time left and the concentration only on hydrogen capacity allow a partial evaluation of the investigated materials.

- The approach, as portrayed on several consecutive slides, is multifaceted and complex. There is certainly room for improvement in a number of areas, but on the whole the R&D strategy is generally well planned and executed. There is so much going on; it is hard to understand why more progress on the reversibility issues has not been made. Perhaps the problem is intractable for the particular class of compounds being investigated.
- Good combination of synthesis techniques. Ordered approach to exploring most promising areas of phase space. Appropriate use of various levels of theory to guide work.
- Project has nicely focused on DOE gravimetric and volumetric storage targets, with reasonable consideration of thermodynamic needs. Combined modeling (various techniques) and experimental work constitutes an excellent approach. Strong focus on alanates and amides could be questioned.
- Densification efficiency and macrokinetics may depend on sample scale; would be good to investigate scaling effects in the next stages of the project.

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

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

- Don't understand why they are looking at decomposition reactions that are significantly above the lowest-energy pathway? For instance, they claim in the talk that they might be able to get more than 8 wt.% out of the Li/Mg alanate, but that's only from a non-preferred pathway. The preferred pathway (as they point out) is exothermic/non-reversible decomposition, with a much lower weight percent. Why do they think that these non-preferred reactions are interesting? Had a very good layout for an approach to tune thermodynamics: but how did they come up with three exothermic reactions? How did they find System A and System B? Did their modeling approach really lead them to these discoveries or were they serendipitous? Seems like flawed logic is being used in adding compounds to the hydrided side of the reaction in order to improve the thermodynamics. These reactions have been demonstrated to have exothermic decomposition paths, and so adding compounds cannot really raise the energy of the decomposition.
- The progress is good on storage capacity. The technical progress on capacity is made by significantly increasing the desorption temperature. The PI does not present ideas to overcome and reduce the temperature within the fixed target.
- Clearly lots of work has been done in the past year and the project team deserves credit for that. But, other than the claim of some improvement in volumetric storage density, it is not obvious that enough progress has been made to allow the team to meet the coming November deadline. The results with co-reactants offer some hope but the team is still not where it needs to be on, e.g., the reversibility problem.
- New LiMgAl phase formation good result--dehydrogenation results good. How about hydrogenation? MBNH systems look interesting as well, but no rehydriding info.
- Good progress has been made, but limited hope for reversibility (perhaps not quite as critical a requirement anymore). The dual computational + experimental approach has been shown to be valuable.
- Would be good to use total exergy balance of cycles for hydrogenation - dehydrogenation at final system(s) selection (second law analysis of efficiency).

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

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

- The network of collaboration is well established and effective.
- The presenter did not elaborate on how the five collaborating organizations work together. One can only assume that each organization is effectively coordinated with and contributing to the aggregate activity. Collaborations beyond those among the five team members don't seem to be an important part of the project.
- Good partners.
- A nice collection of collaborations.
- Would be good to expand collaboration with leading laboratories of IPHE countries (Russia, China, India, etc).

Question 5: Approach to and relevance of proposed future research

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

- Approach to stabilize dehydrogenation seems unlikely to work, even in principle. More information about these ideas would be helpful in evaluating their validity. The methodology to screen and predict new reactions seems like it has generally failed this year; why is it likely to work in the future? System/material "scale-up" seems a bit premature, if promising materials have not yet been found. What is the plan here?
- The future steps are reasonable for the limited time available to the project completion. The PI must identify better final criteria for selecting the most promising candidates. The operability of the selected materials is not addressed in the final experimental work.
- The presentation did lay out what needs to be done to meet end-of-project goals. However, the barriers to meeting system gravimetric targets, hydrogen delivery requirements, and reversibility expectations may turn out to be insurmountable for the particular classes of metal hydrides being explored.
- Reversibility key for materials. Needs to be emphasized.
- This project should continue, but the PI is a bit vague on the future plans. It is far from clear how PI plans to accomplish reversibility. Will work continue mostly on the alanates and amides? Will the focus migrate to all-new systems?
- Designed reversible reactions are the most important part of the future plants. Would be rational use of 2006 budget for solving that problem.

Strengths and weaknesses

Strengths
- The project has developed a highly effective approach of theoretical and experimental work suitable for fast screening and analysis of novel materials. The network of collaborations and participating organizations is of value and able to maximize scientific development and technology transfer.
- Reported potentially achievable storage capacities are approaching 9 wt.% H and 90 g H/L.
- A very nice combination of multiple modeling and experimental verification.
- Well designed project; high level of theoretical and experimental research.

Weaknesses
- The project has progressed too slowly, because it has not been able to apply the developed approach in a timely manner.
- This team does not appear to be close to solving the reversibility problem. Reducing the exothermicity should remain the top priority task, even at the expense of storage capacity. The project team puts considerable stock in what the theoretical results indicate. There is no substitute for definitive experimental data.
- Alanates seem to be suffering from very low volumetric densities. There is a growing body of experimental evidence that the amides will never produce NH_3-free H_2 (in spite of project results suggesting the contrary).
- Not substantial.

Specific recommendations and additions or deletions to the work scope

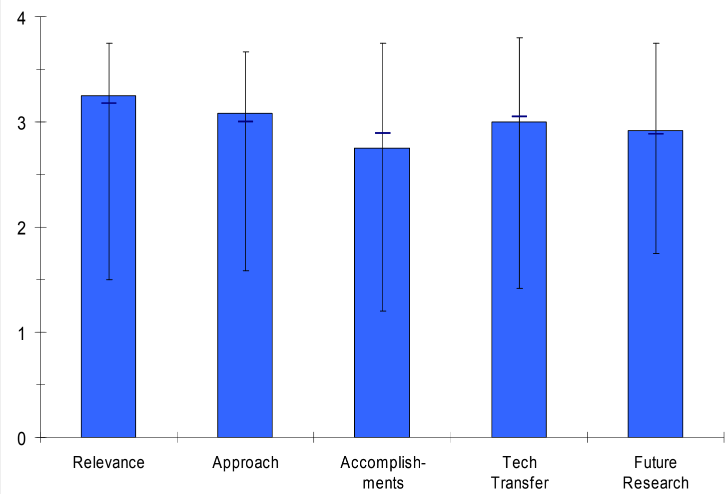
- The project has to include in the list of last period objectives the operability aspects.
- There is little time left to solve the remaining unresolved issues. Pushing forward with the composition(s) that come closest to meeting performance targets may be the best plan forward for the remainder of the project. Is an extension of the contract period appropriate or even possible?
- Does compaction of powder affect gas phase flow and mass transfer in bed?
- If possible, try to move away from the alanates and amides, both of which have their limitations. It may be that emphasis on requiring onboard reversibility may not be so necessary (i.e., the recharging heat problem). Consider extending this approach to the chemical (not-easily-reversed) hydrides. Give cost a higher weight in the project: cost of synthesis and/or off-board regeneration.
- Would be good to continue investigations and add project scope to problems connected with reactor discharging rate, sample-scaling effects on co-reactant segregation, heat and mass transfer, reactor design optimization etc.

Project # ST-11: Discovery of Novel Complex Metal Hydrides for Hydrogen Storage through Molecular Modeling and Combinatorial Methods

David Lesch (PI); Greg Lewis presenting; UOP

Brief Summary of Project

Overall Project Score: 3.0 (6 Reviews Received)



The objective of this project is to discover novel complex metal hydrides to enable a hydrogen storage system that can reversibly desorb 6 wt.% or more of hydrogen between -30 and 100ºC. UOP is applying methods of combinatorial chemistry and molecular modeling to discover materials with optimum thermodynamics and kinetics for on-board hydrogen storage. Virtual high-throughput screening will be used to screen complex hydrides to find materials which could meet the DOE system requirements and focus the synthesis effort on making the most promising materials. Even more importantly, the coupling of combinatorial experiments with molecular modeling of structural and thermodynamic properties will provide insights into the underlying mechanisms of action in these complex materials, permitting the design of hydrogen storage materials which would never have been envisioned otherwise.

Question 1: Relevance to overall DOE objectives

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

- Project properly aimed at solving the storage problem, an extremely important component of the President's Initiative and RD&D plan.
- Project fully supports multi-year RD&D DOE plan.
- This project is working on classes of materials that appear to be "dead ended" as far as DOE H-storage targets are concerned. They have given up on pure alanates (a wise decision) but do not seem to move towards storage material compositions that have a reasonable chance of meeting storage targets for 2015 or perhaps even 2010. The stated objective is 2010 goals.
- The materials sought would enable 2010 goals in theory.
- Conclusion regarding the end of testing of the alanate system is very relevant to the Hydrogen Fuel Initiative. It reinforces the sense of the community that this system cannot meet the DOE targets.

Question 2: Approach to performing the research and development

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

- The combinatorial approach is being implemented effectively and represents a valuable resource to the HFCIT Program as a whole. Even if one questions the choices of materials to measure that are being made in this project, there is much to appreciate about the measurement capability in place and under assembly.
- Definitely well designed.
- The ability to rapidly synthesize and screen compositions of metal hydrides is a valuable tool with which promising materials can be investigated throughout the material phase space. The approach to expand the investigation beyond the $LiNH_2$-MgH_2-$LiBH_4$ system does not appear well thought out or defined.
- The concept of combining first principle calculation and combinatorial experimentation is a good one. Project leaders seem to have chosen a ternary system derived from the literature ($LiNH_2$-MgH_2-$LiBH_4$), rather than an entirely new approach. A quick overview of the alanate limitations was useful.

- H_2 purities problem and thermal problem (heat of formation and effective thermal conductivity) are interconnected. Is it possible to extend model for that analysis?

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

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

- Nearly 1000 compositions screened and only one new mixed phase identified that has promise. Unfortunately, the gravimetric storage capacities being measured are all significantly below target values. The team deserves high marks for the amount of screening data accumulated but they just aren't finding promising performers.
- Recent MgH_2, $LiBH_4$, $LiNH_2$ work is most interesting result to date. New materials are always a triumph. While still not at the goals, it shows the power of the process and identifies new areas to concentrate on.
- The development of both model-based virtual high throughput screening in addition to combinatorial synthesis and screening capabilities are the most significant accomplishments in this project. However, it is well into the life of the project and the equipment is not yet in place and/or functional. Results have confirmed what others in the storage community have felt for some time; namely, that the alanates can not meet the DOE capacity targets. These tools can lead to greatly increased capacity to investigate promising materials.
- For two years of effort, the project seems to have started rather slowly. Some of the high throughput equipment is still not in place. To the project's credit, a final definitive closing of the alanates' door was helpful. The exercise on the ternary system was also interesting in proving the approach, but does not seem to be leading us toward a practical (low desorption temperature) end. There are complicated cyclic stability "composition equilibrium" problems.
- Is the 2nd cycle representative?
- Although the result of screening of alanate systems is negative, these findings are very important for future research (e.g., for those who are still working on alanates for on-board storage).

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

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

- The presenter did not elaborate on how the five partners work together. One gets the sense that most of the work is being done at UOP. Considering the amount of data generated in the past year, the team deserves the benefit of any doubt about how they function collectively.
- Good.
- There was little indication in the presentation that there is extensive and/or effective collaboration among the partners. Their contributions were not evident. Likewise there was no mention on how the tools will be disseminated to the community of researchers in these systems. There is no discussion in the presentation that relates this work to efforts within the metal hydride CoE.
- Good collaborations. It is not quite clear what each of the collaborators have specifically contributed during the last year.
- International collaboration in modeling can be extended with leading laboratories of IPHE countries.

Question 5: Approach to and relevance of proposed future research

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

- Armed with the capability to rapidly screen a bevy of compositions, the team is widening the search to include a broader range of metals and higher order systems.
- The plans for FY06 and FY07 are laid out in a general sense. Two go/no-go decisions are coming up. It is not clear which materials pathways will be selected for expanding the materials space of interest. There is a desire to find materials with heats of adsorption in the 20-50 kJ/mol H_2 but little indication of what materials will be considered.
- The future work seems generally good, but light on specifics. I have my doubts on whether the ternary $LiNH_2$-MgH_2-$LiBH_4$ work should continue (because of temperature, compositional instability, kinetics and NH_3 problems).

- Project has effectively planned its future.
- Not well defined. Li-NH_2 base systems have inherent limitations associated with cycling stability and hydrogen purity. In addition, it is not clear which systems using 20-50 kJ/mol and >7 wt.% capacity are proposed.

Strengths and weaknesses

Strengths

- The computational and measurement capabilities built up for this project are impressive. They represent a resource that could be broadly useful to the hydrogen storage community.
- Approach: willingness to move on when no good indications are found. Model guided experiment.
- The high throughput tools being developed in this project appear to bring improved ability to rapidly screen materials.
- A nice combination of first principle calculation and experimental (combinatorial) efforts. In some ways this work is similar to ST10 [UTRC materials discovery project], but somewhat different techniques are used and independent comparison of the two will be useful to DOE.
- Combinational experimentation and molecular modeling.
- The knowledge generated in this program will greatly contribute to the future research in terms of "what not to do". Clear conclusion was timely made in terms of what not to do in future research for on-board hydrogen storage.

Weaknesses

- Metal hydrides in any form most probably won't meet the current 2015 DOE targets for wt.% H and g H/L.
- High throughput synthesis seems to be taking a long time to bring on line. This is not a trivial item to create, but if it does not come on line and stay on line, the program loses its most powerful method to evaluate the new systems' actual ability.
- Progress appears to be a bit slow. DOE funding is relatively modest, but UOP is contributing more than DOE so one could reasonably expect faster progress.
- The past work on the alanates and ternary system was useful, but the latter should probably not be continued. (PI has already abandoned any future work on the alanates). Potential progress in an amide-containing system seems doubtful, given high desorption temperatures, low kinetics and probably ubiquitous NH_3 contamination. Increased activity on the borohydrides, although difficult and high-risk, should be encouraged.
- Analysis of results on the basis of cycle 1 and cycle 2 can not be representative for selection of final systems.
- Unfortunately, due to original selection of the scope of the project, based on current results the project has relatively low probability of success of meeting the DOE targets.
- Difficult to analyze the likelihood of future success since we are not privy to materials to be used in high throughput synthesis. Hopefully the power of the process will permit a breakthrough.

Specific recommendations and additions or deletions to the work scope

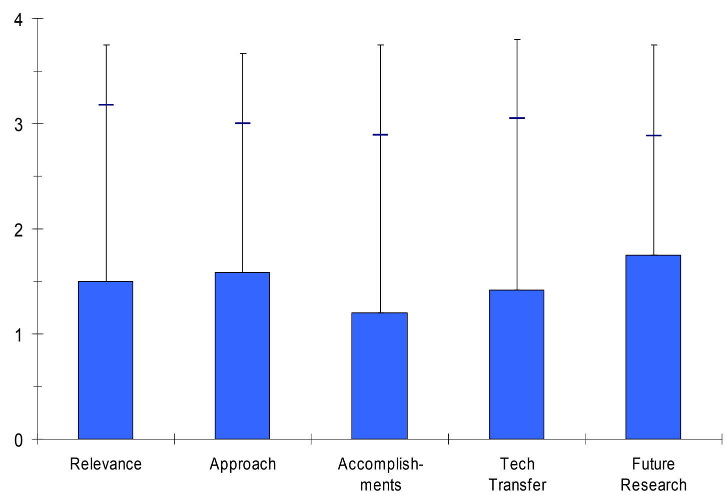
- With the combinatorial capability now in place, why not open the door to more than just metal hydrides. It seems that this project should have a substantive connection to the metal hydride CoE to avoid duplication of effort (or is that connection already in place).
- Improve interaction with the MHCoE to utilize these high throughput tools.
- The past work on the alanates and ternary system was useful, but the latter should probably not be continued. (PI has already abandoned any future work on the alanates). Potential profit in an amide-containing system seems doubtful, given high desorption temperatures, low kinetics and probably ubiquitous NH_3 contamination. Increased activity on the borohydrides, although difficult and high-risk, should be encouraged.
- Additions to project scope are not necessary.
- Add "useable" capacities for down selecting the candidates, which is measured capacity minus hydrogen equivalent of energy requirement to maintain the system at operating temperature on board. Identify new areas of search for new materials (other than alanates).

Project # ST-12: Hydrogen Fuel Cells and Storage Technology Project at UNLV
Clemens Heske (co-PI) Presenting; Balakrishnan Naduvalath (co-PI) University of Nevada-Las Vegas (UNLV)

Brief Summary of Project

The purpose of the project is to develop a fundamental understanding of interaction of atomic and molecular hydrogen with materials pertinent to the storage and use of hydrogen, thus enabling improved conceptual development, design and testing of storage options, fuel cells, and hydrogen combustion applications. The project emphasizes fundamental research at the atomic and molecular levels to understand the mechanisms of hydrogen adsorption/desorption from potential storage materials, catalysis of hydrogen adsorption and dissociation on platinum surfaces (fuel cell applications) and rate coefficients for atomic and molecular hydrogen interactions in both thermal and non-thermal populations (hydrogen combustion applications).

Overall Project Score: 1.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.5** for its relevance to DOE objectives.

- This project encompasses an immensely broad range of technical research; however, much of the work being done to date appears to be in developing the technical competence of UNLV. While this in itself is not troubling, the need to develop competence in so many diverse areas seems to be a poor use of federal funds. I would recommend that the PI concentrate on a very few (1-3) specific technical areas for concentrated effort which might provide a greater benefit.
- This work is not very well aligned. Much of it is not on hydrogen storage. Some of the storage work is in doubtful areas such as 300K storage in nanotubes. It may be helpful for UNLV but is not likely to advance H_2 storage. Metals on conducting polymers may or may not be productive depending on the actual work done.
- Partially addresses President's Initiative, but appears to be duplicative of other work already completed. Stated purpose during the review is to "increase the infrastructure of UNLV". It's unclear how this supports the President's Initiative.
- This project is very broad and fundamental. From a practical reference plane of the President's Initiative and DOE RD&D plan, it is very hard to see how the quantitative storage targets are addressed or how anything in that vein will come out of this in the 2006-2010 critical time frame (not to mention fuel-cell needs). Weight, volume, cost and refueling time considered secondary to vague fundamental understandings.
- This project is comprised of a family of fundamental research tasks proposed by individual investigators that touches on selected aspects of the science involved in hydrogen storage and fuel cell function. The relevance of this collection of tasks is hard to gauge because it seems to replicate much of the research already under way in the HFCIT Program. The project is very generously funded and with these funds they are building "infrastructures" for physical and chemical studies that are orchestrated to supposedly address issues of relevance to hydrogen storage and fuel cells, while also educating students.
- The project objectives are to improve UNLV capabilities in hydrogen storage and fuel cell technologies. At present, the project has little relevance to the DOE objectives to arrive at storage solutions that meet the DOE targets.

Question 2: Approach to performing the research and development

This project was rated **1.6** on its approach.

- Despite extensive research already being conducted in carbon nanotubes [SWNT] and $NaBH_4$ and growing evidence that neither SWNT nor $NaBH_4$ is likely to be able to approach the DOE storage targets, this project appears to be directing a substantial portion of their funding toward the pursuit of work that is unlikely to add to the scientific knowledge base nor to produce a storage material that can support the need for viable storage systems for hydrogen fueled vehicles. The PI should be encouraged to focus efforts in more fruitful areas.
- Many of the approaches are unlikely to yield practical results. As the speaker said, they are sharpening their tools; as such the work benefits them but is not likely to advance the storage of hydrogen. The $NaBH_4$ system is not suitable as a storage source for thermal decomposition. As noted the nanotube work has been largely abandoned elsewhere. Work at multiple GPa is of little interest, if that pressure were an option, modified AlH_3 would be the system of choice. The theory work may be of some value. Conducting polymer work seems a few years behind the work in the literature, hopefully they are replicating work done already to learn technique.
- Research within UNLV appears to be integrated. Project takes on too many tasks, therefore making project level progress difficult to assess in the future.
- The effort is presented as a bewildering array of poorly connected fundamental studies and people doing those studies. The relatively large number of materials to be incorporated are the subject of studies being done elsewhere and are nowhere supported as to their potential for achieving DOE gravimetric and volumetric targets. Stated objective is an interdisciplinary exercise. This seems to be a very fundamental effort with limited practical relationship to HFCIT needs.
- There is nothing novel or compelling about the research thrusts, the methods, the objectives, or the systems to be investigated. The presenter spent too much time on a few word charts. Summarizing the nature of at least some of the tasks through examples of results would have been more effective. The research is at such a fundamental level that it is not likely to reveal any near term insights of value to the HFCIT Program in meeting 2010 storage or fuel cell performance goals or overcoming major barriers.
- The approach appears to be to work in areas of interest to the individual investigators rather than a comprehensive, focused approach to develop hydrogen storage materials. The focus on fuel cells detracts from the storage effort and vice versa.

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

This project was rated **1.2** based on accomplishments.

- From information presented it is almost impossible to determine what has been accomplished. It appears that technical progress has been slow, despite high levels of spending. The presentation states that 66% of the project is complete which implies that some $2.5 million has been spent. One can always hope that once all the facilities are in place, progress to be reported next year will be substantial and more appropriately in line with the extremely high levels of spending committed.
- One wonders why they started a year after they received the grant, but for the moment they can not be evaluated on results as they have not had time to generate much information. However as much of this has already been done it is unlikely to be of any value.
- Only progress made is procuring equipment and selecting personnel, due to project starting in Jan. '06.
- Project just started apparently. Not much yet in the way of results.
- Given the level of funding allocated to this project, they should be expected to start producing seminal research results in a very short period of time. Based on what was presented, it looks like they are just getting started, so it is probably not fair to express concern about the absence of informative results at this time.
- The project is just starting so no progress as evident has been made.

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

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

- There appear to be no collaborations outside UNLV. The PI mentioned a desire to work with an auto OEM and to more closely collaborate with the centers of excellence, but there were no reports of concrete plans in those areas.
- Interaction inside the group seems possible (remains to be seen if it materializes) but clearly not very well attached to outside groups. The work chosen shows a lack of interaction with the cutting edge of practically meaningful hydrogen storage.
- No collaboration outside of UNLV currently, though it was mentioned that UNLV would like to work with others in the future.
- There are a number of UNLV collaborators, but no apparent connection to outside collaborators.
- The presenter mentioned several times that the project at UNLV would have appreciable outreach but that the outreach was yet to come. Because most (if not all) of the tasks in this project align with other ongoing HFCIT supported projects, it is essential that appropriate coordination/collaborations develop to avoid unnecessary overlap. The project management at UNLV should take the lead in fostering these interactions.
- At this point, the university investigators are just getting organized. No outside collaborations can be established until UNLV has capabilities and can reach out to collaborators. The management structure is not apparent. There does not appear to be one project manager who will try to keep the team focused on the end product.

Question 5: Approach to and relevance of proposed future research

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

- The presentation provided few details about future work planned, whether there are any specific targets or metrics, or whether or not there is a technical timeline with goals.
- Plans were not clearly laid in many cases.
- There is a large basket of fundamental studies proposed with little apparent connection.
- The future work plan as discussed by the presenter may lead to some interesting insights that guide other researchers to solutions for barrier/road block issues, however, the large funding base provided for this project would be better spent if it focused on finding near term solutions rather than on building infrastructures and starting a collection of loosely connected basic research tasks.
- The presentation indicated areas of interest to the individual investigators. Research pathways were not discussed.

Strengths and weaknesses

Strengths
- The biggest strength UNLV has is the quality of its researchers and the potential they have to make substantial contributions to the hydrogen program if only their work can be focused on relatively few areas that align with UNLV's strengths and are directed toward technical areas critical to the success of the DOE program.
- Number of people involved could lead to progress.
- Plenty of funding for the project. Many resources to deploy to the project.
- A variety of subtasks and scientific disciplines.
- It is not possible to discern any strengths attributable to this project at the present time. Next year's Peer Review should be a critical go no-go decision point. Major pay backs in terms of demonstrably useful research results should be forthcoming or at least in evidence at that time.

Weaknesses
- There are too many different projects in too many different technical areas. Too much work appears to be directed toward carbon nanotube research and to $NaBH_4$. Unfortunately these areas seem to have little value as viable storage media. Very little has actually been accomplished technically.
- Choice of target systems, too much of the work seems meant to bring the labs new equipment and to teach and to allow the PIs to enter the hydrogen storage field, and thus a substantial part of the budget is to bring the groups up to speed, not to advance hydrogen storage.
- Lack of collaboration with industry and/or national labs. Scope of project is too broad. Capability of UNLV in the various subjects covered by this project not clear.

- No logical connections among the subtasks or near-term connections to practical needs.
- The project tasks give the impression of being 12 wish list items for a selected group of chemists and physicists. There is no engineering component and that is without question a serious omission if the goal of the UNLV project is to have a beneficial impact on hydrogen storage and fuel cell development. There seems to be a preponderance of emphasis on electronic structure. This aspect of the project could be consolidated and reduced to allow some engineering studies. Federally funded programs should not be about giving scientists what they want; it should be about getting useful science done.
- This project lacks focus. Tasks appear to have been selected on the basis of the individual PIs' area of interest and expertise and not coordinated with each other in a focused approach to address a particular barrier or issue.

Specific recommendations and additions or deletions to the work scope

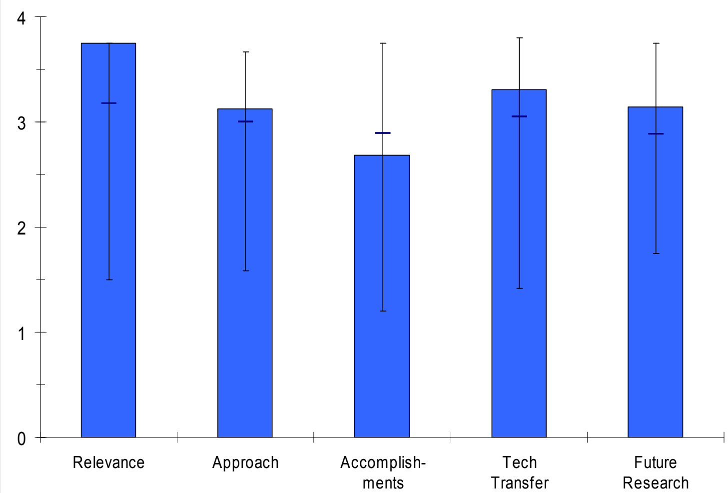
- Find partners with practical interests.
- Recommend stopping work on $NaBH_4$ and nanotubes. Consider increasing work on fuel cells, e.g., thin wall membranes.
- Redirect research to something more aligned with the goals. The work with potential to advance its actual field is more basic in nature, for example the modeling work on collisions, and so this should be funded by NSF or BES, not EERE. The primary aim is to buy UNLV equipment as best can be seen.
- Narrow scope of project.
- Link more closely with DOE to align project with DOE needs.
- Focus work on areas not previously addressed.
- Define some real connections to DOE targets and the RD&D multiyear plan. Find partners with practical experience.
- As presented at the review, this project has all the features and attributes of a centralized research activity that should be sponsored by the DOE Office of Science rather than by EERE. At next year's HFCIT Peer Review, this project should be given a one hour slot in which the presenter(s) demonstrate(s) that they are (1) making or on the verge of making contributions that are relevant to HFCIT goals, (2) developing a strong outreach to the HFCIT centers of excellence and other contributing institutions/organizations as appropriate, and (3) diversifying their task structure beyond fundamental physics and chemistry studies to embrace some engineering studies.
- Propose a management plan that identifies a leader who has the authority to direct UNLV's efforts and maintain focus on the hydrogen storage targets. Short of that, perhaps some of the CoEs could reach out to UNLV to bring up to speed those investigators whose interests most closely mirror those of that CoE.

Project # ST-13: Research at Sandia National Laboratory as part of the DOE Metal Hydride Center of Excellence

Lennie Klebanoff; Sandia National Laboratory-Livermore

Brief Summary of Project

Overall Project Score: 3.1 (8 Reviews Received)



[NOTE: This review is on Sandia's contributions, not on the entire DOE Metal Hydride Center of Excellence. Each of the partners is evaluated separately.]

Sandia National Laboratories has been selected to lead the DOE metal hydride CoE, which is composed of 8 universities, 3 industrial partners and 5 other national/federal laboratories. The CoE is focused on developing new complex hydride materials capable of achieving at least 6 wt.% system hydrogen capacity, improving kinetics of absorption and desorption and thermodynamic plateau pressures, and improving processing and doping techniques that will lower cost. Current materials under study include advanced complex hydrides, destabilized binary hydrides, novel intermetallic hydrides, and other reversible hydride-based materials. In addition to new materials discovery, Sandia will work with all CoE partners in fundamental modeling, materials synthesis and modification, testing of hydrogen storage and delivery characteristics, and engineering science and process development to support and guide the materials discovery efforts.

Question 1: Relevance to overall DOE objectives

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

- The work is firmly pointed at the key needs of the program across all its efforts.
- The metal hydride CoE presentation was well orchestrated, clearly and concisely presented, and left a good impression of the functionality of the CoE. This is a major HFCIT program that is designed to be well aligned with the hydrogen vision. The multi-presenter format worked well because each major topic was covered by a PI working in that topical area.
- The effort completely supports the Hydrogen Fuel Initiative and Multi-Year RD&D Plan for Storage.
- Hydrogen storage is the key enabling technology critical for the success of hydrogen as a transportation fuel. Any work which can further understanding of the basic science of hydrogen storage is vitally relevant to DOE objectives, the President's Hydrogen Initiative and the objectives of the Multi-Year RD&D Plan. The metal hydride CoE is a key research activity which supports all these objectives.
- Obviously, the CoE provides strong value towards DOE targets.

Question 2: Approach to performing the research and development

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

- Management approach is good and well thought out. The individual projects are generally well oriented as well.
- The full set of capabilities established or in the process of being established by the MHCoE (at SNL and partnering institutions) represents an impressive array of tools for the study of hydrogen in metal [hydrides]. Emphasis in the program as a whole is on demonstrating DOE targets for wt.% H and g H/L. The work plan is in accord with community wide understanding of what needs to be done to develop a suitable metal hydride

storage material that meets the DOE targets. The lead presenter made it clear that the work plan is a living plan and that the plan is revisited and revised as appropriate to make changes in emphasis stemming from discoveries, from modeling guidance, and from results that show selected material candidates have no hope of meeting target values for hydrogen storage.
- This is due [the score?] to ambiguous decision points and a lack of clarity surrounding the details of go/no-go decisions. Need to make these decision processes more transparent.
- This is a large effort with many different useful facets. The work described largely covered SNL parts of the CoE. Only a brief introduction to the CoE was given. Other CoE components were presented elsewhere. The overall SNL approach is a combination of theoretical and, more so, experimental techniques. This approach is comprehensive and cannot be faulted. There is some overlap with other efforts within the CoE and world in general, but that is OK.
- The CoE is taking a logical, methodical, and thorough approach to addressing the barriers to a viable hydrogen storage system.
- Destabilizing the thermodynamics is key towards achieving any kind of realistic system. Fast throughput and modeling of materials is crucial to identifying new materials. Either method on its own will be unreliable.
- CoE structure well organized.
- Good management of CoE.

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

This project was rated **2.7** based on accomplishments.

- Amide work on durability is an important contribution, even though it would have been nice if the durability could have been improved. Hopefully this will guide the type or amount of work in this area. LiAlN compound is interesting development though does not seem practical in current form. New complex hydrides are also interesting but almost everything seems "in study" with no hydrogen results. Theory used to pick systems is a good development. Theory approach used seems good. Monte Carlo was a significant step forward in this area.
- In summary, several of the reported FY 2006 accomplishments included (1) ruling out alanates, (2) getting ca. 5 wt.% reversible with Li-Mg-amide, (3) predicting ca. 10 wt.% (theoretical) for Li-Al-N phases, (4) initiating study of nine new materials, including Li-Al-K phases and $Ca(BH_4)2$, and (5) developing a mini hot plate array for combinatorial thermal treatments. Progress in FY 2006 was good but not outstanding in terms of moving closer to DOE hydrogen storage targets.
- Good progress in some areas. However, in areas such as alanes, the regeneration should take a higher priority.
- There has been good progress and understanding, as it should be for a project of this magnitude. The work on the amides was excellent; unfortunately the fine results are not very promising from a DOE target point of view: NH_3 problem, low temperature kinetics, cyclic life, etc. Similarly, project scientists are to be commended in recognizing the limits to the alanates. The new synthesis techniques being developed are welcome. Theoretical methodology has been refined and improved.
- Noteworthy SNL accomplishments include the Monte Carlo approach which is innovative and can help predict possible new storage materials. SNL uses good technique by developing a theory, using experimentation to evaluate the theory and then revisiting theory based on the experimental results. On the other hand, some work is likely to have little value; e.g., the solid state synthesis of Ca-B-H has very low kinetics and reversibility.
- Minimal improvements were demonstrated this year. Good decision to reduce work on amides. Hopefully this CoE hasn't hit the ceiling of what can be accomplished. Has thermodynamics destabilization reached the limit? Slide 17 demonstrates cycling issues and revealed that NH_3 is released in the $LiNH_2/MgH_2$ mix. This was one of the few presentations that demonstrated extensive cycling tests.
- Limited scope of work and small subset of hydride materials were studied relative to funding level.
- Current materials do not appear promising.
- New theoretical tool developed, but not clear if it has the capability to identify promising materials with reasonable certainty.

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

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

- Well coordinated inside the program, but also well attached to the greater scientific community in this area.
- Tightly organized, multi-institutional program that is performing a broad based study of hydrogen storage in metal [hydrides]. The integration of contributions appears to be well managed, as reflected in the one hour CoE presentation and supporting presentations from partnering institutions. The MHCoE has established a Coordinating Council.
- The CoE has wealth of experienced partners. It is not clear how the these interactions are nurtured.
- Many good CoE collaborations, at least on paper, via the CoE. But it is not quite clear from the presentation how they provide synergism and specific contribution to the SNL activities described here. It is hard to see how the collaborations are working from organization, communication and group kinetics.
- The CoE has assembled a world class group of partners to work toward the goals of hydrogen storage.
- Obviously the best researchers in the country are integrated into this CoE.
- Modeling and experimental work do not appear to interact or connect with CoE partners or to other researchers.
- CoE structured well for interactions, collaborations.
- Collaborations appear to be limited to characterization of materials.

Question 5: Approach to and relevance of proposed future research

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

- Appropriate plans, addressing the key short comings of the materials they study.
- The future plans do build in a logical way from what is being learned program wide. The flexibility to redirect and refocus has been demonstrated even in the first full year. Removing/eliminating barriers might take a while for this program. Knowing when enough effort has been expended to justify abandoning specific materials approaches is going to be important for the managers of this CoE.
- I agree on the plans for the future: eliminate alanate work, minimize amide work and focus on the search for new materials, including borohydrides. Off ramps have been effectively used in their last year and seem to be adequately factored into future plans.
- There is so much to learn about metal hydrides and so many possible storage materials to evaluate that future plans are reasonable, build on past research and may very well have some chance of success meeting the program goals.
- Interesting that work on amides is being reduced and borates are increasing. Work on alanes continues. Will there be an overlap with the chemical CoE in the future? The CoE has made some important no-go decisions on alanates and amides which helps them to move on to other more promising materials. Rapid thermal processing technique should prove valuable.
- New materials effort seems limited.
- Good decisions on mid-course directional changes.
- New rapid screening method proposed.
- Where does Intematix fit in to CoE work?

Strengths and weaknesses

Strengths

- Quality of team, management approach, potential storage levels, willingness to leave non-productive systems, interaction between theory and experiment and experiment and engineering.
- The MHCoE is off to a good start. It appears to be well managed and well integrated in terms of partner connection.
- The CoE has some of the most experienced and knowledgeable partners in the field. The barriers are understood. Good progress in some areas.
- A diverse spectrum of scientists and engineers…and excellent laboratory facilities. Long experience in H-storage materials.
- Strong partners doing valuable research. Monte Carlo work innovative and important to be able to predict possible new materials.
- Strong researchers and powerful fast throughput systems will be very helpful in stepping up the pace of new materials discovery.

- Great collaboration with various academic, governmental and industrial institutions. Shifting effort away from alanates and amides towards other systems.

Weaknesses

- Some team members are not as cooperative as would be desirable it seems. Seems like teams are not talking to each other. Kinetics and temperature are still major questions with little progress from last year. Obviously progress can not be made every year in the key challenges, but sooner or later progress is required.
- The storage of hydrogen in metals has been studied for years. In spite of the very large number of new mixed metal compositions for which hydrogen storage data are now available (from the MHCoE and other HFCIT contractors), there is little encouraging evidence that any metal hydride can achieve the 2015 DOE system goal of 9 wt.% H.
- The presentation did not clearly address the interactions of the partners and how the collaborations among the partners results in a more synergistic approach.
- It seems SNL is leading the CoE in ways DOE wants, but it is not quite clear it is making the fullest use of CoE collaborations for its own optimization of time and resources. I sense there is some tendency toward working in a vacuum.
- There appear to be some communications challenges to ensure that work being done among the partners is well coordinated and that possible overlap is minimized.
- Fighting thermodynamics- have they hit the limit? Still little discussion or presentation of other important aspects of storage such as material heat capacity, thermal conductivity and diffusivity which all greatly affect system design.
- Lack of truly innovative concepts for on-board hydrogen storage in the area of metal hydrides. Delaying no-go decision on amides and alanates. Useable capacities of materials are not considered.
- Need more emphasis on experimental materials efforts.
- Need more interactions and partnerships with CoE partners and external partners.

Specific recommendations and additions or deletions to the work scope

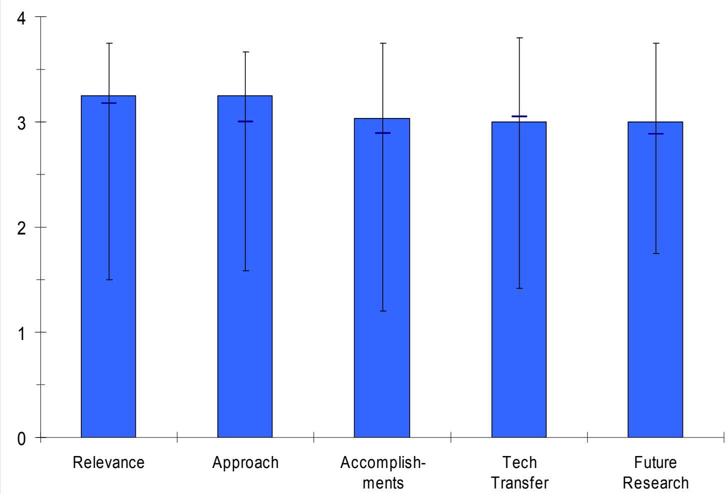
- Should focus on why materials predicted to be good did not live up to expectation $Ca(BH_4)_2$ for example, and try to improve model prediction, or improve experimental speed to working system. Highly unlikely this will recycle at 350°C based on thermodynamic or practical considerations; they should know better than thinking at 100°C and 100 bar above the desorption they could get good reversal. Go to theory and find the right conditions see slide 39 for correct conditions. I would recommend management reinstall a sense of urgency in the CoE partners, while hard to pin down it seems like the partners' focus and speed has become more diffuse in the last half year, and if so, that will hurt the overall program.
- The above stated perception about the value of studying metals for hydrogen storage is shared by many veterans of metal hydride science and it is incumbent on the MHCoE to provide in the not too distant future some definitive evidence that metal mixtures of any composition are worthy of study from the standpoint of DOE's goals for hydrogen storage. One option for dealing with this issue is to make a well considered case that the present DOE target values (particularly for 2015) are unnecessarily high.
- Perhaps go more than "rescue" amide work; maybe probably zero it out. It is suggested SNL start considering the economics of the new systems and synthesis methods being studied.
- Projects should clearly move past alanate research.
- Generate more "outside-of-box" concepts as opposed to focusing on further development of already known systems. Consider "useable" capacities of materials as opposed to considering measured wt.% capacities. Would be good to present 3 parameters for each material, that is measured/predicted wt. capacity, delta H of desorption and operating temperature. Develop strategies (systematic approaches) for materials discovery as opposed to rather empirical process based on "prior art."

Project # ST-14: Lightweight Intermetallics for Hydrogen Storage

J.C. Zhao; General Electric, Inc.

[Partner of the DOE Metal Hydride Center of Excellence]

Overall Project Score: 3.1 (6 Reviews Received)



Brief Summary of Project

The objective of this project is to discover and develop a high capacity (> 6 wt.%) lightweight hydride capable of meeting or exceeding the 2010 DOE/FreedomCAR targets. Specific objectives for FY 2006 include identifying the crystal structures of $Mg(BH_4)_2$ using XRD, neutron diffraction and computer modeling, and performing combinatorial and computational screening of catalysts and dopants for $Mg(BH_4)_2$ to try to make it reversible on-board a vehicle.

Question 1: Relevance to overall DOE objectives

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

- This project is part of the MHCoE. The focus is on combinatorial testing of candidate metal hydride storage materials and on coordinated studies to identify new candidates and elucidate reaction pathways.
- Project is properly focused on national needs to solve on-board storage problem vis-à-vis the President's Hydrogen Fuel Initiative and DOE RD&D plan. Volumetric targets not addressed much.
- Aimed at key problem of the material and material has potential to approach 2010 goal.
- Material research aligns well to the President's Initiative.

Question 2: Approach to performing the research and development

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

- Approach is efficient and precise, aimed well at important goals.
- Focusing on one material, magnesium borohydride, could be beneficial in not having the distraction of pursuing multiple materials at the same time.
- The high throughput approach seems like a good one, although novel promising materials were not really found. In going forward, with the focus on $Mg(BH_4)_2$, it is not clear whether this is going to utilize the high-throughput approach, or a more directed focus on one specific material.
- Although combinatorial techniques are central to other DOE projects, this project provides some clearly different synthesis techniques that are likely more amenable to commercial scale-up. PI's organization is a very well-known facility based in practical materials science.
- Reported successful development and application of a robust, combinatorial measurement system. Strongly focused on $Mg(BH_4)_2$ and related metal hydride systems; could in principle be more broadly applied within the MHCoE program. In situ XRD and neutron diffraction are providing meaningful structural and mechanistic information.

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

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

- Combinatorial work on LiAlSi blends useful, if not advancing the storage limits known. $Li_6Mg(NH)_4$ system a good development. $Mg(BH_4)_2$is an interesting development too.

- Combinatorial/high throughput screening methodology appears to be valuable for material selection.
- Though nothing promising was found in the light-metal systems, or the amide/imide systems, there still was good technical progress, and it's good to see that this group is willing to move on to new areas if they don't see promise. The synthesis and decomposition of $Mg(BH_4)_2$ is a promising avenue for this group to explore further.
- A good start has been demonstrated, perhaps slightly below expectations for the relatively high funding for this project. Many preliminary results are negative, but those are important results anyway. It is good to see contractor knows when to quit. "Amide" results, like others, again show the NH_3 problem and strongly suggest such work be generally deemphasized.
- Screened more than 10 ternary systems. Found that vacancy ordering influences structure. Working on $Mg(BH_4)_2$ with 16 wt.% H theoretical capacity. Several new compositions identified for further study. But, still no demonstrated breakthrough composition that can meet the FY 2006 goals for storage capacity and delivery.

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

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

- Part of the MHCoE.
- Collaborations dubious with SNL or other places.
- Doesn't seem that the communication between GE and Sandia is terribly good. For instance, the Sandia presentation proposed future work on the Na-Si-H system, but immediately after, GE showed a slide saying that the Li-Na-Si system had been thoroughly screened, and they had given up on it. How does the GE work on $Mg(BH_4)_2$ fit in with the Sandia work on borohydrides? Is there redundancy here?
- Good collaborations, at least on paper, within the CoE. No CoE or outside industrial collaborations.
- Solidly integrated into the MHCoE. It seems this project is getting samples from most synthesis sub tasks of the overall CoE.

Question 5: Approach to and relevance of proposed future research

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

- Appropriate plans.
- Focusing on magnesium borohydride dopant predictions can be valuable to the entire MHCoE.
- Strong focus on $Mg(BH_4)_2$ seems like a good idea; need to ensure that Sandia (also focusing on borohydrides) is not going to duplicate this effort!
- I must question the strong emphasis on $Mg(BH_4)_2$ for the future effort, given the high desorption temperatures seen thus far and the doubts on reversibility. Very good to try catalyzing the reaction, but don't wait too long on the go, no-go decision.
- Future plans build sensibly on findings to date within the project and within the CoE as a whole. Removing/diminishing barriers will be a challenge for the entire CoE.
- Scope of proposed research for 2006/2007 is too limited to Mg boron hydride. Need more innovative ideas for future plans.

Strengths and weaknesses

Strengths
- Willing to end work that will not meet goals.
- Project appears focused. Very good collaboration.
- A good materials science lab with long experience on combinatorial experience.
- The main strengths are an experienced staff, thoughtfully developed equipment, and intelligent application of computational tools.
- Good experimental design including in situ structural characterization of hydride material (XRD) and detailed gas phase analysis. Prompt assessments of results and timely go/no-go decisions on aluminides, silicides and amides.

Weaknesses

- Duplicative to Sandia work.
- High temperature for reversibility may be problematic for this material.
- Possible overemphasis on $Mg(BH_4)_2$ as a solution. A “one card” gamble.
- No mentionable weaknesses were noted.
- Again, useable capacity issue. For majority of known metal hydrides (including destabilized) the useable capacities range from 2.5-2.9%. Not clear whether the proposed systems are/will be outside of this box. Thermodynamic aspects (endothermic heat effects of hydrogen desorption) are not considered. Also, the project is heavily focused on magnesium boron hydride that reduces probability of success.

Specific recommendations and additions or deletions to the work scope

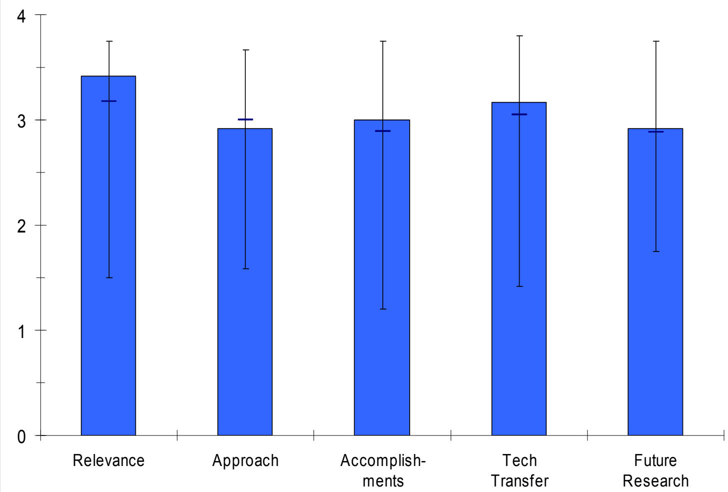
- Need to split up work between SNL and GE and work together.
- Continue searching for "new and different" materials as much as possible.
- I must question the strong emphasis on $Mg(BH_4)_2$ for the future effort, given the high desorption temperatures seen thus far and the doubts on reversibility. Very good to try catalyzing the reaction, but don't wait too long on the go, no-go decision.
- The plot on slide 6 of the presentation (also presented in other places in the talk) tells a story about metal hydrides and the DOE storage targets; this information needs to be reconciled. All the materials represented in the slide fall on a line that lies considerably below the pathway to meeting DOE targets. That's happening for a reason and it's telling us something that needs to sink in, i.e., metal hydrides may be classically limited in what can be expected from a hydrogen storage perspective.
- Estimate storage energy efficiencies of the materials by considering enthalpies of desorption (useable capacities). Generate more approaches beyond Mg boron hydrides.

Project # ST-15: Synthesis & Characterization of Alanes for Automotive Applications
Jason Graetz (co-PI) presenting; Jim Wegrzyn (co-PI); Brookhaven National Laboratory (BNL)
[Partner of the DOE Metal Hydride Center of Excellence]

Brief Summary of Project

The objective of this project is to understand the strengths/weaknesses of using AlH_3 as a storage medium by quantifying the reaction kinetics, thermodynamics, and energy requirements for regeneration. This will be done by synthesizing 3 polymorphs of AlH_3 (α, β and γ) with material capacities ≥ 8 wt.% (gravimetric) and ≥ 0.10 kg-H_2/L (volumetric). AlH_3 polymorphs with suitable H_2 pressures at temperatures near the operating temperature of a PEM fuel cell (~85°C) will be identified and it will be determined if AlH_3 can be formed by direct high-pressure hydrogenation of Al powder at pressures <103 bar (Go/No-Go decision in FY06).

Overall Project Score: 3.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Important and novel work aligned well with targets.
- In line with program objectives. Work is of relevance to attainment of R&D goals.
- On-board storage in solid structures is perhaps the single most important need for the development of the hydrogen economy. The focus on alane is one of the more promising pathways.
- Fully supports the multi-year RD&D plan.

Question 2: Approach to performing the research and development

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

- Focused on key technical barriers.
- Reasonable, systematic approach to the objectives of the project. The link to and integration with the MHCoE is instrumental for covering scientific and technical barriers. Project profits from access to unique materials characterization facilities. The system engineering part could be more ambitious.
- Sound approach from theory and synthesis to experiments to tanks.
- The study of polymorphs leads to understanding the decomposition characteristics of AlH_3, and seems to show how the material must be synthesized and how it must be treated. The presenter, however, was not clear in his presentation as to how the accumulated data would be used. The polymorphic nature of the material, with no clarity on what parameters will determine which polymorph of alane will form, leads me to be concerned about the control that one might have over this material if on-board a vehicle, especially how the temperature would be controlled.
- Very disappointing to see this work focused so strongly on decomposition. Regeneration is really the key obstacle, but is a much harder problem to solve. Should have been focused on this from the beginning.

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

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

- Good progress with respect to objectives. The volumetric H capacity for the aluminum hydride really stands out - three times the DOE system target! The direct hydrogenation of spent Al powder requires extremely high hydrogen pressures placing a doubt to the applicability of the material; the 'No-Go' decision was correctly taken. System target/value and not just materials should always be kept in mind - may affect the project's progress and deliverable.
- The presenter showed very definitive properties for the polymorphs of alane. This work seemed to be very well thought out and performed. It is not clear, from the presentation, however as to how these data will be used.
- The team made interesting progress on understanding polymorphs and decomposition. However, I think this is the wrong problem to focus on. They need to concentrate on regeneration. The consideration of rehydriding with pressure seemed really like a waste of time. This team should have known at the outset that a 9 kJ reaction is simply not going to be rehydrided with modest pressure. Why was this even part of the project?
- Quoting formation energetics rather than the more conventional decomposition reactions made it appear as though all the signs of delta-H and delta-G were incorrect. This was a bit distracting. Decomposition of AlH_3 is weakly endothermic.
- Direct decomposition is a valuable finding, showing the need for phase purity.
- In the case of solving the regeneration problems the systems can be the most efficiency.

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

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

- Good coordination inside CoE, appropriate outside connections.
- Incorporation into MHCoE is beneficial so is the foreseen collaboration through IPHE, however link to industry was not clear.
- The presence of the CoE provides a natural collaborative mechanism. Another reviewer (or listener) remarked on the fact that due to the regeneration issues, the alane projects should be housed in the chemical hydride CoE rather than in the metal hydride CoE. I do not agree; there is a large alane body of researchers in the [Metal hydride] CoE. The regeneration is one issue - albeit an important one.
- Would be good to expend the collaboration with IPHE countries research centers.

Question 5: Approach to and relevance of proposed future research

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

- The future work needs to focus on regeneration. More work on kinetics and decomposition is scientifically interesting, and will certainly need to be done if this material is even to make a viable storage technology. However, if there is no pathway to regenerate it efficiently, the work on kinetics will be wasted effort. Regeneration strategies don't look very promising - Yes, delta-G appears in an exponential, but it needs to be reduced by quite a bit (e.g. two orders of magnitude away from where it needs to be?). Is there really a possibility to change delta-G by this much via the approaches mentioned to rehydride with pressure?
- Plan includes subscale tank development and theory work, regeneration work, which is commendable in a subproject. Regeneration is especially important.
- Future research plans appear reasonable however the plans for the full system approach could be more ambitious.
- While the work being proposed on regeneration of the alane is very important, there was no indication as to how the work on the polymorphs will be used in any future work.
- The most important problem - regeneration is proposed for future work.

Strengths and weaknesses

Strengths

- Amount of hydrogen stored.
- The incorporation in the MHCoE strengthens their research and expands their vision of materials and possible systems. Access to unique characterization facilities, expertise, technical resources and instrumentation. The IPHE collaboration scheme is beneficial.
- A major strength is the need for this type of research. Another is the presence of the CoE.
- High scientific level of experimental and technological approach.
- Good and quick no-go decision regarding synthesis of AlH_3 from elements. Considerations of the thermodynamic aspects of hydrogen desorption. Strong experimental basis at BNL. Planned regenerability studies for AlH_3.

Weaknesses

- Material must be removed and put back in tank.
- Engineering aspects are an issue. The progress towards the system, as opposed to material, targets should be addressed, to avoid confusion.
- The work seems somehow disjointed. It was difficult in the presentation to determine what the results meant in the real world. How will an alane system be designed, for instance, that will avoid the temperature instabilities of some of the alane polymorphs. Another issue is, of course, regeneration. An off-board regeneration system is going to be much more expensive.
- Planned scale-up study (1 kg tank) appears irrelevant on this stage of the project. Addition of NaH or LiH would bring the system to well-known alanates. Decomposition of AlH_3 at > 40ºC is of great safety concern since equilibrium pressure for AlH_3 is 10^5 bar at ambient temperature.
- AlH_3 has already been studied in quite some detail from DFT approaches (structure, thermodynamics, etc.). Are the proposed modeling efforts really going to add value beyond what has been done already?
- Not substantial.

Specific recommendations and additions or deletions to the work scope

- Probably need a focus on the tank refill for offboard system. Need to work on regeneration process and push down the cost. This is critical!
- This is a promising material with the potential to reach the targets, however regeneration and associated costs are still problematic and should be intensively addressed. Increasing effort and speeding up progress on first principle analyses, using input coming from crystal structures collaborative work, could benefit the project. Also interaction with the chemical hydrogen storage CoE - link with the ionic liquids work - could be considered, for the regeneration off-board.
- Need to look at the ramifications of the developed data as stated above.
- Expand collaboration with IPHE country research centers and universities.
- Eliminate scale-up studies for alane-based tank. Address potential pressure build-up issue for AlH_3. Develop alternative approaches in case a no-go decision regarding AlH_3 would have to be made.

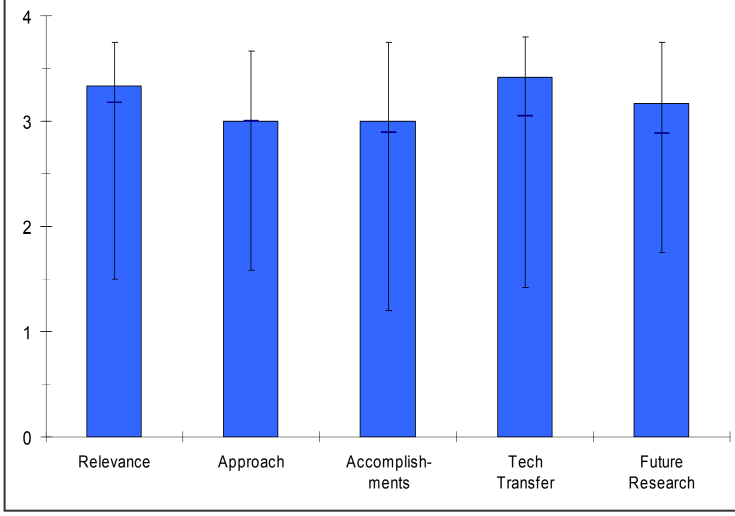
Project # ST-16: Thermodynamically Tuned Nanophase Materials for Reversible Hydrogen Storage

Greg Olson; HRL Laboratories LLC

[Partner of the DOE Metal Hydride Center of Excellence]

<u>**Brief Summary of Project**</u>

Overall Project Score: 3.1 (6 Reviews Received)



The overall objective of this project is to develop and demonstrate a safe and cost-effective light-metal hydride material system that meets or exceeds the DOE goals for reversible on-board hydrogen storage. Specific objectives include:

1. Identify and test new high capacity Li- and Mg-based destabilized hydrides
    a. Screen candidate $LiBH_4$ + MgX destabilized systems and evaluate energetics and kinetics
    b. Down-select systems for additional work
2. Apply nano-engineering methods to address kinetics limitations
    a. Develop solid state approaches for efficient synthesis of nanoscale reactants
    b. Assess hydrogen exchange rates in nanoscale MgH_2/Si and destabilized complex hydrides
    c. Evaluate sorption kinetics of reversible metal hydrides in nanoporous scaffold hosts

<u>**Question 1: Relevance to overall DOE objectives**</u>

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

- This project is part of the MHCoE. A nano engineering approach is being taken to the study of hydrogen release through destabilization. Also looking at ways to minimize microstructural dimensions to enhance diffusion.
- The focus on the thermodynamic destabilization of the high capacity material $LiBH_4$-MgH_2 is very good because it may have the potential to meet the DOE 2010 capacity goals.
- Addresses key problems (capacity, thermodynamics, and kinetics) in a planned way, and allows high capacity.
- Contractor focuses completely on storage problem with full attention to DOE targets necessary to realize RD&D objectives and HFI.

<u>**Question 2: Approach to performing the research and development**</u>

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

- Studying destabilization through addition of alloying elements. Basically, they are manipulating the activity of the strong hydride former to lower binding energies (an old, well known approach). Also, applying dilution milling methods, porous scaffold structures, and aerogel matrixing to shorten diffusion paths or more correctly increase surface area to volume ratios.
- The two-fold approach of thermodynamic destabilization and nano-size kinetic effects to lower hydrogenation and dehydrogenation temperatures is excellent, particularly for the materials approaches based on $LiBH_4$. The intended study of scaffold effects on the $LiBH_4$-MgH_2 system performance is excellent.
- Approach is quite good. More efficient way to address delta H and look for more suitable high capacity materials. Then attack remaining barrier of kinetics.

- The approach has been pioneering and has offered a way out of the thermodynamic dilemma that restricts the light metal hydride thermodynamics. The approach is catching on and has been emulated both within and outside of the DOE program (a true measure of its conceptual success).

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

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

- Working mainly on Li- and Mg-based hydrides. They report having characterized 12 reactions. Getting 5 to 10 wt.% H capacity. New light shed on loss of reversibility in $LiBH_4$. Dilution milling helps but doesn't extrapolate. Some results coming in for the porous scaffolds and the aerogels.
- The results obtained with the MgH_2-Si system, although not very encouraging, are comprehensive and should be sufficient to make a reasonable go/no-go decision in September 2006. The results of the scaffold approach lowering the reaction temperature of $LiBH_4$ are interesting, albeit there are hydrogen capacity penalties due to the presence of the scaffold. They have set the stage for further scaffold work on the $LiBH_4$-MgH_2 system.
- Studied a significant number of new materials predicted to be good. Looked at morphology effect. Scaffold - aerogel work is a nice alternate to the homogenous catalyst route.
- Progress has been very good and the effort pursued with unusual logic and well-contemplated direction. Good, frank discussion of remaining problems, e.g., MgH_2-Si system. The problems remain with kinetics, but the nanoparticle and scaffold approach has made good progress. It certainly remains to be seen if desorption temperatures low enough for fuel cell or ICE vehicles can be practically achieved.

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

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

- Well integrated into MHCoE. Part of "destabilization sub team."
- Technology transfer/collaborations appear excellent. This project is a part of the metal hydride CoE.
- Good inside collaboration in the CoE. Also good attachment to industry and outside academia.
- Good collaborations within the CoE, with partners' roles clearly defined.
- Good collaboration across the entire MHCoE.

Question 5: Approach to and relevance of proposed future research

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

- Future plans build sensibly on findings to date within the project and within the CoE as a whole. Removing/diminishing barriers will be a challenge for the entire CoE.
- The proposed study of Li-Si-N systems is good. The proposed study of scaffold effects on the kinetics of the $LiBH_4$-MgH_2 system is good.
- Suitable based on problems identified and goals.
- The work plan is clear and logical and cannot be improved much. The search for new systems is especially important. Coordinator of the destabilized hydride group of the MHCoE.

Strengths and weaknesses

Strengths

- The presenter (presumably the PI for this task) seems to be keenly aware of the issues he is addressing and the limitations of the approaches used in his project. The presentation was delivered in a very clear concise manner. Some conceptually interesting thinking is going into this project.
- Very sound approach of combining thermodynamic destabilization and nano-size kinetics to lower the reaction temperature of high capacity hydrogen storage materials.
- Concept power, capacity possible.
- A powerful new thermodynamic approach, combined with highly capable technical people. Excellent track record so far.

- Excellent focus on scaffolding of $LiBH_4$ to improve on kinetics.
- Excellent job of leading sub-team on destabilized hydrides.

Weaknesses

- There are no serious weaknesses to this project. The chances of a successful outcome will rest on getting lucky with some alloyed composition and/or microstructural embodiment that happens to exhibit an uncharacteristically rapid release of hydrogen at a temperature below say 200°C, yet still stores a large enough quantity of hydrogen to meet DOE targets for 2010 and eventually 2015. “A tall order.”
- Kinetics.
- Group has a tough problem with kinetics. "Nano" is an overworked term and will be a difficult solution to economically and permanently apply.
- Destabilization is a trade-off. Introducing destabilizing elements will reduce gravimetric capacity as well as reduce delta H. Can useable capacity be higher than 2.5-2.9%, i.e., outside of the common "box", e.g. MgH_2 as reference. (Useable capacity is defined as a difference between measured capacity and hydrogen equivalent of energy requirement to compensate for endothermic heat effect upon hydrogen release from the material.)
- None.

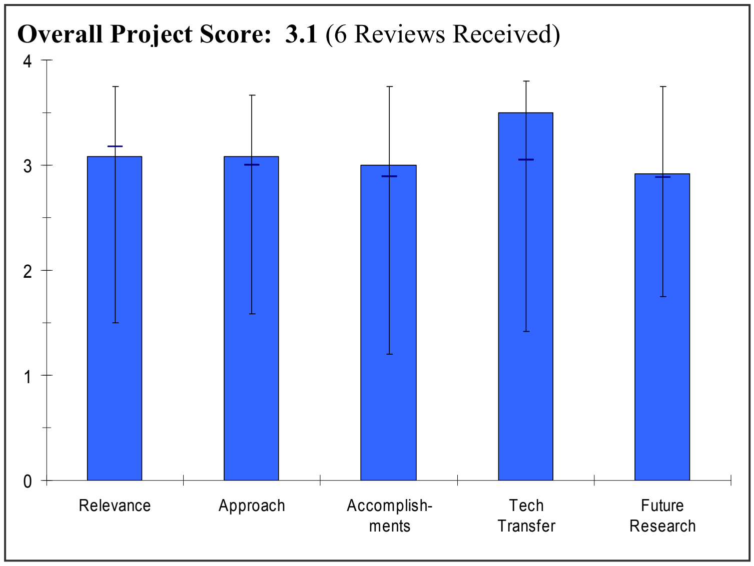
Specific recommendations and additions or deletions to the work scope

- The "destabilization" plot in slide 7 of the presentation might lead some people to believe that compositions with lines headed towards the DOE target boxes will actually get there at low enough temperatures. This is a clever but also deceptive way to indicate we might be headed in the right direction.
- None.
- Scaffold types may help. If it would be possible, the $LiBH_4/MgH_2$ system should be tried in the aerogels.
- Even before moving on to a prototype system, some preliminary cost estimates of this approach should be made. Should also add some preliminary safety studies.
- Introduce useable capacity as a parameter for materials selection.

Project # ST-17: Development and Evaluation of Advanced Hydride Systems for Reversible Hydrogen Storage

Bob Bowman (PI); Jet Propulsion Laboratory

[Partner of the DOE Metal Hydride Center of Excellence]



Brief Summary of Project

The overall objectives of this project are:

1. Validation of initial storage properties and reversibility in light element metal hydrides and assess their aging durability during extended cycling
    a. Nanophase, destabilized hydrides based upon LiH, MgH_2, & $LiBH_4$ produced at HRL, Caltech, & other MHCoE partners.
    b. Complex hydrides (e.g., amides/imides, borohydrides, & AlH_3-hydrides) provided by SNL, NIST, BNL & other MHCoE partners.
2. Support developing lighter weight and thermally efficient hydride storage vessels and experimentally demonstrate their compatibility with appropriate complex and destabilized nanophase hydrides.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project gives an independent validation of the MHCoE materials. The focus on life testing is an important development aspect.
- Good systems to study to help the goals of the program.
- PI fully addresses H-initiative and RD&D plan in terms of real targets.

Question 2: Approach to performing the research and development

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

- A wide collection of topics with other researchers (HRL destabilized hydrides, BNL AlH_3, amide work, etc.). Mostly looks like they're doing NMR studies of materials for researchers. Is this "service facility" really the model for this project? NMR studies are certainly important in this area, but they seem like a bit of a waste considering the PI's talents. From the approach slides, they seem to really be focused on durability, hydride bed designs and lifetime testing. But, the results slides don't show this focus.
- The project is well structured and reasonably integrated with the activities of the MHCoE. The PI has clearly described the role and the contribution of JPL. The timing of the contribution in the decision phase requires better description.
- Both basic, applied and engineering studies are a good approach. Systems have right capacities.
- Project is well designed but task 5 (engineering analysis should be extended).
- This project provides very valuable support services to all project groups of the MHCoE. They are important contributions: (a) validation of PCT properties, (b) cyclic testing, (c) NMR, and (d) hydride tank design. The PI is especially capable in all areas and is serving a valuable role to the entire CoE.

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

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

- Lots of interesting NMR work, but it still looks like bits and pieces, and isn't really clear what the overall goal is. Focus doesn't seem to be really on the most promising materials (LiH/Ge, $NaAlH_4$, etc.).
- The project shows a very good progress according to the objectives.
- Generated information on phases and changes in bonding for other CoE partners. Germanium work seems of no point.
- Would be good to include in the task 5 the investigation of heat and mass transfer and impurities role in reactors.
- Given the recent start and relatively low funding (to date) for this project, admirable progress has been made in a short time. Key inputs of new data have been added to all project areas of the CoE. NMR results impressive in such a short time.
- Progress towards Sept. 2006 on defining top-level parameters for a storage vessel has not been demonstrated.

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

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

- The project has a good integration in the MHCoE with important collaborations.
- High quality partners inside and outside the CoE.
- Very comfortable and thorough collaborations with most of the CoE members. PI is well known to the outside world and long-experienced in scientific collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Plan forward in the future is unclear. What is the direction and mission for this project? How does it fit into the CoE? Didn't see much mention of durability, hydride bed design, etc., in the future plans.
- The future work is well presented and technically feasible, because the presentation of achieved results demonstrates competence and adequate resources. Life testing and system studies require a better description of technical resources and test facilities.
- The most important problems: alanes and engineering analysis and design would be good to expand the investigation in that directions.
- Good. Continue as planned.

Strengths and weaknesses

Strengths
- The described results and the work plan show adequate resources and technical personnel to carry out the characterization work.
- Well designed project, well developed cooperation.
- PI's long experience, capability and reputation. Provides critical NMR contributions to the entire CoE.
- To provide analytical and testing support to all CoE groups. Future development of storage vessels conceptual designs based on JPL's experience.

Weaknesses
- JPL should be doing something other than just supporting others with NMR work.
- Not substantial.
- None obvious.
- Presentation was overloaded with spectroscopic data for various systems studied without successful delivery of messages on what the conclusions were. Project only provides support to other MHCoE members on characterization and stability testing. Selection of materials for testing and characterization is questionable (e.g., Mg-Li-N-H).

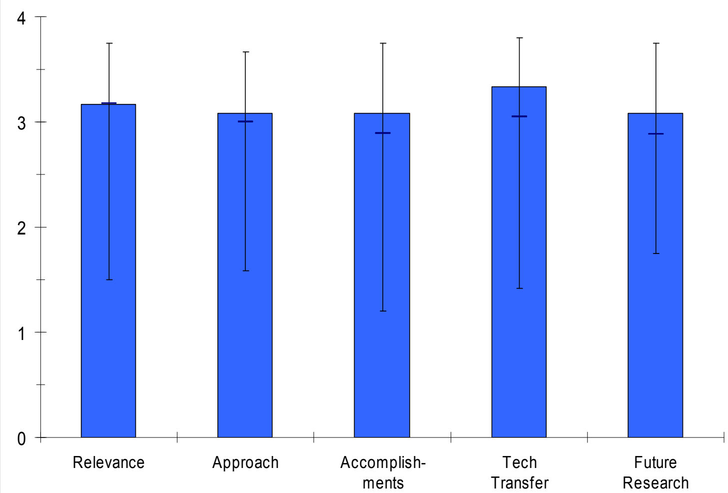
Specific recommendations and additions or deletions to the work scope

- Would be good to extend the task 5 (engineering analysis + design).
- The presentation contains an evident mismatching between the Task and the planning: Task 5 is not included in future steps. The life testing work must be presented, because there is a preliminary milestone in August 2006. Details on the life testing methodology must be indicated and discussed.
- Most of what was shown was data, but there was not a great deal of interpretation as to what this meant for advancing hydrogen storage. It would be highly desirable to tie this a route to progress.
- To add in task 5 the research of thermal problem, impurity problem and storage vessel scale effects.
- Continue to provide exactly this sort of support to all CoE project groups.

Project # ST-18: Metal Hydride-Based Hydrogen Storage

Ian Robertson; University of Illinois Urbana-Champaign
[Partner of the DOE Metal Hydride Center of Excellence]

Overall Project Score: 3.1 (6 Reviews Received)



Brief Summary of Project

The overall objectives of this project are to support and guide development of complex metal hydrides to meet systems requirements by providing CoE partners with structural and chemical insight of candidate systems and providing experimentally based and validated theoretical modeling. Specific objectives in FY 2006 are to determine degradation during transfer to analytical instruments and conduct structural and chemical analysis of systems of interest to partners, and to develop a structural database for information sharing with partners. The approach combines use of advanced characterization capabilities with first principles electronic and thermodynamic calculations.

Question 1: Relevance to overall DOE objectives

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

- Relevance is good in experiment and theory, working on problems of importance.
- The Hydrogen Fuel Initiative and RD&D plan are not directly addressed. By service association with the CoE, it can be reasonably assumed that these objectives are reasonably incorporated in this work.
- This project is part of the MHCoE. Electron microscopy methods are being used to examine the micro- and nano-scale structure of hydrogen storage materials. Also, exploring how defects and contaminants affect hydrogen uptake and release kinetics. Companion theoretical studies are examining the energetics of hydrogen uptake and released. A much needed capability within the MHCoE.
- Project only provides support on materials fundamental understanding and modeling to the key CoE participants.

Question 2: Approach to performing the research and development

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

- Theory approach is potentially complemented by experiment. But [there is] not a clear plan to improve hydrogen cycling at moderate temperature. This work may help plan better materials or improve current ones, but it is not clearly planned to happen.
- As best I can tell, this project seems to be a service to the CoE for experimental and theoretical skills that are not otherwise available. But it not clear how some of the work differs from others in the CoE and other DOE projects, e.g., VASP thermo calculations at UT [United Technologies], Ti catalyst studies. But I accept that there must be unexplained differences. How does the modus operandi function? Does the contractor get requests from the other CoE members each meeting? Or did the contractor pick general areas of support early in the CoE to continue for the duration.
- Well-designed and planned project. Possible segregation of components in charge - discharge operation should be investigated.

- Transmission electron microscopy (TEM) coupled with energy dispersive spectroscopy and electron energy loss spectroscopy (EELS) is being used to interrogate the fate of hydrogen storage materials. Paralleling electronic structure calculations provide information that can be correlated with the EELS results and with storage material performance (e.g., kinetics).

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

This project was rated **3.1** based on accomplishments.

- Made catalog of signs [signals?] of materials sullied by air or e beams. Matching known TP data in a meaningful system with calculation is a good accomplishment
- The project has barely started (15%), but there seems to be some progress already made. Good start on structural database, services to UH [University of Hawaii], HRL, etc.
- The effects of sample contamination by ambient impurities have been evaluated. The kinds of reactive metals that catalyze hydrogen release (e.g., Ti and Nb) tend to oxidize in ambient environments. These results offer important insights about metallic storage material processing and handling limitations/requirements. Beginning to study how contaminants affect kinetics. DFT and Monte Carlo calculations are progressing. Excellent agreement obtained between measurement and calculation. Issues involving how repeated hydridation and surface contamination influence storage material restructuring are being explored.

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

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

- Many partners listed, but not clear how they bring new theoretical power to the CoE.
- There are stated collaborations with several CoE partners. Are there any citable examples of iterative interaction [effective feedback] yet?
- This project is effectively melding into the activities of the MHCoE. Coordination/collaborations were spelled out in the presentation. Results are passed on to other MHCoE member institutions through an on-line data access website.
- Excellent deal of collaboration with MHCoE partners.

Question 5: Approach to and relevance of proposed future research

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

- Plan seems to be help on what ever they are asked to do.
- Rather straightforward. Is that what the CoE partners want at this point in time? What is the mechanism to change the work plan to suit changes in technology and CoE emphasis?
- The plans for the future follow naturally from both the recognizable needs of the entire MHCoE for advanced, high resolution electron microscopy and the results of FY 2006 activities. The DFT/MC calculations and associated modeling can be expected to provide seminal insights into how electronic structure and impurity elements affect structure and performance of metal hydride storage materials.

Strengths and weaknesses

Strengths

- Potentially powerful methods.
- UI seems to have some important specific skills useful to the CoE.
- High Theoretical level, good coordination in MHCoE.
- The electron microscopy PI is a recognized expert in the field of micro- and nano-structure science. This level of electron microscopy should be an integral part of all three hydrogen storage centers of excellence.
- Good spectroscopic evidences provided with respect to validating models and predictions.

Weaknesses

- Seems to have no well defined direction. Theory and experiments should go together, not act separately.
- It is not clear if the CoE has a mechanism to maximize its benefit from UI and how it keeps UI from duplicating other DOE efforts in the same area.
- None.
- This project has no perceptible weaknesses.
- Issues of adequacy of the results on samples obtained in situ in the chamber of spectrometer with those of actual systems operating under reaction conditions.

Specific recommendations and additions or deletions to the work scope

- Develop their own plan to create or predict a higher capacity, good delta H material.
- Not substantial.
- The DOE nanoscience centers will have many tools for exploring materials at the nanometer scale. As these centers come on line and begin accepting users, the CoE managers should explore the available capabilities of the nano centers to take full advantage of what they can offer the HFCIT program.

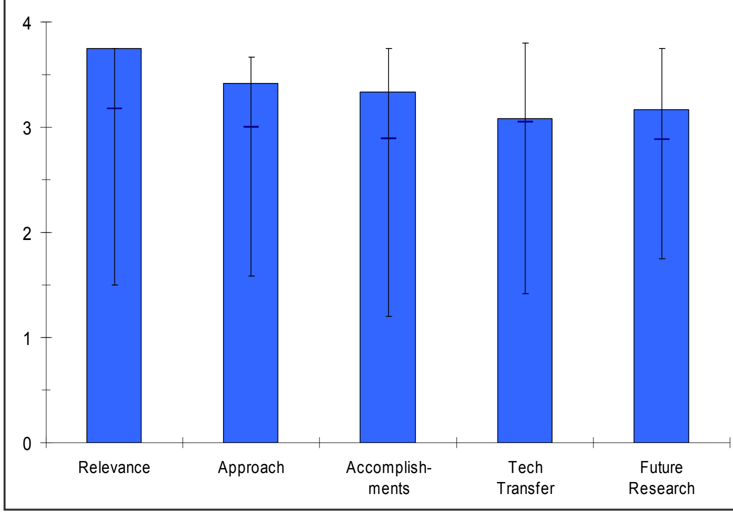
Project # ST-19: System Level Analysis of Hydrogen Storage Options

Rajesh Ahluwalia; Argonne National Laboratory

Brief Summary of Project

The overall objectives of this project are to: 1) Perform independent systems analyses for DOE; 2) Model and analyze various developmental hydrogen storage systems; 3) Analyze hybrid systems that combine features of more than one concept; 4) Develop models that can be used to "reverse-engineer" particular technologies; and 5) Identify interface issues and opportunities, as well as specific needs for technology development. This is being done through the development of thermodynamic and kinetic models of processes in advanced metal hydride, carbon/sorbent, and chemical hydrogen storage systems.

Overall Project Score: 3.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project provides very relevant support, in terms of analysis and assessment, for all of the primary hydrogen storage system candidates.
- Provides an important independent tool for analysis and assessment to the DOE program managers.
- Can provide important insights to system improvements and optimization that involve trade-off of multiple system parameters.
- This is a very valuable tool that can be used by both DOE and the researchers to help determine the pathways for hydrogen storage that are worth pursuing.
- This is particularly important since hydrogen storage is the most critical need for achieving the hydrogen economy.
- High quality analysis vital to guide experimentalists in meeting targets.
- The project provides valuable enabling tools and analyses that support the storage program.
- This kind of modeling is required to determine if adsorbents particularly at 77K could ever be practical for system design. It identifies clearly (within error of some assumptions) what kind of storage capacitates are required to offer a clear advantage over just compressed technologies. It helps to place all the different claims at different operating conditions into some kind of relative order.

Question 2: Approach to performing the research and development

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

- The researcher has identified the key areas of storage which can be analyzed by this method and has developed detailed processes for building the models. It all appears to fit together well. The approach that would be used for the metal hydride storage systems is particularly well developed.
- Good decision on making tool Excel-based. This probably made things harder for developers, but it will make the model useable by almost anyone. Good Job!
- Model shows high degree of rigor and sophistication. Combines ease of use and rigor.
- The approach is very good and intended to provide tools and analyses that DOE will use to evaluate the progress of storage projects.

- The approach includes providing an analytical tool to researchers within the DOE Hydrogen Storage Program.
- Making the tool available to the PIs will be a valuable asset in assisting to make them think of their materials and their affects on system performance. It will certainly put the push on improving volumetric capacity.

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

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

- The results appear to be accurate. However, it is difficult to tell. The presentation did not offer the methodology for obtaining and/or developing input for the models. Low results for the reversible alanate and for activated carbon are expected to be low, so there is no surprise.
- Model for experimentalists is a great contribution. Storage Team needs to see that all materials researchers have access and training.
- Analysis Working Group formation should be a great benefit to program.
- So far, ANL has succeeded in providing the analytical tools and models required. The MH storage system analytical tool is particularly interesting as it considers volumetric efficiency which is not being done elsewhere.
- Analytical capability has been extended to compressed gas/liquid storage, metal hydride and carbon-based storage systems.
- Useful extension of capability to examine regeneration efficiencies/and energy requirements for chemical hydrogen storage systems.
- This year, the tool was made a bit more user friendly and modeling on chemical hydrides was added. The tool still needs to include more information about system fill performance, heat balances, etc and system dormancy rates under various operating conditions.

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

This project was rated **3.1** for technology transfer and collaboration.

- Good contacts with various groups to get data needed for model.
- Storage Analysis Working Group should really help storage analysis efforts.
- Considering the scope and goals of the project ANL is doing a good job of coordinating with others in the field, with DOE and with the centers of excellence.
- Need to develop a closer working relationship with the TIAX work.
- Extensive collaborations have been established with the storage centers of excellence and independent researchers.
- There does not seem to be a single mechanism for coordinating with the projects. Project leaders need to have access to models quickly so they don't waste their time working with materials that won't work. Perhaps the CoEs can act as coordinator.
- More collaboration, to avoid duplication, with TIAX would be useful.
- Need to link with H_2A especially in the area of chemical hydride regeneration.
- Should also work more closely with TIAX to define the system design and UTRC/Ovonics, etc. to better understand system design limitations.
- This tool is especially valuable for the carbon CoE as it stresses the importance of the volumetric capacity of a material and what is required if it is to provide any significant advantage over cryo-compressed gas alone.

Question 5: Approach to and relevance of proposed future research

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

- It appears that the "real" work for utilizing the models will now begin.
- This year's work has set a high standard, but development of the chemical hydride tool will be another great contribution.
- Future work to continue what they are currently doing is a good plan and consistent with the project's goals and with the needs of the hydrogen storage program.

- Future plans to extend and expand the analytical capability appear to be reasonable and appropriate.
- More information is required on the system fill, heat balance and well to wheel efficiencies as this will help determine if adsorption based technologies' potential offers any advantages over metal hydride based technologies.

Strengths and weaknesses

Strengths
- Good approach.
- Delivered quality tool for program and experimentalists.
- Strong analytical modeling skills and experience support the storage program at DOE and the CoEs.
- ANL is doing some innovative work not being done elsewhere.
- Seems to provide good value for the budget being spent.
- Good progress and technical accomplishment.
- An independent, unbiased analytical tool for storage system performance analysis and assessment.
- Simple tool that provides good insight for decision making about which technology has advantages over another and where the optimal or crossover points may lie.
- Appears to be very useful tool(s).
- Addresses a wide gamut of storage methods.

Weaknesses
- None--assuming model is proven.
- System design includes many assumptions and not enough specification based upon real world specs or system performance. Admittedly, there is not much public information available to include in the model.
- Although the model developing methodologies appear to be logical, we don't know enough about the inputs to determine if the analysis tools are really accurate.
- The interfacing mechanism is unclear.
- The results of the analyses is only as good as the assumptions that are input into analysis thus it is important to continue to solicit information on the input parameters from various researchers in the storage program and then take a conservative approach to developing the specific inputs for the analysis tool.

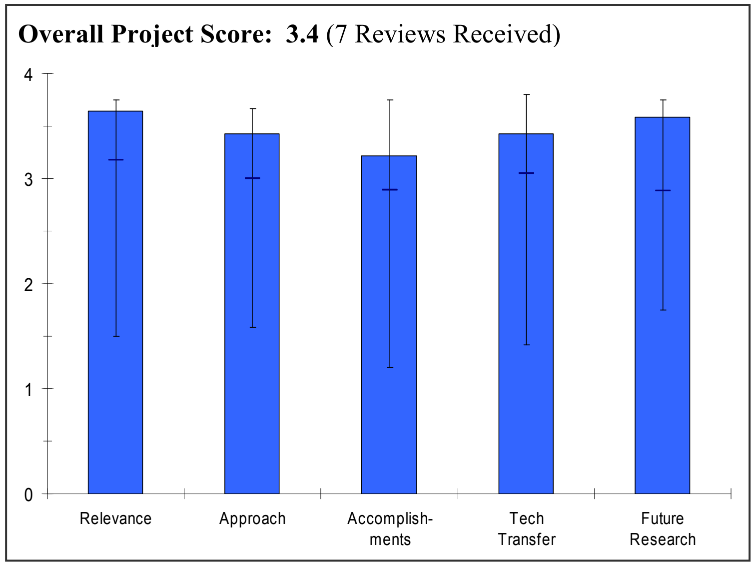
Specific recommendations and additions or deletions to the work scope

- Consider adding sensitivity analysis tabs to tool to give researchers a quick feel for what improvements might have most impact.
- Use feedback from users to improve model--both function and ease-of-use.
- Looking forward to seeing CHtool.
- Is model transparent, i.e., can users see Visual Basic subroutines?
- The MHtool for analyzing hydrogen storage systems needs to be validated against hardware.
- ANL & TIAX should be encouraged to continue close communications regarding analytical models to ensure consistency.
- ANL needs to develop an organic collaborative working relationship with TIAX. These activities are necessary and can be further enhanced by further coordination and collaborative work between ANL and TIAX.
- Please add detailed heat balance and energy balance for fill procedures on sorbent based systems. Please include a thorough dormancy analysis under various driving conditions similar to what Lawrence Livermore National lab presented with Dr. Aceves.
- Develop formal interfacing processes that will streamline analyses on particular candidates.
- Expand collaborations with TIAX.

Project # ST-20: Analyses of Hydrogen Storage Materials and On-Board Systems

Steve Lasher; TIAX LLC

Brief Summary of Project



TIAX is evaluating the projected manufactured cost and performance of four broad categories of on-board hydrogen storage options: baseline (compressed hydrogen), reversible on-board (e.g., metal hydrides), high surface area sorbents (e.g., carbon-based materials), and regenerable off-board (e.g., chemical hydrides) systems. System-level conceptual designs, process models, activities-based cost models, and lifecycle performance/cost predictions are being developed for each system based on developers' on-going research, input from DOE and key stakeholders, in-house experience, and input from material experts. This is an on-going and iterative process so that DOE and its contractors can increasingly focus their efforts on the most promising technology options.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Early estimates of the potential cost of hydrogen storage systems are very important and provide valuable insights to both the researchers and DOE program managers.
- This project is focused on system level evaluations of on-board hydrogen storage units. At the present stage, the validity of the assumptions that have to be made to complete the analysis tends to limit the accuracy and therefore affect the influence of the results on the hydrogen vision.
- The project provides valuable enabling tools and analyses that support the storage program.
- These independent analyses are key, especially if the program is to rationally assess storage claims and relative benefits.
- Sensitivity analyses (tornado plots) are good to identify key areas where R&D can make a difference.
- This project provides a "reality" check on the status of key hydrogen storage systems in relation to the DOE targets. It is important to the assessment of the overall progress towards the storage goals.
- Excellent project.

Question 2: Approach to performing the research and development

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

- The model development process has been established and includes inputs from researchers, developers and stakeholders.
- Sensivity analyses for input variables are very important since many of the parametric inputs are still being experimentally determined and are subject to large variability.
- The systems analysis studies use the best information available on system component performance and knowledge based assumptions vetted by appropriate HFCIT program participants, developers, and stakeholders.
- The work is comprised of technical assessment, cost modeling, and persistent refinement. Single and multi-variable sensitivity analyses methods are employed.
- As results come in from the storage centers of excellence and other relevant projects in the HFCIT Program, the level of detail in the systems analysis studies will increase and the confidence level will go up.

- The approach is very good and intended to provide analyses that DOE will use to evaluate the progress of storage projects.
- Analyses deliver critical parameters to describe storage performance within the larger vehicle system.
- The approach being pursued is excellent and very thorough.

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

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

- Cost and performance systems analysis results were presented in detail for four system types. It was concluded that sodium alanate and sodium borohydride based systems will not meet 2010 performance targets.
- Continued refinement and extension of model to all primary hydrogen storage candidates.
- Interestingly, the systems analysis results and the results reported by developers for the same systems were in good agreement; most probably because the assumptions were the same. Question is--were the assumptions generally correct?
- TIAX has provided the analysis needed to support the storage program.
- Good results.
- The results obtained on the sodium borohydride system are very good.
- Would be good to intensify engineering part of project.

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

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

- Good collaborations to gather information on system inputs and model structure.
- Apparently some collaboration on model development, but the contributions of the collaborators are not apparent.
- The people performing these systems analyses appear to be as thorough as possible in doing their homework. This gives confidence that the results are credible. Mainly, this credibility comes from recognizing that the systems analyzers took great pains to obtain the very best information and insights available for each system they treated. This occurred most probably because of networking and coordination with a broad spectrum of knowledgeable people during the input gathering stage.
- Considering that the main goal of the project is to analyze work being done by others for use by DOE in evaluating the progress of storage projects, the interfaces reported seem to be appropriate.
- Need to develop an organic collaboration with the ANL activities.
- Work with H_2A and other contractors keeps this group knowledgeable about latest work in area.
- This project appears to have excellent interactions with the system developers in obtaining information related to the TIAX analyses. Such interactions are critical to the accuracy of the TIAX analyses.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- Future plans seem reasonable- continual updating of input parameters based latest experimental data is essential.
- The future plans involve wrapping up the reported studies and moving on to yet another set of systems.
- Future plans are being developed in conjunction with DOE and focus on key analyses needed by DOE.
- The next area of focus will be on the liquid hydrocarbon (HC) system being developed by Air Products. This is definitely the next system that should be evaluated.

Strengths and weaknesses

Strengths
- Strong analytical skills and long experience working with DOE and in the field of hydrogen, fuel cells and storage.
- Provides good value for the budget.
- TIAX is providing valuable results to the DOE and tech team on system analysis and especially cost model development.
- Good analysis to guide programs.
- The approach and execution of this project are both excellent.
- High scientific level of research and good CoE organization.
- The model provides an important cost estimate and comparison among the various hydrogen storage technologies from a total system perspective including life cycle analysis.
- Doing these kinds of analyses can be a “depressing and thankless chore”. It's a good thing we can find folks who are willing to do them because they are important to the program.

Weaknesses
- Some redundancy with the ANL analysis and assessment modeling effort.
- The focus on sodium alanate and sodium borohydride is outdated since developmental work on these systems is being substantially reduced or eliminated because these technologies do not show the potential to meet DOE performance targets.
- Systems studies always have a soft spot in them that others tend to “poke at”. The problem comes from having to define important answers when you don't have all the important details nailed down.
- None.
- Non-substantial.

Specific recommendations and additions or deletions to the work scope

- Focus of this work should continue to be on cost analysis to minimize over lap with ANL work.
- HFCIT systems analyzers have to lay their wish list on the table. The information they most definitely need to make credible analyses should be known to the entire community of developers, data gathers, and stakeholders.
- TIAX should be encouraged to carefully check for reasonableness of any data being received from suppliers with other information sources available, such as other suppliers or OEMs which have worked with the supplier. TIAX should do its best to evaluate information for feasibility and validity before accepting it at face value.
- TIAX should be encouraged to closely communicate with ANL regarding analytical models to ensure consistency.
- It is not clear how the TIAX and ANL work interact together and how the project can leverage their expertise to increase their output. TIAX has many valuable expertise that should be better integrated and coordinated with the ANL system analysis work.
- To intensify engineering investigations according to project.
- None.
- Consider Monte Carlo methods for better estimates of uncertainties.

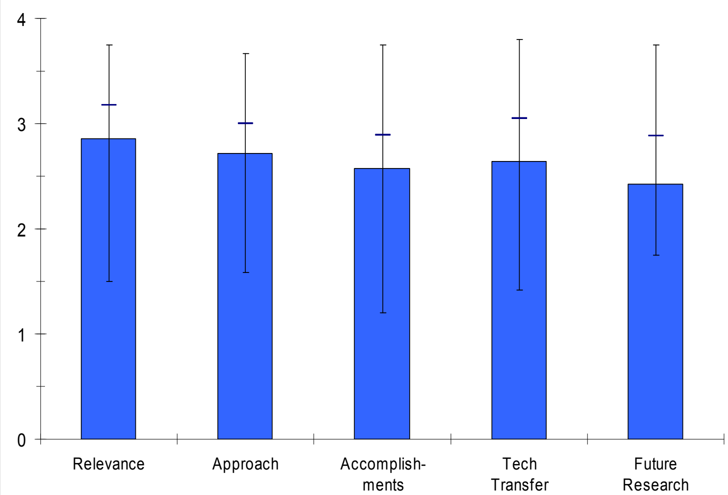
Project # ST-21: Carbide-Derived Carbons with Turnable Porosity Optimized for Hydrogen Storage

Jack Fischer PI; University of Pennsylvania; Yury Gogotsi (Co-PI), Drexel University

Brief Summary of Project

The objectives of this project are to:
1. Develop and demonstrate efficient, durable and reversible hydrogen storage in carbide-derived carbons (CDC) with tunable nanoporosity (2004-2005).
2. Determine the optimum pore size for hydrogen storage using experiment and theory (2005-2006).
3. Identify post-processing strategies and catalytic additives which maximize the performance of CDC-based hydrogen storage materials, using experiment and theory (2006-2007).
4. Finalize the design of a CDC-based H_2 storage material that meets 2010 DOE performance targets and commercialize it (2007-2008).

Overall Project Score: 2.6 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Generally aligned to program objectives & relevant to President's Initiative.
- Material potential for the DOE system target is not clear and material itself is identical to activated carbon.
- Certainly aimed at key goal of gravimetric, probably challenged on volumetric goal.
- This project is one of the independent, novel material awards that started in the beginning of FY04. Penn is trying to develop high surface area carbons that are made by removing the metal atoms from materials such as Ti, Si, Zr, or B carbides. The project goal is to produce materials that can meet the 2010 DOE target of 6 wt.%. The relevance is lessened somewhat because it appears that even with additional development, as discussed in the presentation, the material capacity would be about 6 wt.%, and thus the system level capacity would be much lower.
- This project is targeted towards the gravimetric, volumetric and cost goals of the hydrogen storage effort.
- This project aims the development of new improved solid state hydrogen storage materials. Storage is a critical component of DOE's RD&D plan.

Question 2: Approach to performing the research and development

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

- The approach is well thought out and has the potential to produce porous carbon structures at relatively low cost. The effort is focused on understanding precursor characteristics, processing conditions, and post treatments that are necessary to produce subnanometer pores and pore size distributions for binding hydrogen. The presentation indicates that the 2007 target of 4.5 wt.% can be met at LN_2 temperature and 50 atm pressure. It's hard to see how this approach can yield materials that, when placed in an appropriate system, would meet the DOE targets.
- The project approach is an interesting one towards introducing large volume fractions of very small porosity into a carbon matrix.

- Well-thought, reasonable approach capitalizing on the design rationalization of carbon materials. Good emphasis on methods and reproducibility giving confidence to the data and to the sample screening results. Nice coupling of theory with experiment.
- CDCs unlike activated carbon but like MOFs offer the advantage of having a narrow/uniform and tunable pore size distribution. However they have a much lower surface area and pore volume. They would have a clear advantage if they could be functionalized in a way others cannot. This is yet to be shown.

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

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

- Creat[ing] small pores can be an interesting technology.
- 4.5 wt.% excess is not extraordinary today; but not bad for activated carbon. Not bad for 7 months work however. [The project started in the beginning of FY05].
- There appears to be solid progress in developing the understanding of the fundamentals involved in tailoring these carbide-derived carbons for maximum hydrogen uptake.
- There is no comparison plot of the pore size distributions of the TiC, ZrC, SiC, and B_4C materials that are being studied. How do they compare?
- Interesting analysis showing that the gravimetric hydrogen storage capacity normalized to total pore volume is optimized in materials with primarily micropores rather than mesopores. Promising results reported here assist in getting a better insight into the hydrogen adsorption mechanism in porous carbon. Still this [CDCs] needs to be demonstrated that this is a 'workable' solution.

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

This project was rated **2.6** for technology transfer and collaboration.

- Strong points. Selection of evaluation for the mechanism investigation.
- Partly connected.
- Collaborations between Penn and its partners are in place and it is apparent that each has a significant role in the project.
- The best performing materials should be sent to SwRI for independent verification of the hydrogen storage results.
- Nice mix of expertise and testing facilities/methodologies. Industrial link is missing and should be sought after much earlier than intended in the project plans.

Question 5: Approach to and relevance of proposed future research

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

- Scope to [be] superior [to] activated carbon is not clear.
- Plan is more like goals, only the use of catalysts is at all specified. One clear item is round robin test, and that is good.
- Near-term and future plans are presented in some detail to inspire confidence that maximizing the hydrogen uptake in these materials can be accomplished.
- The program objectives and work needs to be re-aligned with DOE's short-term deliverables based on system capacity requirement. The use of 77K temperature is not a practical solution and a room temperature sorbent should be given higher priority. The proposed work does not address these.
- The future work to increase the volume fraction of very fine pores and to increase the heat of adsorption in these pores is good. Introducing catalyst particles into such fine pore structures may be difficult.
- Well planned future activities for a better understanding of the fundamentals of hydrogen adsorption in porous [materials]. Nevertheless, a breakthrough may not be so easy to achieve. Good use of resources and mix of techniques.
- Priorities should be set on increasing the strength of the hydrogen bond substantially above that of normal physisorption. If successful, then the push for higher surface area is justifiable.

Strengths and weaknesses

Strengths
- The presentation was very good. Sufficient detail was presented to judge that progress has been good. Some novel carbon materials have been produced with controlled pore size and distribution. These materials have higher hydrogen bonding energy than more conventional high surface area carbons and have the potential to have greater hydrogen uptake. Plans are well formulated.
- PI's experience and competency in the field.
- Excellent science. The materials studied are potentially low cost ones.
- Very good approach followed: looking at the rational design of adsorbents.
- Cost.

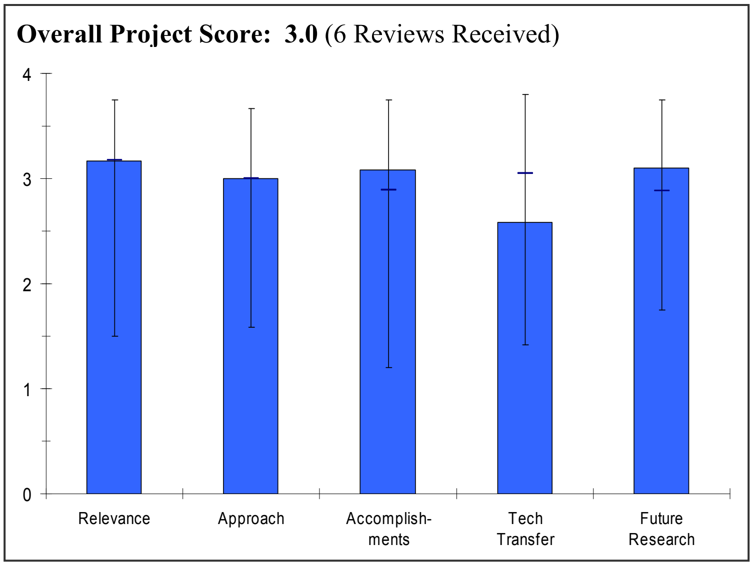
Weaknesses
- There is no clear advantage upon activated carbons in term of hydrogen uptake due to the surface area.
- Volumetric storage not likely to be obtained. "Hazy" plan for progress. Delivered hydrogen pretty low because so much is held at pressure below engine/fuel cell operating pressure.
- The presentation indicated that the material itself barely meets the 2010 target (6 wt.%). The system would be considerably lower.
- The program objectives and work needs to be re-aligned with DOE's short-term deliverables based on system capacity requirement. The use of 77K temperature is not a practical solution and a room temperature sorbent should be given higher priority. The proposed work does not address these.
- Given the current results, this project does not look like it has a high probability of meeting the 2010 DOE gravimetric target, even at a temperature of 77K.
- Still a risk area that may not deliver by the "end of the day."

Specific recommendations and additions or deletions to the work scope

- Materials for independent confirmation important to achieve. Be sure to include some "gold standard" labs. Need to work on improving deliverable hydrogen.
- Investigate any means to increase hydrogen uptake such as doping with light metals to determine if these materials have the possibility to achieve the 2010 targets on a system basis.
- The program needs to be reviewed and re-aligned with the overall DOE goal.
- Recommend to clarify the difference and benefit over the activated carbon. At this moment it is not clear the superiority of this material
- It may be useful to do some hydrogen permeation measurements through free-standing membranes of these microporous materials. This could give information on hydrogen diffusion through these structures.
- Need to tie in the material development work with the final storage system targets. Theoretical modeling for these types of materials could assist progress, however it must be challenging since these are amorphous materials. Kinetics and hydrogen diffusion issues may also be addressed on CDC prepared membranes. Consider emphasizing the added value of the uniform pore size in the CDCs giving them an advantage over the high surface areas activated carbons.
- Recommend to clarify the difference and benefit over the activated carbon.
- At this moment, the superiority of this material is not clear.
- Determine how to make good pores in high quantity and increase binding energy.
- Industrial link is missing and should be sought after much earlier than intended in the project plans.

Project # ST-22: Hydrogen storage in Metal-Organic Frameworks

Omar Yaghi; UCLA/University of Michigan



Brief Summary of Project

The overall objective of this project is to develop strategies for achieving Metal-Organic Frameworks (MOFs) that have increased uptake at higher temperature. This is being done by utilizing new concepts for increased surface area, implementing strategies for higher adsorption energy, and developing strategies for increased hydrogen density. Scale-up of favorable MOFs will be done and samples will be delivered to DOE for independent verification of storage capacity.

Question 1: Relevance to overall DOE objectives

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

- This project involves exploratory studies of metal organic framework (MOF) structures that have large internal surface area and have shown modest ability to store hydrogen at low temperature (77K) and high pressure (50 to 100 bars).
- Project is planned to address multiple challenges to support the Initiative.
- This project has the potential to develop low-cost hydrogen storage materials that can be added to high pressure hydrogen gas tanks, to significantly increase the tank hydrogen storage capacity, albeit at lower tank temperatures such as 77K.

Question 2: Approach to performing the research and development

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

- MOFs are synthesized by methods developed over several years by the PI and his co-workers. They are then characterized by XRD to confirm their structures and tested for hydrogen storage capacity.
- MOFs may have potential to address storage uptake and volumetric barriers.
- The metal-organic framework approach being pursued in this project is outstanding, truly world-class.

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

This project was rated **3.1** based on accomplishments.

- From the presentation, it is hard to tell what was done specifically for this HFCIT project and what was part of the history of accomplishments by the PI and his group. In round numbers, the project achieved [ca. 7 wt.% H] at 75K and 50 bar. These conditions are not likely to be viewed as being attractive for vehicle applications. But, there has been some nice science done by the Yaghi group over the past five or so years to elucidate the key features of MOF materials as sorbents.
- A lot of good work has been done in this project, but clear progress to DOE targets is not apparent.
- The project reports the development of a MOF-177 material that exhibits 7.5 wt.% hydrogen storage at 77K. This is the highest adsorption-based hydrogen storage value ever obtained to date.
- This is the first time an adsorbent reaches an excess capacity of 7 wt.%.

- Presenter has not provided comparison of volumetric storage characteristics of MOFs with those of compressed hydrogen (700 bar, 298K as well as 50 bar, 77K). Surface area characteristics of MOFs should be also expressed in volumetric terms as more critical for MOFs.

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

This project was rated **2.6** for technology transfer and collaboration.

- It is not obvious that this project is very well connected to any aspect of the HFCIT program.
- Collaborative relationships not clear though a couple of different organizations were mentioned.
- Significant technology transfer appears to be taking place through interactions with BASF. However, collaborations with other DOE hydrogen storage researchers appear to be somewhat more limited.

Question 5: Approach to and relevance of proposed future research

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

- Striving to get higher and higher effective internal surface areas is certainly the way to go, but they could "hit the wall" pretty soon (if they haven't already).
- Future research plan is laid out though not many details shown.
- Generally, increasing surface area would diminish the bulk density and thus have a negative effect on the volumetric energy density. One should optimize both the gravimetric and volumetric densities.
- The future research approaches may lead to MOF materials with significantly high hydrogen capacities at 77K, and perhaps at higher temperatures as well.

Strengths and weaknesses

Strengths
- The PI and his group are world leaders in the discovery/development of MOF materials. Lots of nice science has been done by that group. An impressive body of knowledge has been developed.
- MOFs present an interesting and new approach to solve some hydrogen storage challenges. Researcher appears to be very capable.
- Remarkable capability of synthesizing new improved materials.
- Highly innovative, world class research project that is rapidly yielding important results.

Weaknesses
- It just doesn't look like MOFs will meet DOE hydrogen storage targets at all or at least under conditions that are attractive for vehicles. Incremental advances beyond the present achievements won't be enough.
- Future plans not very detailed. Performance to DOE targets not clear.
- The group deserves better adsorption characterization capacities.
- More interactions of this project with DOE researchers would be beneficial to the carbon-based community at large.

Specific recommendations and additions or deletions to the work scope

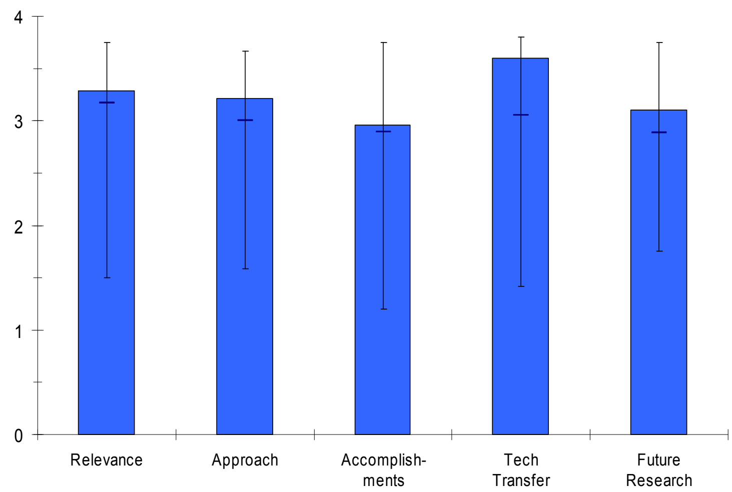
- Show performance to DOE targets more clearly. Solidify details of future work. More clearly identify the roles of other collaborators in project.
- Conduct calculations on total surface area requirement for the adsorbents per storage vessel to store required amount of hydrogen, based on experimental data. Compare specific surface areas of MOFs (volumetric) with that required per storage vessel to see if those can be attained.
- None.

Project # ST-23: NREL's research as part of the DOE Carbon Center of Excellence

Mike Heben; National Renewable Energy Laboratory (NREL)

Brief Summary of Project

Overall Project Score: 3.2 (8 Reviews Received)



[NOTE: This review is on NREL's contributions, not on the entire DOE Carbon Center of Excellence. Each of the partners is evaluated separately.]

The focus of the National Renewable Energy Laboratory's work is on hydrogen storage in carbon-based materials in conjunction with the DOE CoE on carbon-based hydrogen storage materials. The objectives are to determine the extent to which metal-carbon hybrid materials can reversibly store hydrogen, to tailor the mechanism of hydrogen storage through nanostructural control, and to develop low cost, reproducible, and potentially scalable processes for production. NREL performs activities in five task areas in support of the DOE mission and ensures that CoE activities are aligned with DOE goals, promotes communication and collaboration to expedite progress towards targets, and creates a nimble research and technology development environment to pursue new opportunities as they arise, in support of DOE goals.

Question 1: Relevance to overall DOE objectives

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

- Goal is good, moderate delta H is certainly a need. That said, several of the several projects are unlikely to meet the goals even if fully successful, most of the actual results are still all theoretical, despite 2 years of these theories existing, little or no testing of the ideas experimentally exists.
- This project is obviously aligned with the President's hydrogen vision and the RD&D objectives. Storage is key to the Hydrogen Initiative and the carbon-based option is a part of the overall effort. However, it seems obvious that this option is further behind in its science base than the other two options being pursued and it is not clear that it can get much beyond the 2006 target of 6%.
- The CoE initiative and its work program is completely in line with the overall DOE objectives.
- Carbon based sorbents are a critical group of novel materials for hydrogen storage. The diversity and depth of the program makes it an important component of the storage subprogram.
- From the basic research of H_2 physisorption in nanomaterials we have obtained 2 major key aspects that enhance this weak interaction: the existence of Pi-systems and metal sites. Two approaches of the hydrogen storage problem are based in a combination of these two: MOFs and metal decorated carbon based materials (C_{60}, CNTs, etc). My personal opinion is that both are equally important, and even though they are built with a different philosophy, they actually belong in the same family of materials that combine Pi-systems and metal sites.
- Adsorbent based materials have the potential to offer alternatives to metal hydride approaches however the CoE really needs to move away from reporting storage values at subambient conditions. Subambient conditions will greatly complicate a tank based system and reduce further the challenging volumetric issues that most adsorbents face. A plausible explanation as to how milder enthalpies (8-12 kJ/mol) can be achieved at 100-200 bar pressures - Are there exceptions to the Van't Hoff plots??

Question 2: Approach to performing the research and development

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

- Approach is largely sound, theory to experiment to development, but the process seems stalled in theory in many areas. Spillover work is however a nuts and bolts program possibly making progress. Program should get credit for expanding its focus since last year to include more programs that are not carbon-nanotube-focused. On the management side, the CoE has put in place leaders in the area and seems to meet often and try to get people working. I may not fully agree with all aims, but they are managing the people to try to accomplish them.
- The progress suggests the approaches are appropriate, but it is difficult to assess them given the limited information available on so many projects. Certainly the fostering of collaboration is a good approach.
- Targets appear not feasible within the tight timeframes proposed at least judging from the current status and progress made.
- Broad R&D portfolio, new ideas; offers potential for higher density materials and goes beyond the SWNTs increasing the chance of success. Well organized and focused. Coupling of theory/modeling with experiment is beneficial to the project execution, timing and overall progress.
- This program has a strong focus on theoretical/modeling approach. It is not clear if the current structure and management scheme can narrow down the choices in a timely fashion. The synthesis, scaling and storage prototype testing are not clearly defined.
- Again, numbers reported must move in the direction of ambient temperatures and a plausible explanation for how 100-200 bar pressures can be achieved with 10-12 kJ/mol enthalpies. MOF and nano engineered molecules seem interesting and easily modified/synthesized but work on carbon nanotubes needs to be seriously considered. Particularly NREL as a project needs to move away from buckyballs etc. These molecules seem difficult and expensive to synthesize. The CoE is young but it needs to increase the pace in which modeled molecules are actually synthesized and proven to be stable.

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

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

- Spillover work is an accomplishment, nearly doubled capacity available. Many theories have produced ideas to pursue (small D [diameter] nanotubes, methods for propping tubes at critical distance). Made B [boron] doped nanotubes but storage currently low. Potential Fe addition to bucky ball, not clear yet, but believe cross linked to another ball. If there was not conflict over the validity of the predictions this might be higher, the key in the end is real, independently verified hydrogen storage.
- I would rate the progress as excellent in the given time, but because of the limited science base as mentioned in [question] 1 relevance above, I doubt the effort will overcome all the barriers in the storage area.
- Meeting the deadlines of go/no-go for SWNT. Standardized reference points for reporting correct amount of physisorption hydrogen need to be applied to all CoE members. Same applies for BET surface area, i.e. standard reference points, procedures etc.
- A fair amount of work, good progress, substantial accomplishments. Significant degree of innovation and novelty for discovering new promising materials - still though, need to accelerate shift from models to materials synthesis and demonstration. Careful steps were taken for understanding potential sources of errors, for validating measurement techniques and looking into data repeatability and accuracy.
- Very good progress is made on molecular modeling aspect. Good progress is made in some areas of synthesis, identification and storage testing. The issue of SWNT go/no-go should be resolved soon and free up more resources to the other molecular candidates. The synthesis and testing and characterization should be given higher priority in the coming years.
- The theoretically promised materials have not been synthesized yet.
- Much more progress was made than in the previous years, MOF is the highlight along with James Tour work on nano engineering carbon nanotubes. It seems as though the work is converging to structured molecules of combined metals and organics. MOF start with metal and add organics; nanotubes start with organics and functionalize in metal. Both methods seem to converge to the same idea of achieving these Goddard rules. Some evidence was presented regarding that the metal would not agglomerate.

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

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

- Connectivity good.
- NREL seems to be doing a superb job on this aspect.
- CoE is well covering many different kind of material and technologies.
- Well structured network; good blend of modelers and experimentalists; strong collaborative effort. Leading role in the organization of many conferences and extensive list of publications and presentations.
- There is a good mix of expertise in this CoE. It's not clear how often there are internal reviews and how closely the various group communicates with each other.
- CoE has strong partners and is well organized, however NREL really needs to explain and push to partners and reviewers the volumetric and enthalpy/pressure challenges that are key to the existence of CoEs.

Question 5: Approach to and relevance of proposed future research

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

- Though it will be important to increase resources devoted to verifying predictions in reality, this can not be the theory CoE.
- The plan is logical, but I do question the rush to systems work given the lack of strength in the science base.
- Fundamental understanding on issues related to hydrogen physisorption needs to be focused on. Targets for [FY]07 appear to be very unlikely based on current status.
- The proposed work is appropriate for further progressing towards reaching the objectives. If the SWNT work is not finally retained (there is a 'Go'/'No-Go' decision point still this year), experience accumulated so far in the field is valuable. SWNTs could be the molecular building blocks for advanced adsorbers. Passage from materials' scientific exploration to engineering design could be accelerated.
- Surface areas need to increase significantly in order to improve volumetric capacity. Need to increase impetus on MOFs. Work on the “bucky bells” etc. seems academically or scientifically interesting but should seriously be evaluated for practicality to H_2 storage.

Strengths and weaknesses

Strengths
- Very good team that seems to be actively collaborating.
- Strong leadership to achieve targets with carbon-based materials, even [once] it was quite difficult. Use of basic science and calculations to show the enhanced physisorption is very good.
- Many members working on different synthesis routes.
- Strong collaborations, diversity in expertise, innovation, novelty, and an overall systematic approach that increase the probability of success.
- Excellent progress on molecular modeling. A good mix of candidates for synthesis and testing. Strong technical background in the area. The spillover work is promising.
- Outstanding theoretical work.
- Many novel ideas and easily synthesized and modifiable molecules are evident. Partners show great control over their materials.

Weaknesses
- Realization of this material to on vehicle tank is not clear.
- Integration of efforts between members not obvious. Progress towards goals.
- Still a risk area that may not deliver. Indeed, a breakthrough in this research area may be difficult; in any case, if it is to happen, the CoE is the best mechanism for achieving it through the optimal use of resources and integration of efforts. Management of such a big group/CoE is challenging – nevertheless NREL is on track with the coordination/communication scheme already in place.

- The program needs more focus on empirical development such as synthesis, identification, and testing. The program needs more internally defined criteria on go/no-go decisions.
- The theoretically promised materials have not been synthesized yet.
- Volumetric capacities remain low. NREL needs to give plausible argument as to how this can be addressed. A technically plausible plan also should be presented on how to produce good storage capacity at 1100-500 bar range with lower enthalpies (6-12 kJ/mol). How can the thermodynamics be bent?
- Yaghi has left, so the only real progress in physical storage is Yang's work. Need to emphasize the things that are working more. Dillon is talented but asking her to realize these materials almost alone is not wise, more resources must be devoted to this effort.
- BC nanotubes probably aim at too low a target storage.
- As mentioned above, the science base is weak.

Specific recommendations and additions or deletions to the work scope

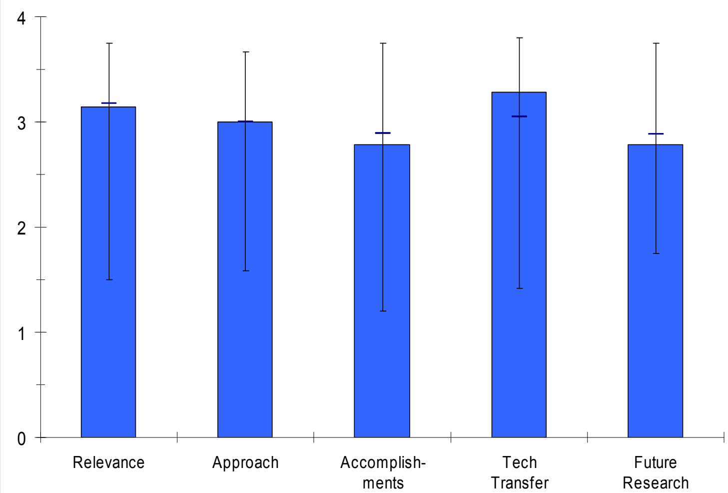
- Be sure to evaluate leadership as well as tech work.
- Regarding the spillover results, mechanism of hydrogen atoms interactions need to be checked by measuring reversibility, kinetics, etc as a 1st step.
- Equal attention should be paid to the gravimetric and volumetric storage capacity of the materials investigated. Down selections and go/no go decisions on materials/approaches should be fully respected and the project should be flexible enough to move on when required to other materials. Use effectively the mechanism currently in place for the management of this sizeable, expertise-diverse CoE; this is critical to the success of the project. Associated challenge: re-direction of resources, within the CoE, when and where it is required.
- Advantages of the carbon based physisorption is pressure balance. If the bonding energy became too large whether this advantage is still remaining or not is questionable. Physisorption is quite difficult to measure accurately with a small amount of material, common protocol for the measurement is very important. Repeatable, reliable measurement being established in the project is very precious and work is appreciated.
- Need to verify theory predictions, Manpower distribution needs to change to many people trying to verify high storage predicitons and few on theory. Key is to make material progress in concert with the predictions. This is only likely with several more people working on making these materials, give Dr. Dillon some serious help. This goal of 12 kJ for room temperture storage is almost certainly not going to work except at high pressure, they really need to check this calculation.
- I would recommend emphases on the science base for at least two more years, then go to systems work after an assessment.
- Need to show the milestones and roadmap for the entire program. Need to show approximately when the decision points are and methodology to reach consensus. It is recommended to pull the go/no-go decisions slightly forward. The program need to start some priliminary work on storage system development or coordinate with the system analysis work. The CoE mangement needs to make the decision making process more transparent. The CoE needs to re-aligned some of its goals versus results obtained by ANL and TIAX analyses.
- Extra effort must be put in the synthesis of the proposed materials.
- Seriously consider reducing SWNT work and increasing significantly work on MOF type materials, conductive polymers and clathrates.

Project # ST-24: Enabling Discovery of Materials With A Higher Heat of H_2 Adsorption

Alan Cooper; Air Products & Chemicals, Inc.

[Partner of the DOE Carbon Center of Excellence]

Overall Project Score: 3.0 (7 Reviews Received)



Brief Summary of Project

In this project, Air Products is developing a hydrogen storage measurement technique that could accelerate new materials development, providing critical guidance to DOE and their contractors enabling new materials development. General quantitative computational models are being developed for new materials resulting in efficient materials discovery/optimization towards meeting DOE system targets for hydrogen storage.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Well aligned and of relevance to the DOE objectives.
- As part of the Carbon CoE, this program has high relevancy.
- This project is part of the carbon CoE. They measure H_2 adsorption and heat of adsorption, and they calculate the Gibbs excess adsorption energy. These are all fundamentally important parameters for assessing hydrogen storage materials.
- Supports progress by partners, not key to program but useful to those outside the program.

Question 2: Approach to performing the research and development

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

- Should achieve their goal with this approach.
- The project approach is excellent, particularly as it combines theoretical and experimental aspects in the proper proportions. The aspect of rapid, inexpensive measurement techniques can be expected to be central to success.
- Good, systematic, clear approach engaging theory and experiment.
- The approach does not address the storage goals adequately. The decision making process is not clear.
- The measurements being made provide important technical data and guiding insights for the development of carbon-based hydrogen storage media. The results of modeling heats of adsorption provide insights about the limitations of carbon based materials and about what can be done to improve the storage capacities.
- Provide "guidance" to others? Can this be regarded as a task?

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

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

- Established capsule method.
- Developed a physisorption calculation method.
- Would have liked to see more of the funding used.
- The theoretically promised materials have not been synthesized yet. The progress of the project is not satisfactory.

- The sorption capsule technique for simple, inexpensive hydrogen storage measurements via a weighing approach is excellent. The SWNT data at 25°C is particularly revealing in terms of the potential for pure SWNTs to be able to store any significant levels of hydrogen at room temperature. The bundling morphology of SWNTs is shown to have relatively little influence on hydrogen storage behavior. Fundamental modeling indicates that boron incorporated into graphite (but not nitrogen) can enhance hydrogen bonding.
- Satisfactory degree of accomplishments and sound progress. Innovation, under both modeling and experiment, to support the materials development work and speed up the overall progress.
- The high throughput screening (HTS) measurement tools in place at Air Products are of great value to the HFCIT program. The presenters showed that the HTS equipment provides accurate results and demonstrated that carbon nanotubes adsorb hydrogen on a scale similar to but slightly greater than graphite. Specific results of interest are (1) that only a fraction of the H_2 is strongly adsorbed because the heat of adsorption drops with increasing H_2 loading and (2) the nature of similarities and differences in how homogeneous and heterogeneous carbon nanotubes adsorb/desorb H_2.
- Wasn't the capsule technique developed previously?
- How does the cutting and measurements on SWNTs relate to NREL work? Why are we still doing this?
- I'm not convinced of the validity of the ab-initio calculations on the B-C structure.
- The proposed materials and the first results do not seem to be very promising.
- The heat of adsorption is the key parameter associated with the gravimetric and volumetric hydrogen storage capacities and temperature/pressure conditions of materials that store hydrogen by adsorptive mechanisms.

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

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

- Well connected.
- Through the carbon-based CoE, this project is making significant overtures for others to disseminate and employ the rapid measurement techniques and fundamental modeling approaches that are being developed.
- Increased collaboration with members of the CoE.
- Air Products appears to be well coordinated with the CoE, including NREL and other partners.

Question 5: Approach to and relevance of proposed future research

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

- Path to objective is not clear, no likely systems were defined and method of finding them is not clear.
- The future study of hydrogen spillover effects is good.
- Reasonable plans for the future.
- Well paid attention to the accuracy of hydrogen adsorption measurements and effort on increased collaboration with partners in this field.
- It is not clear how the go/no-go decision affects this project. What contingencies are planned?
- The future work builds on FY 2006 progress and focuses directly on key barrier issues for carbon based hydrogen storage materials.

Strengths and weaknesses

Strengths

- Very solid work, both experimentally and theoretically.
- Systematic approach to the problem; innovation. Fair attention paid to accuracy of hydrogen adsorption measurements and of predictive modeling.
- The PIs have higher degree of competency in the field.
- This project is an important part of the CoE. Air Products is well equipped and well staffed to make seminal contributions to the progress of the CoE.

Weaknesses

- No clear route to goal of adsorption at significant density on carbon based materials at room temperature and moderate pressure.
- There appears to be a certain reluctance to pursue new materials unless further funding is forthcoming.
- Speed of transition from SWNTs to other materials.
- Redundant work on SWNTs. Ambiguous approach on future direction. Some contingencies are not addressed.
- Not clear what the value-added component of this project is relative to the CoE. Much of the capability seems redundant to other partners.
- There are no obvious weaknesses in the organization and conduct of this project.

Specific recommendations and additions or deletions to the work scope

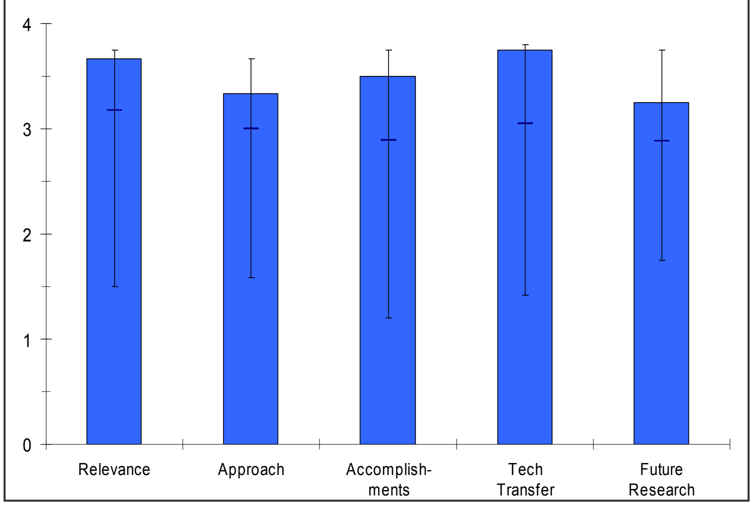
- Need to make concerted effort to calculate or otherwise define systems with high capacity in physisorption, make them, and test them.
- Accelerate move to new boron and nitrogen-containing materials building up on recent work. Consider possible benefits of intensifying partnerships and further exploring collaborations for new hydrogen materials testing program.
- None.
- Keep up the good work. These seemingly basic measurements and calculations are essential to developing the understanding required to make progress towards removing the remaining barriers in the hydrogen storage and delivery area.

Project # ST-25: Neutron Characterization in support of the Carbon and Metal Hydride Centers of Excellence

Dan Neumann, Presenting; Terry Udovic (co-PI); NIST

[Partner of the DOE Carbon and Metal Hydride Centers of Excellence]

Overall Project Score: 3.5 (6 Reviews Received)



Brief Summary of Project

The overall objective of this project is to support the development of hydrogen storage materials by providing timely, comprehensive characterization of CoE-developed materials and storage systems using neutron methods. This information will then be used to speed the rational development and optimization of hydrogen storage materials that can be used to meet the 2010 DOE system goal of 6 wt.% and 45 g/L capacities. In FY 2006, structures are being characterized, and compositions and adsorption/absorption site interaction potentials for hydrogen in/on several candidate materials are being identified. CALPHAD calculations of potentially promising alloy-hydride phase relationships are being done. These interactions will be refined and studied to obtain a greater understanding of them. This information will be extended to characterization/calculations of new materials.

Question 1: Relevance to overall DOE objectives

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

- Neutron characterization is an important experimental support activity for the objectives of both the metal hydride and the carbon-based materials centers of excellence.
- Important tool to help expedite storage material selection process.
- Fully and directly addresses the President's HFI and multi-year plan in support of the C & MH CoEs.
- NIST efforts are highly relevant to the Hydrogen Initiative. NIST plays a critical role in the characterization of materials that are developed by the MH and carbon CoEs.
- Provides critical support to MH and carbon CoEs

Question 2: Approach to performing the research and development

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

- The focus of the neutron activities is good. Neutrons are being employed for chemical composition studies, crystal structure determinations, the location of hydrogen sites in crystal structures, and hydrogen binding sites. The CALPHAD activity appears to be somewhat non-interactive with the neutron activities and not well-integrated with them.
- Ability to characterize materials under real world conditions instead of lab conditions is/will be a valuable tool for material selection.
- This heavily cost shared project provides neutron services and thermodynamic calculations to both the carbon and MH centers of excellence. Although some neutron work is done via other collaborations, it is none the less invaluable to have the principal national neutron analysis facility involved in the DOE program. Response to request from CoE partners seems to be very quick.
- The approach at NIST is to utilize their neutron source to characterize materials by determining structures, compositions, and adsorption/desorption interaction potential of hydrogen in newly developed metal hydrides

and carbon-based materials. In some instances, NIST utilizes their computational expertise to determine phase relationships in promising alloy hydrides. The approach aligns closely with the objectives of both the metal hydride and the carbon-based centers of excellence.
- Neutron methods are critical to understanding hydrogen in or on materials.

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

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

- Neutron studies have been conducted in a number of areas of importance to the metal hydride and carbon-based materials CoEs. The establishment of the hydrogen adsorption sites in the MOF material is particularly notable.
- The project has just started but has generated many useful sets of data already. Cost to DOE is very low.
- NIST has achieved all of the objectives set out by the CoE partners. They provided accurate compositions of materials synthesized by the CoE partners. NIST determined the hydrogen adsorption sites in MOFs. They also observed what appeared to be evidence of hydrogen spillover in carbon nanohorns.
- Provided good support to both MH and carbon CoEs.
- Difference Fourier technique appears to be a powerful tool to determine H binding sites.
- in-situ measurement technique very valuable in understanding reaction pathways.

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

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

- Neutron studies are part of two of the three centers of excellence. The integration of the neutron characterization work with the xenters' activities and needs appears excellent.
- This project closely supports both the carbon CoE and MHCoE. Project appears to have good collaborative relationships with other team members.
- Good collaborations with many of the carbon and MH CoE members.
- Collaborations are excellent. NIST work is for the most part directed by the CoEs, so data is immediately transferred to the CoE partners and is used to structure follow on work.

Question 5: Approach to and relevance of proposed future research

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

- There is no discussion of future neutron in-situ and higher pressure activities, which would seem to be quite useful.
- Near term - support nanotube go/no-go decision.
- Continue providing neutron services as needed.
- Proposed work focused on continuation of the neutron scattering of various metal hydride systems and on thermodynamics computations. The proposed work is clearly laid out and should help speed the discovery of new promising materials.

Strengths and weaknesses

Strengths
- Basically, neutrons are being employed as an important analytical tool by both the metal hydride and the carbon CoEs.
- NIST has high capability in neutron methodology. Strong relationships with carbon CoE and MHCoE.
- High competency in the field. Good utilization/synergy of the resources within the national laboratories. Powerful analytical techniques and independent analysis.
- World class neutron facility with rapid interaction/collaboration ability.
- NIST utilizes their neutron source and expertise to support materials development efforts in the metal hydride and carbon-based centers of excellence.

Weaknesses

- This work does not appear to be as valuable in coming up with new materials. If volume of materials to be characterized increases, is there capacity to handle it?
- The CALPHAD thermodynamic H-Li-Mg-B-Si database does not include nitrogen.

Specific recommendations and additions or deletions to the work scope

- Continue to support and speed the material selection process.
- The project provides technical/analytic service to the CoEs. The work could have been shown as part of the poster presentation. The proper evaluation of this project is done by their primary customers such as metal and carbon CoEs.
- This reviewer is not convinced CALPHAD calculations are that useful to the overall effort. There are many other first principle modeling and thermodynamic calculation projects within the DOE program. But it is OK to keep that in the project.
- Why is the CALPHAD work being done? What project or PI is it being done to support?
- Expand the CALPHAD thermodynamic database to include nitrogen.
- NIST may be able to aid the CoE partners in determining the pathways to improved materials. Within the constraints of available resources, the CoEs should be encouraged to utilize NIST expertise in materials development as well as characterization.

Project # ST-26: Cloning Single Wall Carbon Nanotubes for Hydrogen Storage

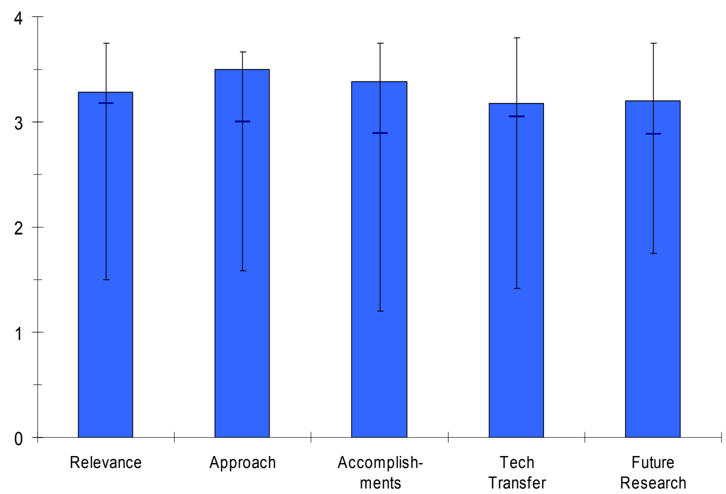
James Tour; Rice University

[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

The overall objective of this project is to develop nanostructures and nanoengineering processes that enable synthesis of hydrogen storage materials that can be used to meet the 2010 DOE gravimetric (6 wt.%), volumetric (45 g/L) hydrogen storage system goals, with excellent uniaxial thermal transport properties. This will be accomplished by developing processing techniques to produce specific types of nanomaterial structures with increased available surface area.

Overall Project Score: 3.3 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project is obviously aligned with the President's hydrogen vision and the RD&D objectives. Storage is key to the Hydrogen Initiative and the carbon-based option is a part of the overall effort. However, it seems obvious that this option is further behind in its science base than the other two options being pursued and it is not clear that it can get much beyond the 2006 target of 6%.
- Aligned if 60 g/L volume goal is feasible. Ability to make pure confomers of nanotubes of one diameter will assist in finally defining the potential.
- Meeting DOE 2006 targets for carbon nanotube seems difficult based at least on current hydrogen wt.% status.
- This is a leading research group developing their synthesis methods [for] large scale production of aligned single wall carbon nanotubes. Emphasis on cloning is less important for this nanotube application than for others done by this group. The program as constituted is designed for relevance to advancing carbon nanotube synthesis in general rather heavily focusing on the hydrogen storage application.
- The project is focused on hydrogen program goals and addresses key targets of RD&D objectives.
- Cloning nanotubes is a very interesting idea. Cloned nanotubes decorated with metals look promising materials for hydrogen storage [due to] pi-systems, large surface areas and metal binding sites.

Question 2: Approach to performing the research and development

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

- Cloning is a suitable approach, as is the goal of close spacing with control.
- Looks good.
- Concept and technique are novel but concerns about data reproducibility.
- The approach to making tubes with controlled intertube separation seems promising. Here collaboration with Yakobson is important. Also the approach to large scale synthesis is promising. The approach to making tubes with room temperature 6 wt.% capacity is high risk.
- Good approach exploring nanoengineering and moving systematically into the prediction of optimum structures and hydrogen storage capacities.
- Nanoengineering seems to be the only solution to hydrogen storage problem.
- Reproducible nanotube production process with optimized parameters is critical to their use as storage medium.

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

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

- Made 75 to 80% density nanotube bundles. Cloned nanotubes. Storage at 3 wt.% or so. Accomplished a swelled fiber and then cross linked to hold value at 9 angstroms.
- Doing extremely well.
- Hydrogen absorption barriers should be carefully considered when designing the CNTs [carbon nanotubes]. Project needs to consider several issues, i.e. densification won't be useful if it impacts hydrogen diffusion.
- Progress with cloning is good, but not of high relevance to hydrogen storage program. Progress with vertical tube alignment is very good and will help with thermal management. Use of oleum [fuming sulfuric acid?] to control the interlayer separation is going well. The progress with adding Li and cross linking agents is good. Interaction with Yakobson work is progressing well for the interlayer spacing control project.
- Significant accomplishments – robust approach.
- A lot of milestones and decision points in very near feature.

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

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

- Probably could be better linked to others, NREL seems their main partner.
- Good. But seems to be mostly relying on own work.
- More collaboration is needed with other groups within the carbon CoE.
- This group interacts well with industry and the research field overall. The Interaction within Rice University is strong.
- Collaborations through the CoE.
- A good balance of theory and experiment.

Question 5: Approach to and relevance of proposed future research

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

- Cross linking of individual nanotubes at various spacings. Lithium intercalation. Downselect to best sizes.
- Logical progression.
- Dopants for enhancement of interactions need to be selected carefully and several dopants might need to be considered. Data reproducibility might be a concern.
- With a focus on the milestone of at least 4 wt.% hydrogen mass-uptake at room temperature in 2006, the challenge seems large from where they are now. Advancing the science and technology to meet other goals seems more promising.
- Rather ambitious program but a systematic approach has been adopted for the near future work.
- The RT [room temperature] adsorption measurement of the C/Li systems should be carried out sooner than planned to show their real potential. There is no point of perfecting the SWNTs bundle geometry if the potential of achieving high RT adsorption is not there.

Strengths and weaknesses

Strengths
- Ability to design precisely defined systems.
- Knowledge and hard work.
- The Tour group is very strong in the synthesis of nanotubes. They are doing well with mass production methods, nanotube alignment and control of intertube spacing.
- Novelty and robustness of approach.
- Techniques and success in densification of CNTs.

Weaknesses

- None obvious from the presentation.
- Engineering aspects are not yet addressed — Heat of filling for a 5-kg hydrogen system, able to be filled in a reasonable time, is not a trivial problem; 'showstopper'.
- The prognosis of meeting hydrogen uptake goals is very challenging for both the short term and the time line of the program. Has the team considered whether the amount of lithium uptake needed to meet 2010 DOE goals for room temperature hydrogen storage is reasonable from an experimental viewpoint. Focus is very strong on increasing capacity. Little work is in progress to evaluate other needs for a hydrogen storage system.
- CNT properties impact on hydrogen physiosorption not thoroughly considered. Densification of CNTs is a good approach, however over densification should be avoided, how to control? reproducibility?, etc. Intercalation should be carefully considered and optimized specially in these reported dense systems
- Reasonable chance this will never work. But it is important to find out.

Specific recommendations and additions or deletions to the work scope

- Continue as planned. The results speak for themselves.
- Consider whether scale-up needs to be addressed earlier on in the project. Also, should the work on the Li-doped system with the room temperature uptake be given higher priority in the program?
- Recommend less emphasis be given to nanotube chirality, and recommend that the team focus primarily on tube diameter control. Recommend that difficult challenges in developing a good hydrogen storage material other than storage capacity be identified and the probability for successfully addressing these challenges be assessed.
- Recommendation to consider shifting to basic research.
- As soon as possible validate that the 9 angstrom materials are making major headway in storage.

Project # ST-27: Advanced Boron and Metal loaded High Porosity Carbons

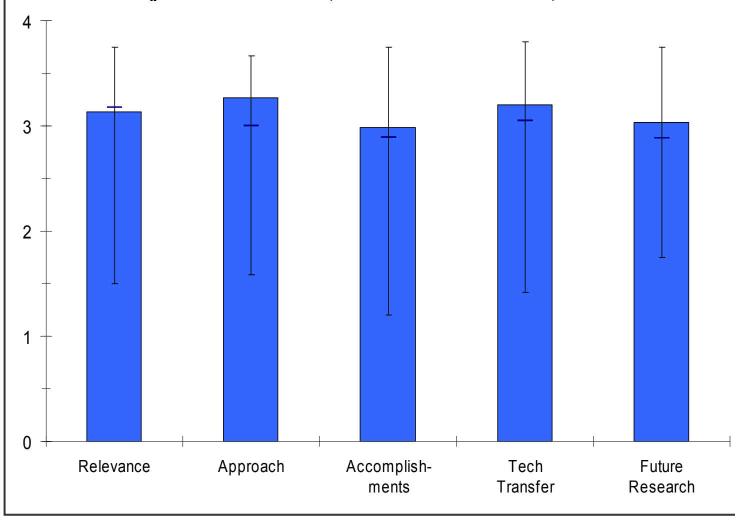
Peter Eklund; Pennsylvania State University

[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

The overall objective of this project is to develop advanced hydrogen physi/chemisorption materials that have reversible, low mass density, low volume, good thermodynamics. The goal is to achieve reversible storage of ~6 wt.% at 200K, 100 atm by 2008. High specific surface area (SSA) carbons are the focus of this work. The carbon framework will be chemically modified for enhanced H_2 binding energy. Boron will be substituted to enhance the binding energy of hydrogen. Boron is a light element and the only one known to substitute in the sp^2 framework without serious structural distortions.

Overall Project Score: 3.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project serves the overall DOE objectives and supports the hydrogen program; it addresses a number of key barriers.
- Onboard hydrogen storage is critical to the success of the Hydrogen Fuel Initiative.
- This project is part of the carbon CoE. The effects of loading boron and certain metal atoms into the framework of high porosity carbons on hydrogen storage capacity is being investigated.
- The program goals address pertinent barriers for the DOE program on hydrogen storage. Science advances and technology advances are both addressed.
- Boron in C based materials enhance the H_2 binding and the B-C materials look promising for hydrogen storage applications.

Question 2: Approach to performing the research and development

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

- Well founded approach involving and coupling a number of techniques and exploring a number of possibilities.
- Good fundamental science approach towards incorporating boron into carbon materials.
- Boron incorporation into carbon is accomplished by electric arc vaporization. Electron microscopy, hydrogen adsorption measurements, neutron diffraction, Raman spectroscopy, and DFT calculations are performed to characterize the products.
- Approach of boron doping to enhance binding energy seems promising. The BC_3 approach may have high pay-off but may be hard to achieve, but this is a good idea for a research program. Combining calculations with experiment in a strongly coupled way is highly encouraging.
- A good combination of techniques and approaches for a spherical investigation of the problem.

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

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

- A satisfactory list of accomplishments following the use of complementary methods and taking advantage of access to different techniques, both experimental and theoretical. Substantiated claims of boron substitutes into the carbon framework (doped SWNTs).
- Good progress in incorporating boron into carbon lattices. Progress towards meeting the hydrogen storage capacity target has been limited.
- It was shown that boron additions increase the overall binding energy for hydrogen. Unfortunately, not all of the boron in the target gets into the carbon lattice. There seems to be some limits to how much boron one can introduce. DFT calculations provide some insight about H binding energy. Progress is modest at best.
- The team effort has made a lot of progress in the past year in synthesis by three different approaches, in understanding the role of boron through very successful neutron work at NIST, and through developing a high throughput synthesis method. The discovery of a surface roughening technique is another significant advance. The research quality is high.

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

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

- A good collaboration scheme under the CoE.
- Greater coordination and independent testing of storage materials is expected as the project progresses.
- Collaborations exist with several partners in the CoE.
- Interactions among Penn State collaborators seems good. Good interactions with industrial groups at Air Products and Carbolex. Productive collaborations with NIST in doing important scientific advance in boron-carbon materials synthesis are noteworthy.

Question 5: Approach to and relevance of proposed future research

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

- Well planned future activities concentrating on the main issues to be resolved using a continual developmental feedback loop of synthesis, hydrogen storage and design/modeling. Appropriate actions have been decided to verify and extend the observed enhancements in storage capacity and binding energy bearing in mind the actual system targets.
- The research plans build on the results of the FY 2006 effort. This will include volumetric studies of hydrogen storage in B-substituted carbons and continued development of high-impact boron-carbon structures.
- Proposed research on boron-carbon systems is impressive. This should be strongly pursued. Proposed work matches emphasis to increase hydrogen uptake. Some effort to check other factors on their barriers to achieving the 2010 DOE target for hydrogen storage (viewgraph 2) should also be contemplated.

Strengths and weaknesses

Strengths

- Kg level of CNT is producible and controlled production of enhanced physisorption material.
- Complementarity of methods used and collaboration with key research groups in this field.
- Good fundamental science approach towards incorporating boron into carbon materials.
- Both from a scientific and a programmatic standpoint, this is the strongest program I was asked to evaluate. The group uses two approaches that are backed up by a strong experimental program at both Penn State and NIST and a strong, tightly coupled theory/experimental program. Although the hydrogen storage problem is very hard, the approach taken shows promise and progress has been outstanding.

Weaknesses

- Energy to create the CNT is enormous.
- Tight timetable; need to timely move from theoretical calculations to material synthesis, demonstration of hydrogen storage performance and actual design of a viable storage system.
- Although the group is cognizant of the many factors needed to be successful in the hydrogen storage problem in general, the group could say more about the future plans about addressing some of the factors in hydrogen storage, other than increased storage capacity. In defense of this research group, increased storage capacity is the most important consideration right now because it is viewed as a show-stopper.
- No perceptible weaknesses.

Specific recommendations and additions or deletions to the work scope

- It is not clear that the modification of CNT has potential [or not]. Especially energy to create such a material seems quite inefficient.
- Because of the high capability of this team, they may be a team that can see if physisorption may have other attributes for hydrogen storage that chemisorption does not have, and to make some quantitative comparisons between the big picture outlook between chemisorption and physisorption. Additional funds would be needed to investigate this enhanced scope, but in the long term this may be an important investment for the overall program.
- The thermodynamic 'penalty' for cryogenic hydrogen storage may be too high; intensify your efforts following your current approach accounting also for operability.
- Issues raised about roughness effects should be explored to assure that there are no misconceptions.

Project # ST-28: Hydrogen Storage by Spillover

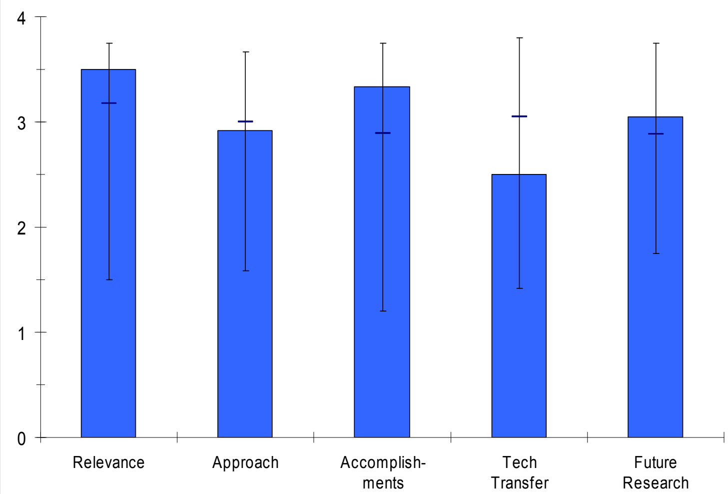
Ralph Yang; University of Michigan

[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

The overall objective of this project is to develop carbon-based hydrogen storage materials with capacities in excess of 6 wt.% (and 45 g/L) at room temperature. This will be done by developing and optimizing our new bridge-building techniques for spillover to enhance hydrogen storage. This will result in a mechanistic understanding for hydrogen spillover in nanostructured carbon-based materials for the purpose of hydrogen storage.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project is investigating the implications of a previously known surface science phenomenon called spillover in the context of hydrogen storage.
- This project is focusing on room temperature hydrogen storage in inexpensive carbon-based materials.
- Possibly the most well aligned program in the CoE.
- Project seems to fully address President's HFI and DOE RD&D plan.
- Interesting concept on catalyst performance that may produce useful results for many other materials, not just limited to carbon structures?
- Energetics of hydrogen spillover of 3-4 kcal/mol is optimal for hydrogen storage application at ambient temperature. The concept of spillover effect for hydrogen storage is novel.

Question 2: Approach to performing the research and development

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

- A bridge building approach is used to emulate conditions for spillover that occur in certain catalysis embodiments. There was much discussion about the applicability of the concept after the presentation. The presenter needs to address the concerns expressed.
- The project focuses on optimizing the hydrogen spillover effect from catalyst particles into hydrogen-storing substrates, such as carbon and MOFs. It may potentially have wide applicability to a number of different materials systems.
- Approach seems to be productive. Need to work harder on improving rates.
- There are a number of world-wide efforts on carbon that are exploring a spillover (metal addition) approach. It is not quite clear what is different about this "bridging" method. In any event, this is a rather low-cost effort that deserves to continue.
- Bridge building technique seems to be the key difference to traditional metal doped carbon molecule approaches. This seems to be making the difference.
- Volumetric storage density of ~40 g H_2/L was measured based on pelletized powdered obtained after adsorption. Need to consider compacted bulk density of pelletized material to consider meeting DOE system targets.

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

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

- The reported results showed a bridge enhancement effect. Unfortunately, the storage capacity levels were well below ones of interest for meeting DOE [wt.%] H system targets. More experiments are needed to confirm the spillover notion.
- Excellent progress is reported for hydrogen storage at room temperature, with 4 wt.% reported (but not described in detail). This is the highest room temperature value reported for a carbon-based material to date.
- Doubled hydrogen uptake and roughly 4 wt.% at low pressure (100 atm).
- Progress has been made, at least as stated on paper, for carbon and MOFs. A patent has apparently emerged that involves making and attaching the metal/carbon bridge to the storage substrate. Contractor has developed a carbon that has multiple weight percent reversible H-capacity at room temperature (as well as reasonable volumetric capacity). However, given the dismal history of reproducibility in carbon, it is imperative that the results be independently checked.
- Has achieved one of the best room temperature carbon based materials to date. Still has work to do on volumetric and kinetics and try out other materials other than activated carbon.
- Although the project is in relatively early stages, the approach of "bridging spillover" may lead to materials meeting 2010 weight targets.

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

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

- Supposedly, some verification work was done by NREL and NIST.
- This project is a part of the carbon-based materials CoE. However, somewhat expanded interactions with the CoE members would be highly beneficial.
- Seems a bit isolated, but certainly connected to outside programs.
- There is relatively little collaboration (only NREL and NIST). Connections should be made to confirm the preliminary results and to think toward the building of a demonstration tank.
- Dr. Yang should now be working more closely with other material producers to see if his bridges can be applied to and enhance their materials.
- In this stage the project is "localized" at the University of Michigan. Planned collaboration is only with respect to verification and characterization. There was no scientific collaboration on the material development discussed.

Question 5: Approach to and relevance of proposed future research

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

- The future plans are certainly appropriate because they should resolve many of the issues about the importance of spillover. Let's see what the project produces for next year's review.
- The project displays a focused, clear plan for the future work.
- Plans are appropriate with the exception of the most important thing, getting independent confirmation.
- Reasonable, albeit rather lacking in specifics.
- Important to try concept on different receptors such as MOFs which may yield better results, still needs to propose plan on how to improve the volumetric density, Does this phenomenon only work on a single layer adsorption of the catalyst? Is this why kinetics are slow? Did not propose a plan to improve kinetics.

Strengths and weaknesses

Strengths

- The spillover concept is sufficiently intriguing that it deserves more study.
- The reported value of 4 wt.% hydrogen storage at room temperature via the spillover approach is the highest room temperature value reported thus far for a carbon-based material.

- Potential for getting actual high density hydrogen storage on light materials.
- Seems to be achieving reasonable room temperature storage levels in carbon for the first time. Should be easy to reproduce.
- Novel approach for hydrogen storage that can be applied to a series of materials.
- Not working at 77K, actually attempting storage at room temperature.

Weaknesses
- Based on what was presented in the talk, several issues need to be resolved before there will be any community wide acceptance of the concept's significance or utility. Additional confirmatory experiments must be done.
- Speed of the hydrogenation/dehydrogenation process.
- Uses an expensive catalyst (Pt).
- Volumetric storage densities were evaluated based on volume of pressed pellets. Since this parameter is critical for carbon-based materials, there is a need to provide more thorough measurements of volumetric densities that can be attained in the storage vessels.
- The reported 4 wt.% room temperature hydrogen storage value needs to be independently reproduced by other research organizations. Specimens should be sent to SwRI for independent analysis.
- Slow kinetics. Still precious metals are required but hopefully that could be significantly reduced with the introduction for the bridges to aid spillover.

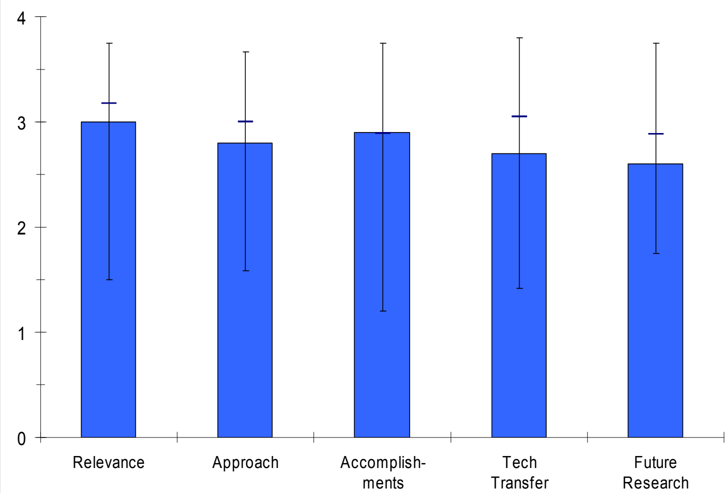
Specific recommendations and additions or deletions to the work scope

- The bridge may add to the volume and mass of the storage material embodiment in a detrimental way. The PI should give attention to this matter.
- Focus on economics (base metal catalyst?), improved volumetric capacity, thermodynamic measurements and practical container considerations (heat rejection during rapid recharge). More partners would help. Highest priority should be to confirm preliminary results.
- Focus on improving kinetics, and lowering pressure required. MUST get the material tested at SwRI to confirm result.
- Dr. Yang should now be working more closely with other material producers to see if his bridges can be applied to and enhance their materials.
- None.

Project # STP-02: Effects and Mechanisms of Mechanical Activation on Hydrogen Sorption/Desorption of Nanoscale Lithium Nitrides

Leon Shaw; University of Connecticut

Overall Project Score: 2.8 (5 Reviews Received)



Brief Summary of Project

The University of Connecticut is investigating, modeling, and developing a novel, mechanically activated, nanoscale Li_3N-based material that is able to store and release ~10 wt.% hydrogen at temperatures below 100°C with a plateau hydrogen pressure of less than 10 bar. Research in FY 2006 is focused on the effects of mechanical activation on hydrogen sorption/desorption kinetics of the $LiNH_2$ and LiH mixture, stability of LiH in different environments, and dehydrogenation behavior of the MgH_2 and $LiNH_2$ mixture.

Question 1: Relevance to overall DOE objectives

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

- The project seeks to lower hydrogen reaction temperatures in the lithium amide system through the approach of mechanical activation.
- The project gives good support to meet DOE objectives on relatively new storage materials. The PI did not directly refer to the DOE objectives.
- Investigating the fundamental mechanisms involved in mechanically activating metal hydrides to increase the kinetics of reversible hydrogen storage is very relevant to the Hydrogen Fuel Initiative.
- The effort adequately addresses the need for solving the storage problem by optimizing the amide systems. Storage is a key problem for the Hydrogen Fuel Initiative and RD&D plan.

Question 2: Approach to performing the research and development

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

- The project approach appears to be over-extending itself. It should just focus on understanding and optimizing mechanical activation effects.
- The project is well organized and technically structured to overcome the main problems of the studied hydrides. The approach examines all the technical possibilities in a complete manner to reduce the sorption/desorption temperatures.
- The approach is to mechanically activate metal hydrides by ball milling, chemically modify amides to destabilize them, and to employ catalysts to enhance adsorption and desorption.
- Contractor is focusing on mechanical milling to solve the temperature and kinetics problem with $LiNH_2$, $LiNH_2$+LiH, and $LiNH_2$+MgH_2 along with several associated other details (e.g., the stability of LiOH in O_2-containing environments). This is a relatively overworked area not likely to get very far toward DOE 2010 system targets and beyond.

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

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

- Mechanical activation studies to date have been thorough, and do suggest some lowering of hydriding/dehydriding temperatures. Results suggest that mechanical activation at room temperature appears to enhance a complicating and potentially deleterious effect of decomposition of $LiNH_2$ to Li_2NH plus NH_3.
- The progress is very good with clear comprehension and appraisal of the possible modification mechanisms. The use of separated approaches gives convincing answer on the possible improvements and limitations. The problem of ammonia production is well considered and will deserve continuous attention. The stability results must be better explained even in relation to the used methodology, not completely clear.
- Connecticut has demonstrated that mechanically activating lithium amide-lithium hydrides and lithium amide-magnesium hydrides decreases the activation energies of hydrogen adsorption/desorption in these systems. Dehydrogenation starts at room temperature, but temperatures over 200°C are required for complete release of hydrogen. Two catalysts investigated so far have not improved kinetics.
- Extensive nice work has been done. Progress has been made in accelerating low temperature kinetics, but ubiquitous NH_3 has been again confirmed, an “Achilles' heel” of these particular systems when it comes to supplying a fuel cell with H_2. The PI is optimistic about defining ball mill conditions where NH_3 does not occur, but similar marginal results from around the world cause this reviewer to have some skepticism.
- Conclusion appears to be wrong: loss of H_2 during mechanical activation does not indicate reversibility at room temperature

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

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

- This project may benefit from expanding its collaborations with other research activities, particularly in the area of catalysts for the lithium amide system.
- The network of collaboration may be improved with some exchange with the MHCoE.
- There is evidence of collaboration between Connecticut and PNNL. However, their relationship to the rest of the MHCoE is not too clear.
- Collaborations rather limited.

Question 5: Approach to and relevance of proposed future research

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

- The catalyst and theoretical/modeling aspects of the future work should be eliminated and the focus remain on understanding and optimizing the mechanical activation effects. Mechanical activation processes should be performed at lower temperatures, such as liquid nitrogen temperature.
- The future steps are well justified by the experimental results and analysis of the work already done.
- Future plans are presented but their relation to other work in the CoE is not clear.
- Li-amide and all its mixtures are highly overworked areas which have not lived up to promise during the last year particularly. I am not in opposition to a little more work, but I think there is substantial risk is spending much more time and money on this system that is unlikely to produce NH_3-free H_2. I do not criticize the diligence and skill of the participants; they have perhaps just chosen a questionable system.

Strengths and weaknesses

Strengths

- Mechanical activation lowers the hydrogen generation temperature by approximately 100°C, at least on the first dehydrogenation cycle.
- The project is well organized with clear targets and technically justified approaches.
- Good, careful experimentation.

Weaknesses

- The project has limited collaborations.
- An overworked family of materials that have potentially insurmountable problems (e.g., NH_3 generation and N mass transport). I must also question the very widespread modern tendency to use ball milling as the solution to all storage materials problems. Ball milling is not cheap under such long time conditions.
- It appears that ball milling of the $LiNH_2$ promotes its decomposition to $Li_2NH + NH_3$ at temperatures of approximately 50°C. This would appear to have a deleterious effect on the reaction $LiNH_2 + LiH = Li_2NH + H_2$, since it reduces the amount of $LiNH_2$ available for this reaction. Furthermore, the potentially mitigating reaction $\frac{1}{2} NH_3 + \frac{1}{2} LiH = \frac{1}{2} LiNH_2 + \frac{1}{2} LiH$ does not occur completely, and so much of the NH_3 escapes, with the loss of its associated hydrogen. This is why the milled material shows only 5.5 wt.% hydrogen, as compared to 6.5 wt.% for the unmilled material.

Specific recommendations and additions or deletions to the work scope

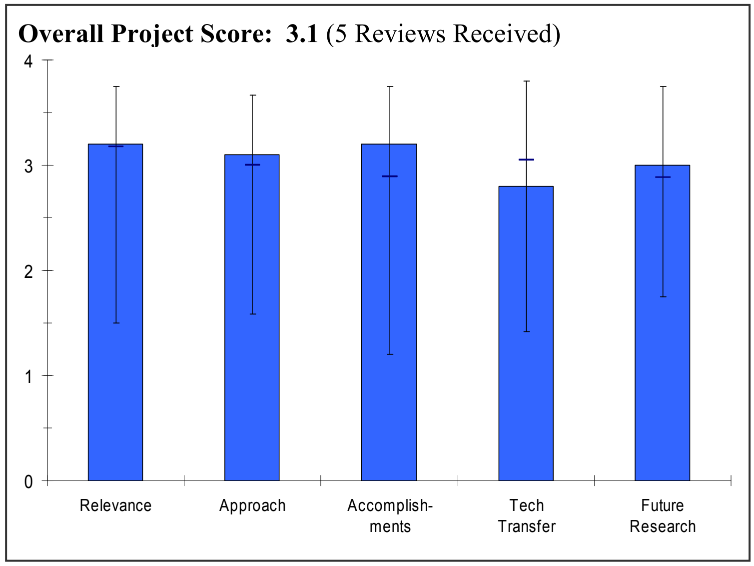
- The project approach appears to be over-extending itself. It should just focus on understanding and optimizing mechanical activation effects.
- The project must include, more clearly, methodologies for stability tests and for addressing the problem of NH_3 production with different hydrides.
- Be prepared to move on to newer materials with more potential and less problems. Also, I think we need to see some practical processing cost numbers associated with such long-time ball milling.
- The project needs to include effects of cycling on hydrogenation/dehydrogenation capacities and temperatures. Ball milling at lower temperatures such as liquid nitrogen temperature should be examined. Study of catalyst effects should be eliminated, since the project is not large enough to investigate this well, and this is already being examined by other researchers.

Project # STP-03: First-Principles Modeling of Hydrogen Storage in Metal Hydride Systems

Karl Johnson(PI); University of Pittsburgh; David Sholl, Carnegie Mellon University
[Partner of the DOE Metal Hydride Center of Excellence]

Brief Summary of Project

The University of Pittsburgh and Carnegie Mellon University are computing thermodynamic properties of metal hydride alloys, including ΔH for known reactions to test the accuracy of the approach and ΔH for new reactions to identify promising destabilized compounds. Interfacial properties of hydrides are also computed in this project. Hydrogenation in destabilized hydrides and other systems is studied to assess reversibility, identify common hydrogenation pathways that might be applicable to other materials, and assess the role of interfacial transport. The goal is to design a practical destabilized hydride system.



Question 1: Relevance to overall DOE objectives

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

- Good effort, largely aimed at discovery of suitable novel materials/reactions. Well aimed at the relevant problems to support the HFI.
- This first principles modeling of metal hydride systems is very valuable to the MHCoE in identifying promising hydrogen storage materials that have sufficient capacity and low activation energies. The work is very relevant.
- Important contributions to the MHCoE.
- The project gives good support to meet DOE objectives.
- Materials do not meet targets. Destabilized materials still promising and need to be understood.

Question 2: Approach to performing the research and development

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

- The project offers a useful tool for selecting hydrides with potentially interesting characteristics.
- Good computational methods. Mapped new hydrides including some with reasonable capacities.
- Modeling/DFT approach is a good candidate for discovery of novel reactions, and can screen reactions much faster than experiment. Proved utility of this approach in present year by prediction of a handful of reactions with high densities and suitable thermodynamics.
- The approach appears sound.
- Modeling approach focused on materials and processes (e.g., destabilized hydrides) studied in the CoE.
- Looking at critical thermodynamic properties of materials.

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

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

- The project has well progressed in relation to the fixed objectives. The lack of experimental data limits the validation of the results of the various models' application.
- Good results for new hydrides. Interesting results for oxide surfaces.

- Computational screen has produced several candidate reactions! This is exactly the kind of thing that DFT is useful for in a materials discovery effort. Nice to see a good demonstration of this utility. Not clear whether phonon (zero-point) contributions have been included in these energetics or not. Last statement on summary slide needs a bit of explanation, since experimentalists at this meeting (HRL, Stanford) claim that Mg_2Si is not reversible under any conditions investigated thus far.
- University of Pittsburgh has investigated over one hundred reaction schemes that had not previously been studied for hydrogen storage. Pittsburgh believes that the accuracy of the computational methods is within about 15% after comparing the results with experimentally determined values of heats of reaction. Five new destabilization schemes were identified that have high capacity and favorable thermodynamics. Presumably the CoE partners will try to synthesize these materials and determine their characteristics.
- Screened large number of reactions and identified a few promising ones.
- Supported Mg_2Si experimental work.

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

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

- The coordination with some MHCoE must be improved, because of the common interest in identifying suitable hydrides.
- Need to work more closely with experimentalists to confirm results. Oxide formation hypothesis could be confirmed.
- Dissociation studies: based on premise the dissociation of H_2 might be a strong kinetic barrier. This project should communicate with the effort at Stanford; these authors seem to have shown quite convincingly that H_2 dissociation is not a limiting factor.
- The collaboration among the CoE partners with Pittsburgh appears strong. If the other CoE partners follow through with these promising leads, it will reinforce the value of the CoE concept to the DOE program. This could well be an illustration of the total effort and accomplishment of the CoE is greater than the sum of the efforts of individual partners on their own.

Question 5: Approach to and relevance of proposed future research

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

- The future steps are well justified by the experimental results and analysis of the work already done.
- Computation approach sound and should continue, need to initiate more collaboration with experimentalists.
- Need to see a pathway (either within this project, or generally within the CoE) that these novel predictions are going to be experimentally tested! This was not apparent from the CoE's presentation [Sandia presentation, there was not a formal center presentation.] (not really a criticism for this project). Why not? Is there a lack of coordination between this effort and Sandia? Make sure to coordinate future efforts with Sandia and Stanford to avoid computationally investigating kinetic mechanisms that have already been experimentally proven to be not rate-limiting.
- Future work plans are a little vague.

Strengths and weaknesses

Strengths
- The project has adequate resources and expertise in applying a set of models.
- Sound computational approach. Examination of destabilized materials.

Weaknesses
- The project has limited collaborations.
- Need more collaboration with experimentalists.

Specific recommendations and additions or deletions to the work scope

- The project requires more collaborations to fruitfully integrate theoretical activities with the experimental ones.
- Ensure that these results are incorporated into the University of Illinois data base for use by the CoE members.

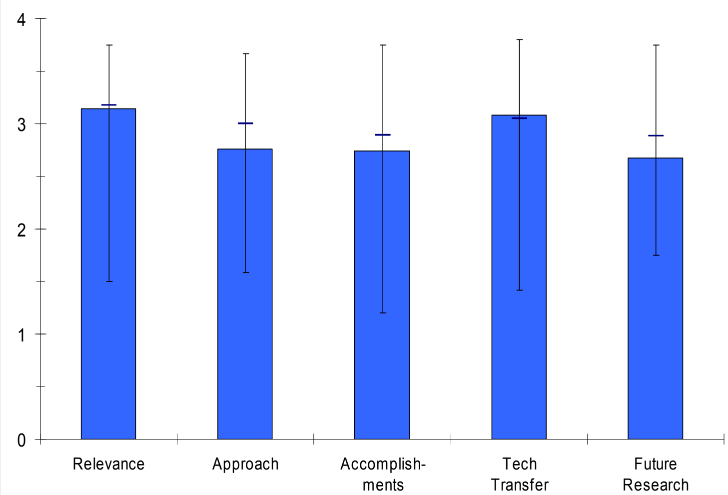
Project # STP-04: Hydrogen Storage Research in support of the DOE National Hydrogen Storage Project

Don Anton; Ted Motyka (PI), Ragaiy Zidan, Savannah River National Laboratory (SRNL)

[Partner of the DOE Metal Hydride Center of Excellence]

[SRNL has several projects supporting the DOE Hydrogen Storage Program. All storage work was covered under this single review. SRNL is a member of the MHCoE; partner to Alfred University (microspheres), and partner to the UTRC materials project.]

Overall Project Score: 2.8 (5 Reviews Received)



<u>**Brief Summary of Project**</u>

This project at SRNL is conducting research to identify rechargeable hydrogen storage media with a gravimetric capacity of 7.5 wt.% or greater. In addition, this project is developing and modeling the performance of gravimetrically and volumetrically efficient solid state hydrogen storage systems.

<u>**Question 1: Relevance to overall DOE objectives**</u>

This project earned a score of **3.1** for its relevance to DOE objectives.

- SRNL has provided support to the development of new materials with the potential to store hydrogen.
- SRNL contributes to the metal hydride CoE efforts by developing hydride synthesis/regeneration processes and engineering on-board systems that affect the release of hydrogen in a controlled fashion. This later activity is critical to understanding how these storage systems can be implemented in fuel cell vehicles. Not sure how relevant the hollow glass microspheres work is to the mission of the MHCoE.

<u>**Question 2: Approach to performing the research and development**</u>

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

- Molten state processing work, and alanate-based materials discovery efforts seem to have a strong overlap with UTRC project (and SRNL is listed as a collaborator on the UTRC project). Since both of these efforts have a considerably high amount of funding, it is not clear that both projects are actually adding value. No indication is given as to why the microspheres work should be pursued. What are the advantages of this approach? What sorts of densities, rates, cost could one estimate to be achievable for these microspheres?
- Multiple approaches in materials discovery are being pursued by SRNL. In addition SRNL is providing support to several organizations that are involved in the DOE hydrogen storage program.
- The approach is ad hoc. The work is a mixture of significantly different projects.
- SRNL is investigating alane rehydrogenation and is planning to make preliminary system designs. They continue to look at various alanates synthesized by means of a molten salt process to produce the alloy. In addition, SRNL is leading the engineering design effort for the CoE. The next generation system designs are building upon their current prototype system development efforts. This latter effort addresses the storage technical barriers of system-based rather than materials-based metrics.
- Technical barriers not addressed.

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

This project was rated **2.7** based on accomplishments.

- For the amount of money in this program (and the possible duplication in some aspects with UTRC), very little seems to have been accomplished in the current year.
- The significant accomplishments that SRNL reported are the development of baseline system models, mass flow models in packed beds, and the fabrication of new system prototypes. In addition, SRNL has developed mechanical-chemical synthesis techniques for producing kilogram quantities of catalyzed complex metal hydrides. In addition, SRNL is assisting Alfred University by evaluating hollow glass microspheres that the university has prepared for their hydrogen storage characteristics.
- Significant progress in several areas during the past year. It is time for SRNL to focus its activities on selected areas that have the greatest potential for meet the gravimetric and volumetric targets for hydrogen.
- Projects appears in early stages to judge.

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

This project was rated **3.1** for technology transfer and collaboration.

- Extensive interactions with a number of hydrogen storage research organizations.
- High degree of interaction with the specific project. Need a closer interaction with the alanes activity in the MHCoE.
- Collaborations appear to be in place through the CoE partners.

Question 5: Approach to and relevance of proposed future research

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

- Alane work: should show some simple theoretical estimate of the efficiency of the proposed electrochemical process. It is critical to know something about the efficiency to evaluate whether or not this idea could possibly work. System-level work and modeling is still the most unique portion of this project. This aspect should be a focus going forward, and the PI should ensure that there is no duplication of effort in this regard with the other analysis activities at Argonne and TIAX.
- The future plans appear to be "more of the same"; activities - need to focus on specific materials that show the potential for meeting DOE performance target. Work on materials that clearly won't hit the target shown must be minimized to complete in a reasonable time frame.
- Future plans were presented. They indicate that work on systems with higher capacity than the alanates will be emphasized in response to comments from last year's AMR [annual merit review]. However, continued work on alanates is also in the plan. It's not clear how much work on the higher capacity materials is planned relative to the work on alanates. The engineering design task will be emphasizing safety related aspects of on-board hydride systems.
- Timeline ends on 9/30/2006. [National laboratories are funded through annual operating plans.]

Strengths and weaknesses

Strengths
- SRNL shows strong experience and capability in a range of materials discovery.
- Strong competency in the area. Innovative ideas on the alane regeneration. Good progress. This program has the only system design work.
- The engineering task is providing important insights into the issues facing the design of on-board hydride systems. That task is also addressing thermal management issues as they relate to integration into refueling stations.
- Multidisciplinary research focusing on several materials and engineering related issues. Collaboration with other CoE members.

Weaknesses

- Given the funding available, the range of activities seem to be too broad- effort should be focused to ensure a critical mass of activities in areas where the materials under development (e.g. alanes) show a real potential for being able to meet the DOE wt.% requirements.
- The project composed of various and substantially different subtasks.
- One of the review comments from last year was to undertake cycling tests with alanates; no cycling results were shown.
- Progress and data validation of concepts.

Specific recommendations and additions or deletions to the work scope

- Focus the activities in order to increase the probability of developing material, using the unique capabilities of SRNL, to meet performance requirements for hydrogen storage.
- Continue supporting activities.
- Need to ask the BNL work on alanes to better coordinate with the SRNL activities. There are a lot of beneficial synergies between the two for the MHCoE activties.
- Show the total project budget. Only the budgets for 2005 and 2006 were shown. The SRNL budget may not be adequate for the amount of work they present.

Project # STP-05: Thermodynamically Tuned Nanophase Materials for Reversible Hydrogen Storage: Structure & Kinetics of Nanoparticle and Model System Materials

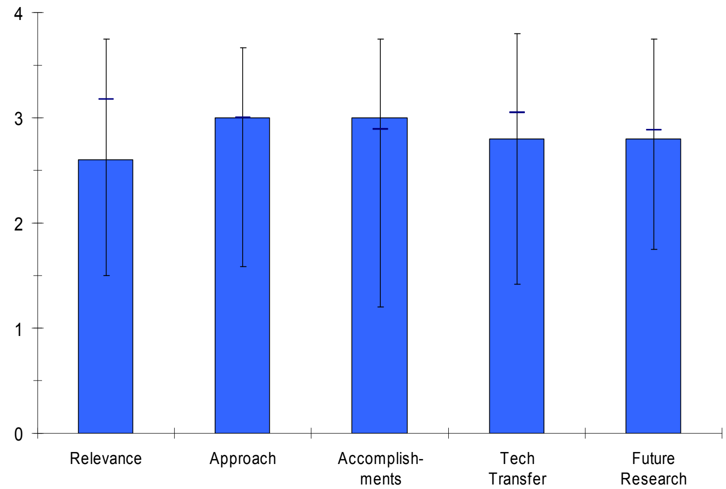
Bruce Clemens; Stanford University

[Partner of the DOE Metal Hydride Center of Excellence]

Brief Summary of Project

In support of the DOE Hydrogen Program, Stanford University is conducting research in the following three areas: in-situ structural studies of hydrogen storage materials using high brightness x-ray source at Stanford Synchrotron Radiation Laboratory, light metal hydride model material systems to investigate phase change and catalytic processes associated with hydrogen cycling, and kinetic modeling of nanoparticle phase transformations to illuminate mechanisms of hydride formation in nanoscale materials.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- President's HFI and RD&D plan indirectly addressed in work, but effort is rather fundamental and supportive without much reference to practical system goals.
- The project addresses some key objectives of the DOE plan.
- The project up to now was aimed at answering the fundamental question whether Mg_2Si can be re-hydrided. The scope, however, is too narrow to have a chance of success in meeting the DOE targets.

Question 2: Approach to performing the research and development

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

- Synchrotron light source is a good addition to DOE's portfolio and makes this project unique. Thin film work provides interesting (and unexpected) insight into the hydriding mechanisms for several reactions.
- The synchrotron XRD work is useful and should help support other DOE-supported activities. The thin film work may have some merit, but this approach is being used elsewhere (not much in DOE program). Maybe a little different approach. How are the nano-thermodynamic calculations different from others. Are they believable?
- The project offers an interesting method to solve main problems of Mg-based hydrides.
- Thermodynamic model is too simple. Is it possible to use macroscopic surface energy?
- The in-situ spectroscopic approach is good in the development of fundamental understanding of the problem of hydriding magnesium silicide.

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

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

- Thin-film Mg_2Si studies provided interesting results, though all attempts to hydride were still unsuccessful. This thin-film geometry could be useful in studying mechanisms for other reversible reactions as well. Could similar experiments be performed with MgB_2 rather than Mg_2Si? Thermodynamic modeling of effect of nanoparticle size on hydriding is interesting, however, the results seem to suggest (contrary to the text on the slide) that a very small ~1-2 nm particles are necessary to move MgH_2 into a reasonable temperature/pressure regime. Is it feasible to imagine making such particles and having them survive cyclic hydriding/rehydriding? (The DFT studies could directly consider the change in bonding for H in MgH_2 as a function of proximity to the surface; how many atomic layers down does one have to go before becoming "bulk-like"?

- The project is rather new, but some useful results have been recorded. This film work has been successfully put into place and will hopefully provide good fundamental results in the future. So far, not much profound in the way of guiding our path to the practical future. The nano-MgH_2 calculations are not very convincing from a practical perspective. PI suggests profound positive changes in MgH_2 thermodynamic stability with drastically reduced particle size, but in fact such results have never been experimentally seen over some decades' work on Mg. The nano-Mg does not tend to stay nano.
- The project shows interesting results with the preparation of nanoscale samples. The thermodynamic modeling for nano size systems offers new research opportunities.
- Experimental results are very useful and interesting.

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

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

- Rather limited. For the synchrotron XRD work alone, the DOE can benefit more if a more thorough set of collaborations could be developed within the CoE and other parties.
- The coordination is well justified and appropriate to the planned activities.

Question 5: Approach to and relevance of proposed future research

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

- Will be very interesting to see what can be learned from synchrotron studies in the future. Future work should focus on this important aspect. Not clear what is planned in this area? Not clear what is going to be done in the area of nanoparticles. Does the thermodynamic modeling predict particle sizes that can be realistically synthesized? Can some of the modeling predictions be directly tested experimentally?
- Generally reasonable.
- The future steps are well justified by the experimental results and analysis of the work already done. The combination of practical and theoretical activities increases the possibility of success.
- Should be better focused on key barriers.

Strengths and weaknesses

Strengths

- Potential to support other DOE-supported activities, synchrotron source service, only thin film studies within DOE program(?).
- The project has adequate expertise and instrumental resources.
- Very good experimental equipment and cooperation in MHCoE.

Weaknesses

- Not very close to those who need to meet DOE targets in the required time frame.

Specific recommendations and additions or deletions to the work scope

- Need to confirm (for the first time) that Mg thermodynamics can actually be significantly changed. Contractor should try to demonstrate distinctly improved nano-MgH_2 thermodynamics (more than one cycle) within 6 months.
- Size effects in thermodynamic equilibrium are well-known but not investigated for nanoparticles. Would be good to extend investigations in this direction.
- Suggest to apply more reactive probe molecules (e.g., chlorine) to understand whether magnesium silicide will react at all under the experimental conditions. After quick go/no-go decisions, consider other more promising materials for fundamental in situ studies (project scope needs to be expanded).

Project # STP-06: Fundamental Studies of Advanced High-Capacity Reversible Metal Hydrides

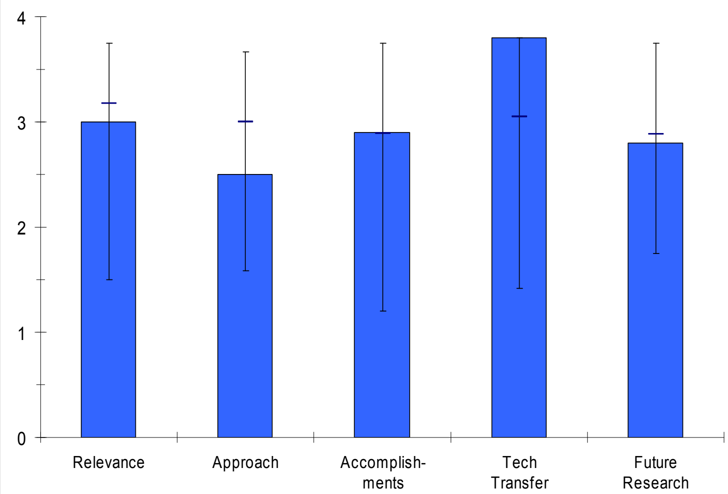
Craig Jensen; University of Hawaii

[Partner of the DOE Metal Hydride Center of Excellence]

Brief Summary of Project

The University of Hawaii is developing advanced high-capacity, reversible metal hydrides for hydrogen storage. The objectives of the research are to characterize the active titanium species in Ti-doped $NaAlH_4$, develop a model of the mechanism of action of dopants in the dehydrogenation and re-hydrogenation of $NaAlH_4$ and related capacity hydrogen storage materials, determine the enthalpy of dehydrogenation of high capacity hydrogen storage materials, and develop catalysts to improve the hydrogen cycling kinetics of "thermodynamically tuned" binary hydrides with potential to meet the DOE 2010 system gravimetric storage capacity target.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Contractor does address President's HFI and multiyear RD&D plan, at least indirectly. The effort thus far is mostly fundamental in nature, at least as presented in specific detail here.
- The project contains activities able to better drive decision on some key objectives of the DOE plan.

Question 2: Approach to performing the research and development

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

- Trying to elucidate the role of Ti in $NaAlH_4$ has been a long-standing puzzle. The current PI has worked on this problem for several years now, but there is still no mechanism forthcoming. At what point does one cut their losses and work on something else? This was the main criticism of this project last year too.
- The approach is based on using a wide variety of experimental tools (often via collaborations) to understand the mechanism of Ti enhancement of $NaAlH_4$ and Na_3AlH_6 H/D kinetics. The approach seems to be derived from Hawaii activities prior to this contract and is sometimes confusing in both time frame and applicability. PI should be reminded to focus on the end objective - to extend the Na-alanate findings to more promising systems. Is that possible, or is Na alanate unique?
- The project considers all the necessary steps to optimize alanates. The project is effective in anticipating no-go decision on alanates by starting the study of alternative hydrides, well selected and motivated.
- Task 3 and task 4 should be coordinated with ST-15 and STP-5 [Brookhaven and Stanford University] projects.
- Prior data obtained by the author as well as by other institutions (e.g., UOP) have clearly shown that Ti-doped alanate will not meet DOE targets.

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

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

- How is it known that Al_3Ti is catalytically inactive? Other results (i.e., from Sandia and BNL) have suggested possible mechanism for Ti at the surface of Al to act as a hydrogen dissociation catalyst. Small point: Al_3Ti is tetragonal, so it's not clear what is meant by "orthorhombic Al_3Ti"?
- An impressive list of findings have been published. It seems to this reviewer that results should have been collected into a useful, unified model of kinetics enhancement by now. All the world seems to be realizing the alanates are not solution and we look forward to the PI carrying this work to the next stage, apparently borohydrides. When will we learn more about the new 11 wt.% borohydride?
- The project has well analyzed the effect of Ti species on alanate behavior. The kinetics of the dehydrogenation process is well characterized and improved by Ti dopants. The work on novel hydrides just started has already given interesting results with a claimed 11 wt.%.
- Spectroscopic results obtained for $NaAlH_4$-Ti systems are useful, however, no mechanisms for potential improvement of kinetics have been revealed.

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

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

- One of the best spectra of collaborations within the DOE program - CoE, national and international. Most praiseworthy! The relationships among University of Hawaii, Hawaii Hydrogen [Carriers LLC] and UOP are not clear relative to the DOE CoE effort described here.
- The project has well organized and justified collaborations.

Question 5: Approach to and relevance of proposed future research

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

- Work on $LiBH_4/MgH_2$ catalysts is interesting; should consider expanding this effort in favor of the proposed $NaAlH_4$ work. Also, HRL has tested a whole host of catalysts for this reaction; need to ensure that this work is not being duplicated.
- Reasonable. Get to the bottom of the Ti catalyst story ASAP and get on to the future. Get beyond alanates quickly, as planned.
- The future steps are consistent with the eventuality of a discontinuation of activities on alanate. The alternative hydrides already chosen all deserve potentialities.
- Would be good to coordinate with ST-15 and STP-5 [Brookhaven and Stanford University] projects.

Strengths and weaknesses

Strengths
- Excellent command of analytic tools. A hard-working PI, group and other collaborators. An impressive example of the power of external collaboration.
- The project has adequate expertise and a consolidated network of collaboration of high level.
- High scientific level of PI and collaborators, close coordination and collaboration.

Weaknesses
- Probably trying to do too much at once. This reviewer is a bit confused by the apparent loose and incomplete ends.
- Non-substantial for project in general.

Specific recommendations and additions or deletions to the work scope

- The cancellation of alanates must be the result of an overall evaluation involving all the results achieved in the Program and the joint evaluation in the MHCoE.
- Would be good to extend collaboration with IPHE-country laboratories.
- Suggest moving away from alanate systems much faster.
- Get to the bottom of the Ti catalyst story asap and get on to the future. Get beyond alanates quickly, as planned.

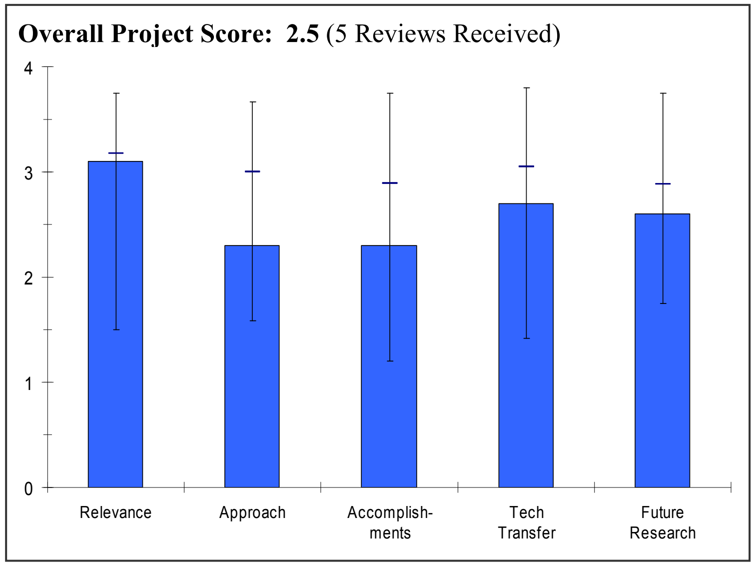
Project # STP-07: High Throughput Combinatorial Chemistry Development of Complex Hydrides

Guanghui Zhu; Intematix Corporation

[Partner of the DOE Metal Hydride Center of Excellence]

<u>Brief Summary of Project</u>

The objective of this Intematix Corporation project is to discover catalysts for metal hydride systems that achieve fast kinetics and high selectivity, thus meeting DOE's 2010 targets for start time (4 s), flow rate (0.02 (g H_2/s)/kW) and refill time (3 min). The approach consists of metal hydrides preparation, combinatorial catalysts preparation, and high throughput screening of catalysts. Work in 2006 is focused on screening catalysts for MgH_2+Si, Li-Mg-N-H, and $LiBH_4$+MgH_2 system dehydrogenation and rehydrogenation. Catalyst screening for $Mg(BH_4)_2$ and $Ca(BH_4)_2$ dehydrogenation may also be studied depending on the synthesis progress at Sandia National Labs.



<u>Question 1: Relevance to overall DOE objectives</u>

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project appears to have relevance to the metal hydride CoE. It's a little hard to tell though because so little information was contained in the poster.
- Work is in support of Sandia and HRL projects within CoE, and thus indirectly is connected and attentive to the President's HFI and DOE RD&D plan.
- The identification of catalysts and new hydrogen storage systems is a critical aspect for reaching the DOE targets.
- Scope of the catalyst search is only applied to well known chemical hydride systems with well known limitations that limits overall probability of success of the project. Combinatorial approach is good.
- Rapid screening important to identifying promising material candidates for further study.
- Supports both MHCoE and CHCoE. [There are 2 separate cooperative agreements between Intematix and DOE; one covering the work with the MHCoE and one covering the work with the chemical hydrogen CoE.]

<u>Question 2: Approach to performing the research and development</u>

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

- The title of the poster is "High Throughput Combinatorial Chemistry Development of Complex Hydrides." From the information contained on the poster, it is difficult to determine if in fact the approach is high throughput or combinatorial. The techniques used in this study are considered proprietary to the members of the CoE.
- The project is very focused toward mostly one area, discovering new catalysts for specific H-storage reactions using combinatorial screening. The second limited area involves some computational efforts to prejudge systems. It is not clear to me how the latter differs from other a priori computational efforts scattered throughout the DOE program.
- The combinatorial approach is a good one.

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

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

- Intematix performed a database search and calculations to identify 15 systems that have capacity of > 6 wt.% and P-T windows that are close to the operating conditions of interest. In addition they showed some screening data for Mg_2Si hydrogenation. No catalysts were found for the conditions of interest. This is not a lot of progress in over a year since the start of the project. There are several milestones between now and September with little indication of where they are in relation to the milestones.
- The results have been limited so far, given the fact the project started in Jan. 2005. To be fair, much time and trouble was spent in setting up the high-throughput equipment and procedures and so progress should be more observable in the next reporting period.
- Despite significant searching, no catalysts have yet been identified for the hydrogenation of Mg_2Si.
- Some success on catalyst search for chemical hydride CoE, but not much support to MHCoE.

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

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

- Collaborations are between the CoE members and appear to be good.
- There are only two collaborations at the moment. SNL and HRL. The CoE concept should make the way for more.
- The project appears to be very well interfaced with the needs and issues of the other projects in the metal hydride CoE.
- Need to have more interactions with CoE members.

Question 5: Approach to and relevance of proposed future research

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

- Most of the work listed as future in fact has very near-term milestones. The collaboration with HRL which may represent the best hope for a significant Intematix contribution to the CoE has a September milestone for catalyst selection.
- Reasonably good, and entirely centered on activities and reactions pioneered by HRL and SNL. That is good and proper, but I have my doubts on the future of the Li-amide system.

Strengths and weaknesses

Strengths
- A needed catalyst screening support for the other CoE partners.
- This is a key activity for the success of a number of approaches in the metal hydrides CoE. The rapid combinatorial screening approach appears to be working well.

Weaknesses
- It appears to be somewhat difficult to perform the in-situ catalyst screening process at the elevated temperature and pressure conditions required of some hydrogen storage approaches.

Specific recommendations and additions or deletions to the work scope

- Intematix needs to be more forthcoming in its presentations.
- Expand support to other CoE partners and other storage systems (e.g., AlH_3, other borohydrides,…).
- The milestone for the identification of Mg_2Si hydrogenation catalysts is June 2006. What happens if no suitable catalysts are identified by that time?
- Why continue with Mg_2Si system?
- None.

Project # STP-08: Synthesis of Nanophase Materials for Thermodynamically Tuned Reversible Hydrogen Storage

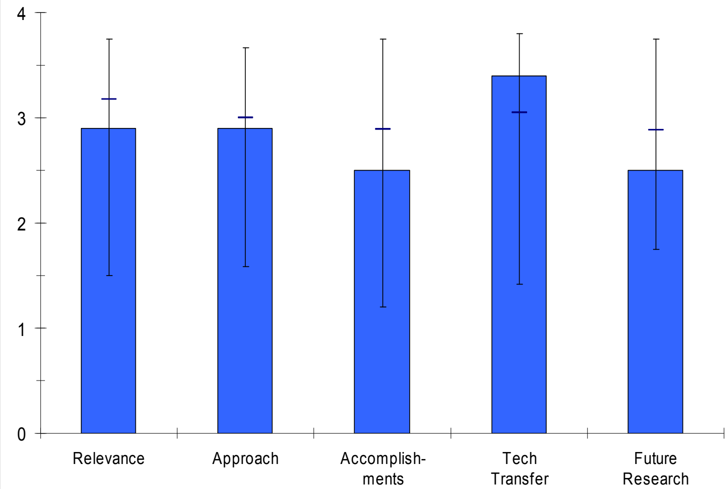
Channing Ahn; California Institute of Technology (CalTech)

[Partner of the DOE Metal Hydride Center of Excellence]

Brief Summary of Project

Nanoscale hydrides and hydride precursors are necessary to understand kinetic limitations, which are ultimately tied to refueling rates. This CalTech project is using a variation of a gas condensation technique to synthesize hydrides with smaller particle sizes and higher purity than those achievable with mechanical attrition.

Overall Project Score: 2.8 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This nano-size approach, if successful, will require that techniques be developed to synthesize larger quantities of materials.
- Very largely addresses the storage needs to achieve the President's HFI and DOE's RD&D plan.
- Clearly focused on what is emerging as the key problem in the sub area of complex hydrides.

Question 2: Approach to performing the research and development

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

- The gas condensation approach is considered to be a good one from the viewpoint of the quality of the nanosized material obtained.
- The project uses a vapor condensation method to produce nanosized particles for increasing the reversibility and kinetics of H-storage reactions, in particular the HRL destabilization reaction $Mg_2Si + 2H_2 \leftrightarrow 2MgH_2 + Si$. The use of vapor condensation is a much-needed tool within the DOE program. I am a little nervous about the very strong focus on the Mg_2Si reaction alone, so far. It may have inherent limits.
- An intelligent approach to the kinetics problem, only real question is if particles will retain their size.
- Stabilization of nano-particles during cycling is not considered.

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

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

- The quality of synthesized nano-size Si particles was very good. No results were presented on the synthesis of nano-size Mg particles.
- Respectable so far, given the rather small funding level for this project. Interesting Si results. The PI is to be complimented for the frank admissions of problems and negative results. Negative results are indeed needed results for the whole picture.
- Made particles and showed they had thin layers of oxide relative to typical materials. Reduced particle size further.

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

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

- The project appears well coordinated with other activities associated with Mg_2Si.
- Excellent collaborations.
- Very well connected. With many participating partners making meaningful contributions.

Question 5: Approach to and relevance of proposed future research

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

- The project should be starting some work on another hydrogen storage material, since it is possible that the hydrogenation of Mg_2Si may not be possible under reasonable conditions, even with the nano-size approach.
- Fine, but should soon move on to other systems than Mg_2Si. That system may turn out to be fundamentally irreversible. It is perhaps not too early to think about the economics of making nanoparticles by vapor condensation into inert gas. Too expensive for large scale vehicle application?
- Plan is good if a bit slow.
- Not clear which systems are proposed to be investigated besides magnesium silicide.
- Why continue on Mg_2Si since it does not appear to be reversible?

Strengths and weaknesses

Strengths
- The gas condensation approach looks like a good way to synthesize high quality nano-size materials.
- Use of a technique other than ball milling to make nanosized materials.
- Well focused on key problem.

Weaknesses
- The quantities of materials that can be synthesized by the gas condensation approach appear to be on the low side.
- A bit too focused on one storage reaction, $Mg_2Si + 2H_2 <-> 2MgH_2 + Si$.
- Program seems to be progressing slowly.
- Limitation to the Mg_2Si systems. Stability of nano-particles under operating conditions is not addressed.

Specific recommendations and additions or deletions to the work scope

- Add another key material for nano-size synthesis and study to this project.
- Expand to other systems where nanosized reactants may have benefit. Start to assess economics and cyclic stability (size retention) of nanosized particles, especially in Mg-based systems.
- Speed-up a go/no-go decision on magnesium silicide approach. Address stability issues for nanoparticles under operating conditions.

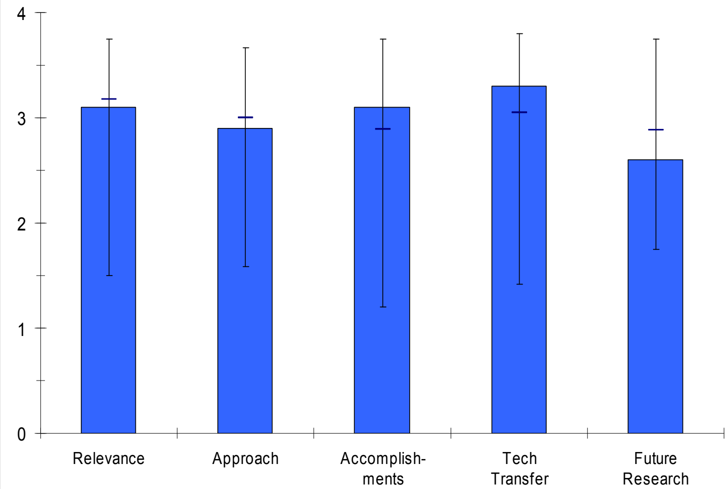
Project # STP-09: Effect of Trace Elements on Long-Term Cycling and Aging Properties of Complex Hydrides for Hydrogen Storage

Dhanesh Chandra; University of Nevada-Reno
[Partner of the DOE Metal Hydride Center of Excellence]

<u>Brief Summary of Project</u>

The primary objective of this University of Nevada-Reno (UNR) project is to determine the effects of gaseous impurities (ppm levels of O_2, CO, H_2O etc.) in the H_2 on long-term hydriding/dehydriding of complex hydrides, and a related secondary objective is to determine the mechanisms of degradation. Research in 2006 is focused on thermodynamic studies, including hydrogen charging/discharging effects, vaporization thermodynamics of precursor materials, and differential scanning calorimetry, as well as crystal structure studies.

Overall Project Score: 3.0 (5 Reviews Received)



<u>Question 1: Relevance to overall DOE objectives</u>

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project indirectly supports the President's HFI and the DOE's RD&D plan. It is not completely clear how some of the diverse activities fit in.
- This program is focused on what will be an area of concern when the kinetics problems are beaten. It is not the most important problem, but it is good that someone is working on problems that will emerge so that the DOE Hydrogen program moves swiftly.
- The objective of the work is to study the effects of gaseous impurities on the capacity and performance of complex metal hydrides. The work at UNR is highly relevant to the MHCoE. Understanding these effects on storage materials is important in the specification of hydrogen quality as a fuel for fuel cell vehicles. Equally important is understand the generation and release of gaseous species upon repeated cycling that may be harmful to a fuel cell.
- There is misalignment with the alloy selected for testing versus the future direction of the MHCoE. The decision on alloy selection should be a consensus among all the principals.

<u>Question 2: Approach to performing the research and development</u>

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

- The project needs to be re-aligned with the ongoing progress in the metal hydride CoE and other related work. There is not enough integration among the relevant projects.
- The project was apparently created to develop a database of experimental knowledge on gaseous impurity effects in various candidate hydrides. This objective is extremely important to solving the on-board storage problem. However, several other activities have seemingly crept into the project. Because this project is rather lightly funded, this will almost certainly dilute the main objective, impurity studies.
- Sound approach to aging, could benefit by sampling gas released form hydride bed to see to what extent impurities were removed, and what purity is going to the fuel cell. This would allow a mass balance on impurities to confirm results, and would give the added benefit of showing how pure the gas going to the fuel cell will be. However, much of the other work is interesting for its own sake, but not very helpful to advance

storage. The various phase transformations and suggestive rate work do not much add to our knowledge of how to store hydrogen. That work would be better done for NSF or BES.
- UNR is studying the effects of pressure cycling on the capacity and performance of metal hydrides, in 2005, Li_3N as a precursor/model compound. In addition to determining the loss of capacity, UNR is also attempting to determine the nature of structural and compositional changes in the material on cycling in order to determine degradation mechanisms. UNR also looks for volatile species released from the system that could be harmful to the storage system or fuel cell system components.

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

This project was rated **3.1** based on accomplishments.

- The technical progress based on the selected alloy is good. The characterization work is very good. The main issue is the pre-mature choice of the alloy. This alloy (and corresponding family of alloys) is subject to potential no-go.
- Good, given the very small funding this new project has received. The impurity cycling on the amide system has been useful. The experiments should be redesigned with selected individual impurities and levels. Is the loss seen with the Li_3N studied due to non-H_2 impurities or NH_3 loss?
- Should check for ammonia levels in exit gas, not only to see if NH_3 loss is exacerbated or diminished by impurities, but also to ensure capacity loss is not due to reaction with N group. Alternately could check N still in sample.
- Excellent progress in understanding a lot of phenomena which are caused by impurities.
- Progress has been quite good for the funding received. Over 1000 cycles have been completed on Li_3N that showed about a 2% decrease in capacity.

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

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

- The alloy selection is out of step with the MHCoE findings.
- Good collaborations in place and to be developed, apparently.
- Adequate and improving.
- UNR has active collaborations with a number of organizations some of which provide a service to UNR to characterize these materials

Question 5: Approach to and relevance of proposed future research

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

- This is a very valuable project. The effectiveness of this project would greatly enhance if there is better coordination on alloys to be studied. Need to address the uncertainties and contingencies.
- The plan has seriously deviated from the main practical objective, impurity/cyclic life quantification, to a confusing variety of other fundamental studies: Li_3N evaporation, crystal structures, DSC studies, lattice dynamics, etc. Why? Aren't many of these other studies going on elsewhere within the DOE/CoE program?
- Future work seems less well formulated. Amides have been largely discounted for hydrogen storage due to ammonia formation. Boron based materials are growing in importance, BH_4 based hydride testing seems more valuable than the explicitly planned work.
- Should be focused on the impurity effects in systems with [on] more big scale.
- A comprehensive research plan is in place for 2007. The plan includes impurity studies, high-pressure cycling studies, in-situ hydriding/dehydriding x-ray diffraction and neutron diffraction studies, and vapor pressure studies. The plan almost seems too ambitious for the budget.

Strengths and weaknesses

Strengths

- Good technical capabilities. Good characterization techniques.
- Impurity/cyclic life studies very important to storage success.
- Important work and many strong tools available.
- Project is well designed and integrated with other research in frame work MHCoE.
- Very good suite of analytical equipment that is augmented by collaboration with NIST and ANL neutron sources.

Weaknesses

- There is a significant disconnect between the substantial work done versus the future direction of the MHCoE candidate alloys. Need stronger coordination with the MHCoE.
- Too many other diversions.
- Diffuse focus, should focus strongly on more work in the key area - degradation of storage material.
- It is not clear how close the collaboration between UNR and the MHCoE. As the CoE shifts focus, so should UNR.

Specific recommendations and additions or deletions to the work scope

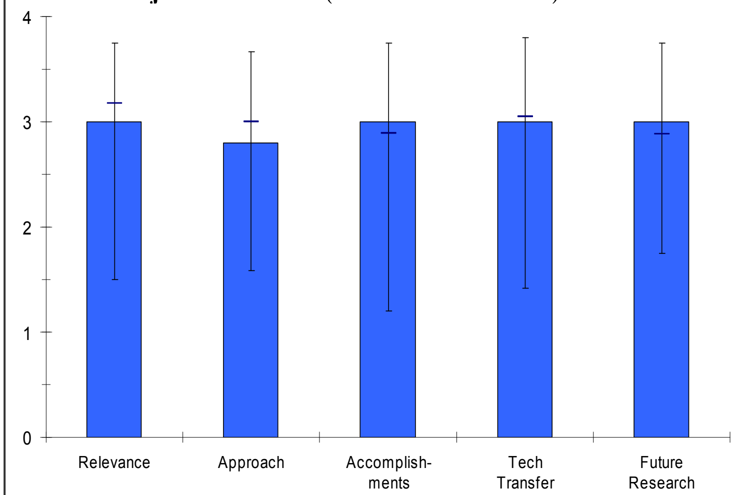
- The project performed substantial work on an alloy that is being discontinued for further work. This project should be considered for re-alignment. The future work scope does not match the future direction of the MHCoE. The proposed could be substantially rationalized. Some of the work could be allocated to SwRI. This project should be closely monitored for its effectiveness.
- Consider virtual elimination of all activities other than impurity/cyclic life studies. There is a widespread dearth of impurity cycling data on most modern storage materials, including alanates, amides, destabilized hydrides, etc. Such data is important to practical engineering considerations, as well as developing trends that may be applicable to other future materials. Suggest more funding for the principal (impurity) effort.
- Focus on hydrides more likely to see use (boron based), and modify apparatus to look for degradation gases in effluent stream, or at least look for loss or conversion of all elements in the material.
- All is good. I recommend expanding the investigation of impurities effects on properties of scales "systems with additional financial support.
- UNR should be strongly encouraged to look for evolution of fuel cell-harmful species upon cycling complex metal hydrides, particularly those that contain N. The experiments on impurity tolerance should be conducted one impurity at a time at various concentrations to enable a fuel quality specification for fuel cell vehicles. This information should be available for consideration by SDOs [Standard development organizations] such as SAE who are attempting to define an international standard for hydrogen fuel quality for fuel cell vehicles.

Project # STP-10: Chemical Vapor Synthesis of Nanocrystalline binary and complex Metal Hydrides for Reversible Hydrogen Storage

Zak Fang; University of Utah

[Partner of the DOE Metal Hydride Center of Excellence]

Overall Project Score: 3.0 (5 Reviews Received)



Brief Summary of Project

The objectives of this project are to discover new solid hydrides that meet reversibility and kinetics requirements, to develop a chemical vapor reaction process (CVS) for synthesis of nanosized solid metal hydrides, and to demonstrate the effectiveness and unique properties of nanosized solid hydride materials for hydrogen storage. This year, the University of Utah has demonstrated the feasibility of synthesis of nanosized metal and metal hydride powders using CVS reactors and discovered that a combination of lithium hexaaluminum hydride (Li_3AlH_6) with lithium amide can produce a system that has 7% reversible hydrogen storage capacity at temperatures below 300°C. An on-going effort is directed toward PCT analysis of this material system. The University of Utah team has also improved and developed a unique high energy, high pressure reactive milling process for synthesis and processing of complex metal hydride material systems.

Question 1: Relevance to overall DOE objectives

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

- This project is targeted at discovering and producing new nanocrystalline reversible hydride materials.
- The project addresses some key objectives of the DOE plan.
- Relevance of ideas is good, but would be higher if the decomposition temperatures could be lowered.
- Materials selection is only limited to alanates and amides that is questionable based on prior work.

Question 2: Approach to performing the research and development

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

- The project approach is considered to be good. The CVD synthesis approach looks promising and is potentially scaleable to larger material quantities.
- The project addresses a new class of hydrides including a proprietary storage material and different technical preparations.
- Not clear exactly how the materials discovery was made: Was it simply a guess? Intuition? Nevertheless, the discovery is interesting.
- Lack of thermodynamic heat balance considerations ("useable" storage capacity). Stability of nano-sized hydrides upon cycling is not addressed. Gas phase analysis (presence of ammonia, etc) has not been presented.

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

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

- A significant amount of results have been obtained. Hydrogenation and dehydrogenation temperatures are relatively high.
- The project has already analyzed and developed novel systems with over H_2 7 wt.%.
- Very impressive amount of work in the first year of a project with little funding! Demonstrated a reversible >7 wt.% storage reaction. Novel idea demonstrated (w/ combination of Li_3AlH_6 and $LiNH_2$), and next year will refine some of these ideas. The processing and thin film growth ideas and results look interesting as well. Temperatures of decomposition still too high (~200-350°C). Why are the temperatures of decomposition for the second desorption so much higher (all the way to 350°C) than the first time (where the reaction seems to be largely complete by 250°C)?
- For the amide work at high temperatures, it's quite likely that there is some decomposition to NH_3.

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

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

- Technical collaborations are taking place in a number of areas.
- The project has some collaborations under development in the coming year.

Question 5: Approach to and relevance of proposed future research

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

- The future work looks reasonable.
- The future steps are consistent with the already achieved results.
- Main approach should be to (a) understand the mechanisms for this reaction, and (b) to try and overcome the obstacle of high temperatures of desorption, and understand the thermodynamics of this two-step reaction (are both steps high-temperature steps, or are one or more simply kinetically limited?). Also, need to obtain and present data on the kinetics of rehydriding. Also, MS should be performed to verify the levels of NH_3 and other species being evolved during this reaction.

Strengths and weaknesses

Strengths
- A good, solid project. A lot of progress is reported, despite the relatively small funding on the project. The CVD approach to materials synthesis looks promising. The high pressure hydrogen atmosphere milling is interesting.
- The novel materials already developed are a good scientific and technological basis.

Weaknesses
- The thermodynamics of the new lithium-based material needs to be determined.

Specific recommendations and additions or deletions to the work scope

- None.
- Materials selection for future research is questionable based on prior art. Suggest reconsidering project scope to move away from alanate/amide systems. Incorporate a quick go/no-go decision on Mg_2Si systems.

Project # STP-12: ORNL's Hydrogen Storage Research in support of the DOE National Hydrogen Storage Project

Gilbert Brown, Metal Hydride Center and Dave Geohegan, Carbon Center; Oak Ridge National Laboratory

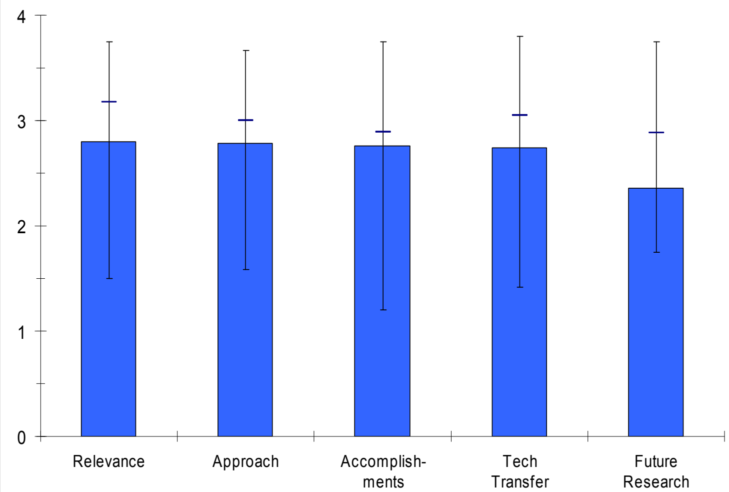
[Partner of the DOE Metal Hydride and Carbon Materials Centers of Excellence]

[Oak Ridge is a partner in both the Metal Hydride and Carbon Centers of Excellence. Both projects were covered under this single review.]

Brief Summary of Project

Oak Ridge National Laboratory (ORNL) has two major areas of research on hydrogen storage. As part of the carbon-based hydrogen storage CoE, ORNL is attempting to control the synthesis and processing of a novel form of carbon – single-walled carbon nanohorns – as a medium with tunable porosity for optimizing hydrogen storage. For the metal hydride CoE, ORNL is developing solution-based synthetic methods for the preparation of complex anionic materials and amides of light elements for reversible storage of hydrogen.

Overall Project Score: 2.7 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Low capacity materials. High cost carbon fibers produced by laser desorption or (maybe) arc synthesis. Alanates still low capacity compared to targets.
- Reversibility is key.
- This project covers ORNL activity in two centers of excellence - carbon and metal hydride. The two areas of research folded into a single presentation makes it difficult to review each separately when composite scoring is necessary.
- Project focused on working towards DOE targets and well aligned with the overall RD&D objectives.
- These are complicated and bulky molecules that are unlikely to achieve any storage density targets.

Question 2: Approach to performing the research and development

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

- Good control over tube & horn properties; good characterization capabilities. Volatile hydride concept interesting, but capacity suffers. Synthesis not so important. Reversibility needs to be addressed more directly.
- A novel carbon-based material - nanohorns- is being developed for potential hydrogen storage and is based on existing experience and expertise at ORNL. In addition, the synthesis of metal hydrides is being studied. The approach taken for each of these very different areas is generally will thought out and effective. Unfortunately the ability of the subject materials to meet minimum requirements for gravimetric hydrogen storage density appears to be very low.
- Well thought approach taking advantage of long expertise in synthesis and of access to a range of characterization techniques. Working interactively with other members of the CoE to understand and optimize these materials.

- [Carbon Project:] The project needs to be integrated with other carbon nanotube research and modeling work ongoing within the carbon CoE. [Metal Hydride Project:] More integration with complex hydrides solid synthesis efforts within the CoE needed.
- [Carbon project:] PI is trying to synthesize a complicated molecule or cluster of molecules and then decorate (without control on location) with precious metals. This process sounds incredibly expensive and the nano horn clusters can't possibly be robust enough to maintain form and cluster shape.

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

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

- Good synthesis and characterization results; poor capacities of fibers. Good alanate synthesis; little progress on reversibility.
- Good progress has been made supporting the project's stated goals and objectives. Unfortunately the materials being studied alanates and nanohorns appear to have storage capacities that fail to meet even the minimum system requirements.
- [Carbon project:] Need collaboration with modeling efforts within the carbon CoE to estimate properties/ evaluate the feasibility of enhancing hydrogen physisorption. Good progress demonstrated in relatively short time. Effective synthesis-processing-characterization loop for growth of tunable porosity, surface area and graphitic structure SWNT and metal decorated SWNT with optimized hydrogen storage capacity.
- Materials have been synthesized and tested but results are poor.

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

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

- Little evidence of external collaboration.
- Strong interaction in both CoEs.
- Established collaboration with other members of the CoE in the form of sample performance characterization.
- [Both projects:] Need to strongly interact with experimentalists and theoreticians within the respective CoEs.
- Didn't list any key partners who are experts in the field.

Question 5: Approach to and relevance of proposed future research

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

- [Project A, Carbon Center] Need to address low temperature tube synthesis. Assume work will stop in the area of carbon nanotubes; go/no-go decision stops nanotube work. Need alternate paths for Project B [Metal hydride center], i.e., if new material isn't reversible, what new approaches will be pursued to attain reversibility.
- Future work should carefully filter out work on materials that have little or no potential to meet storage system requirements (such as alanates and/or nanohorns?) Then focus remaining resources on new materials discovery and/or materials that show promise of meeting targets (amides/imides?).
- Sufficiently planned future activities, building upon recent progress and existing expertise.
- [Carbon Project:] Focus on basic research for effects of optimized properties of the nanohorns. [Metal Hydride Project:] Strong collaboration within the solid state development group of complex hydrides needed.
- Why make more when the samples provided have low storage capacity.

Strengths and weaknesses

Strengths
- Good synthesis, characterization, and modification capabilities.
- Strong materials synthesis and testing capability at ORNL.
- Strong synthesis and processing background; good foundation for sound R&D.
- [Carbon project:] Synthesis capabilities of nanohorn with no apparent graphitic impurities and some tunable properties. [Metal Hydride Project:] Synthesis trials of the low stability complex hydrides with new routes.

Weaknesses

- High dollar, low capacity materials. Alanate work has limited scope. Characterization and synthesis are good, but needs to be more focused on solving reversibility issues with some Edisonian component to try a number of approaches. Some value to understanding why an approach doesn't work, but need ability to try multiple approaches.
- Materials selected for initial study appear to have limited potential to meet DOE system targets.
- Quite challenging field which may not deliver within the tight timescale set for reaching the storage system targets.
- [Carbon project:] Need to integrate theoretical calculations to help optimize properties.
- Materials use expensive catalysts haphazardly placed on the molecules - no control compared to the MOF approach. The materials are bulky and contain many dead spots for storing hydrogen. It is unlikely that these materials will ever achieve reasonable storage densities.

Specific recommendations and additions or deletions to the work scope

- Alanates—attack reversibility more aggressively.
- Refocus future work on new materials discovery.
- Keep close track of progress towards targets and set milestones. Project (A) [Carbon Center] In view of the tight schedule, intensify efforts for understanding the supercritical hydrogen adsorption mechanism for the development of optimized SWNTs and composites with tunable pore sizes, and for getting better insight in their metal decoration with very fine metal clusters in high loadings. Consider added value/feasibility of addressing synthesis scale-up and operability issues, much earlier in the program than foreseen. Comment: Under Project (B) [Metal Hydride Center] it is mentioned: "Percentage of effort in remainder of FY06 and FY07: 10-15% catalyzed alanates, 40-45% metal borohydride, 40-45% metal amide/imide (M-N-H)" contrary to what it was announced during the MHCoE presentation where the effort for a/imides was said to be reduced down to 5% and the work on alanates to be suspended. This may point to the need for better communication within the MHCoE.
- [Carbon project:] More focus on optimization of properties through using modeling efforts within the CoE. [Metal hydride project:] Integration of efforts with solid state synthesis ongoing work within the CoE and also with basic research.
- This project should be terminated. The results demonstrated are extremely low and the synthesis methods are complicated. This project is academic at best and should strongly be considered for termination with the SWNT go/no-go decision. Expensive catalysts are being used and still delivering low results. This work has been done previously and is unlikely to provide valuable knowledge or insight for breakthroughs in future materials.

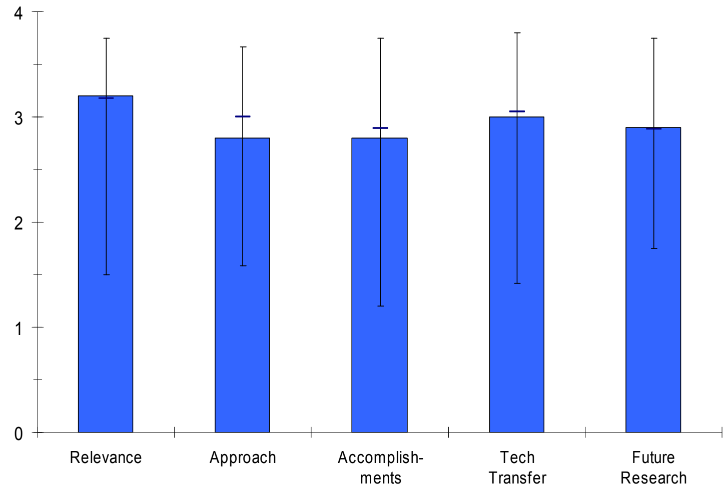
Project # STP-15: Conducting Polymer as New Materials for Hydrogen Storage

Alan MacDiarmid; University of Pennsylvania
[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

The goal of this University of Pennsylvania project is to identify and demonstrate the conducting polymer species previously reported in the literature to give ~8 wt.% hydrogen storage. The project involves confirming that 8 wt.% hydrogen storage is achievable in doped forms of organic conducting polymers, polyaniline and polypyrrole; determining optimum polymer preparative methods, chemical composition, oxidation state and polymer crystallinity and morphology to give quantitative optimum conditions of hydrogen adsorption and desorption; and investigating hydrogen storage by other known types of organic conducting polymers in their semiconducting and metallic forms.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Potential game changer material with uncertainty in results.
- If conducting polymers, or any low-cost polymers, were discovered that could reversibly store and release significant quantities of hydrogen, then this would constitute a major breakthrough in hydrogen storage materials.
- The project addresses most key objectives of the DOE plan.

Question 2: Approach to performing the research and development

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

- Need to address the material handling and purification and its effect on the storage capacity.
- Good approach except use of TPD for H_2 quantification. Use of PCT or TGA seems indicated for materials that can be made in gram quantities.
- This project is providing a thorough investigation to determine the potential of conducting polymers as hydrogen storage materials.
- The project presents an interesting development and application of polymers. The project is well concentrated in solving and verifying chief performances.

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

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

- The current results are affected by the materials' quality. However, the most recent literature results are very promising.
- Good progress in material characterization and initial results obtained. Seems to confirm that materials do not take in much hydrogen.

- Despite thorough analysis, the results of the Korean work that reported 8 wt.% hydrogen in conducting polymers have not yet been reproduced.
- The PI did not present all the results. Only part of the experimental activity is presented, showing interesting progress.
- Although a negative result was obtained, this work is important in attempting to verify a previous result. Null result not exciting, but still important.

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

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

- Well connected.
- There appears to be good collaboration with NREL on this project.
- The project has close collaboration with key partners.

Question 5: Approach to and relevance of proposed future research

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

- Plan seems appropriate, but again TPD seems to be a suboptimal method to determine H_2 uptake.
- There is a go/no-go decision point at the end of FY2006 based on establishing greater than 1 wt.% hydrogen storage in polyaniline.
- The future steps are consistent with the already achieved results.
- Suggest that the impressive capabilities of this laboratory be applied to other new, innovative materials

Strengths and weaknesses

Strengths
- A novel and innovative idea. Strong competency in the field. The most recent literature results suggest 3 wt.% capacity at room temperature.
- Team is well poised to address question.
- The PI is a world-renowned researcher on conducting polymers and received the Nobel Prize for his work in this area.
- The basic expertise and the established collaborations can guarantee successful results.

Weaknesses
- The current results are marginal.
- TPD is not the best way to measure H_2 uptake. Go/No-Go point seems awfully low for a hydrogen storage medium, consider raising to at least 2% in unmodified, room temperature, polymer.
- It may be that the reported Korean work that indicated significant hydrogen storage in conducting polymers was in error, and that conducting polymers are not capable of storing any significant quantities of hydrogen at room temperature.

Specific recommendations and additions or deletions to the work scope

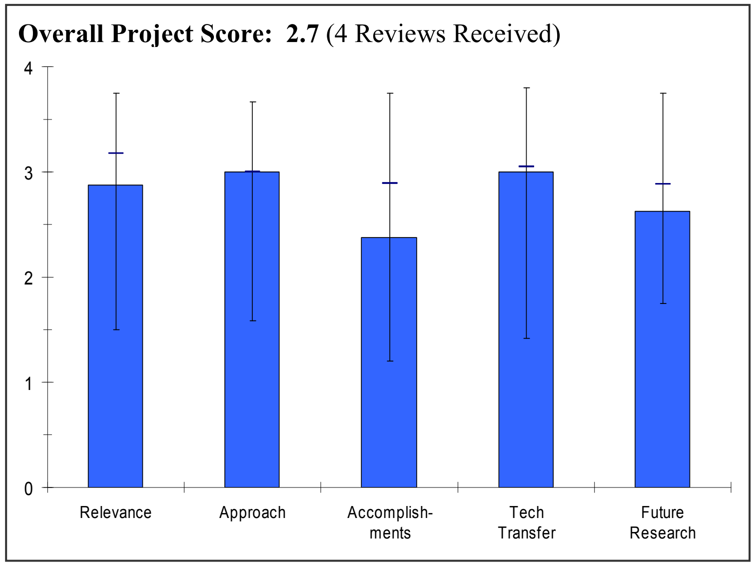
- Need to leverage the activities with the most recent literature results.
- Change to PCT measurements of hydrogen. At the very least confirm with volumetric or TGA in a large sample. If hydrogen is taken up, obtain delta H using PCT curves at various temperatures.
- Should it be determined that conducting polymers are not suitable vehicles for hydrogen storage, this project should be continued, but redirected into a more promising area of research that builds on the research interests of the PI. Based on the slides that were provided during this review, the PI appears to be interested in investigating the production of hydrogen from ethanol via an electrical discharge route.

Project # STP-16: Enhanced Hydrogen Dipole Physisorption

Channing Ahn; California Institute of Technology
[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

The goal of this California Institute of Technology project is synthesis of high surface area physisorbents (in order to achieve gravimetric densities of 7.7 wt.%) with tailored pore size (to reach volumetric densities of 58 gm/liter) and high adsorption enthalpies (ambient temperature operation).



Question 1: Relevance to overall DOE objectives

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

- Aligned well, though gravimetric goal not clearly dealt with. Still very valuable work.
- Aerogels and activated carbon are materials that have to be studied. Nevertheless the first results do not look promising at all.
- There is a possibility of getting close to the DOE 2010 targets with this approach, albeit at a temperature of 77K.
- Potentially physisorbents can provide higher storage densities than compressed hydrogen.

Question 2: Approach to performing the research and development

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

- Approach is appropriate and probable to lead to progress.
- The basic approach of increasing the surface area per unit volume, and increasing the adsorption enthalpy by additions such as boron to carbon aerogels is sound.

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

This project was rated **2.4** based on accomplishments.

- Showed that one can to some extent overcome area limitations with catalysts; 38 g/L in activated carbon.
- A value of 5.4 wt.% hydrogen at 77K with an activated carbon is reported.
- Project did not demonstrate a significant progress towards meeting DOE targets versus prior art.

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

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

- Nicely connected.
- There appear to be good collaborations with other members of the carbon CoE.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.
- Appropriate plans, though unclear how the 1.1 nm pore size will be achieved.
- The future plans look very general and specific targets must be fixed.
- The proposed research on boron additions to carbon aerogels is good.

Strengths and weaknesses

Strengths
- Sound understanding of physisorption processes in materials. Interesting analyses of volumetric effects in high porosity, adsorbing materials.

Weaknesses
- It may be quite difficult to achieve the optimum pore size of 1.1 nm in the carbon-based materials. There does not seem to be a clear route to get to this objective.

Specific recommendations and additions or deletions to the work scope

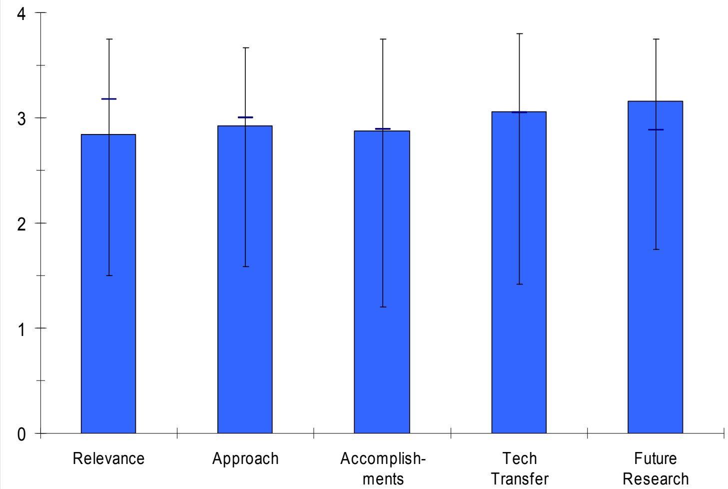
- Establish a route with backup plans to achieve 1.1 nm pores with maximal surface area to mass ratio.
- None.
- Explore higher pressure operation (500-700 bar).

Project # STP-17: Optimization of SWNT Production and Theoretical Models of H_2-SWNT Systems for Hydrogen Storage

Boris Yakobson; Rice University

[Partner of the DOE Carbon Center of Excellence]

Overall Project Score: 2.9 (7 Reviews Received)



Brief Summary of Project

Rice University is developing predictive models of materials structures interaction with hydrogen, in order to optimize their makeup for storage and assess the gravimetric and volumetric capacity. Rice will provide recommendations for the synthetic goals (e.g. diameter, type and organization of SWNT). In 2006, Rice is exploring full utilization of physisorption by van der Waals dispersion forces, using HIPCO method to produce nanotubes of preferred diameter (and length) for better hydrogen adsorption, and performing quantum mechanical computation for precise description of van der Waals attraction.

Question 1: Relevance to overall DOE objectives

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

- Nanotubes still low capacity materials.
- The methodology is interesting but the materials are not very promising for hydrogen storage.
- This project has two major thrusts - modeling of the hydrogen storage capability of single-walled nanotubes (SWNT) and enhanced production of SWNTs.
- The program has a strong experimental group and a strong theory group. More interaction between experiment and theory should be encouraged regarding focusing the program along more relevant directions in the cloning project.
- The project assists in realizing and quantifying the interactions of hydrogen with materials. This could play an important role in directing research resources in a later stage.

Question 2: Approach to performing the research and development

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

- Good mix of QM and classical approaches to get results for large systems.
- The milestone plans and roadmap for this project is ambiguous. Good level of integration within the Rice program and a few other projects. The threshold for go/no-go decision should be transparent.
- The strategy of this theoretical investigation is novel and useful for the storage problem in C-based materials.
- The approach proposed to address both of the technical objectives of this project is sound and is based on utilizing the experience and capabilities at Rice.
- Approach is very good. Theory group is addressing important issues for enhancing uptake and for increasing the temperature for efficient uptake. Consideration of spillover effect should be encouraged.
- The project needs to be integrated with other storage work done within the carbon CoE.
- Same approach as all the other projects. No good explanation is given on how these modeled nanostructures will be formed. Isn't this work being done by Jim Tour and Ralph Yang already?

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

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

- Interesting results for metal decorated tubes. Classical force field for H_2 adsorption onto SWNTs.
- It is difficult to distinguish the accomplishment of this specific project. The work is to address theoretical concepts on H_2-SWNT.
- Interesting results for carbon nano-foam.
- Significant progress has been made in each area this year. SWNT production of >1 g/hr has been demonstrated of extended operational period. Model for predicting hydrogen storage potential for carbon nanostructures has been developed and model output is being assessed for accuracy. Initial storage capacities for these structures appear to be well below the system targets. Additional model improvement and refinement is planned.
- Since goals of the program are for increased hydrogen storage, more progress and emphasis on above 77K storage capability needs more consideration.
- Focus on modeling is needed as a start instead of the HIPCO CNTs generation process. Theory results should be directing the experimental efforts which should come in a later stage. Validation of the model was not addressed.
- Results match reports from 10 years ago. No novel method or results reported. Only thing possibly novel is the carbon foam? What are results and how is it made?

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

This project was rated **3.1** for technology transfer and collaboration.

- Good within university. May want to work more with CoE and others.
- The collaboration with the H_2 spillover work is very good.
- Collaborations are in place with NREL and Air Products and should be expanded to other partners in the carbon-based CoE. Benefits would flow in both directions - to Rice for additional assessment of the modeling effort and to the other partners to explore performance parameters for nanostructures.
- Collaboration with group at Michigan is noted as a plus and collaboration between this experimental group and industry is strong. Collaboration between Yakobson and Tour on the control of intertube spacing is good. Collaboration between Yakobson and Tour on cloning could be better.
- Need to strongly interact with many experimentalists and theoreticians within the CoE.
- Collaboration seems weak at best. What has been delivered to Air Products or NREL?

Question 5: Approach to and relevance of proposed future research

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

- Calculations for spaced tubes look interesting. Will this work be discontinued if go/no-go decision decides SWNTs are not an option.
- Need to address the approach to 7-8 wt.% capacity at cryo conditions. Need to address the route to room temperature materials sorbent development.
- The targets are clearly set and the accomplishment of those will be important.
- Planned research appears reasonable and proper to build on past results. Emphasis should be on model refinement and use to identify structures with the highest potential for hydrogen storage. Then small scale productions of these specific structures should be pursued to validate model findings.
- A good part of future work is with Yang at Michigan. Collaboration between Tour/Hauge and Yakobson to determine intertube distance is a good project. Consideration of other factors in hydrogen storage besides uptake capacity should be considered.
- Need to focus on model optimization instead of the HIPCO reactor.
- The metal dopant and spillover mechanisms are already being worked on. What new knowledge are they contributing? Why are they not partnered with Ralph Yang of U of Michigan to work on spillover concepts.

Strengths and weaknesses

Strengths
- Solid computational work. Good approach to large scale simulations. Nanotube expertise--synthesis and characterization.
- High competency in the field. Good utilization/synergy of the resources within the national laboratories. Powerful analytical techniques and independent analysis.
- Strong SWNT capabilities at Rice coupled with extensive experience and expertise in the field.
- Tour/Hauge is a strong synthesis group and they keep a good, reliable experimental program going. Yakobson is a strong theorist on mechanical properties and is doing a good job with the nanotube separation problem.
- Theory is crucial and needed to direct materials design and results validation. Approach captures main key properties affecting H_2 physisorption.

Weaknesses
- May need more collaboration with others doing similar work. SWNTs still low capacity materials.
- Need to address the approach to 7-8 wt.% capacity at cryo conditions. Need to address the route to room temperature materials sorbent development.
- More emphasis should be given to increasing the temperature where hydrogen can be bound to carbon. Metal particle addition may not be enough. Too much emphasis is given in experimental program to controlling nanotube chirality during growth for hydrogen storage.
- HIPCO process should come in a later stage or be part of other projects. No clear link exists between the theory work and the HIPCO process.

Specific recommendations and additions or deletions to the work scope

- Focus on search for new structures/geometries that maximize storage potential – Continue production improvements at a low effort level until scale-up is justified - production of SWNT structures with low storage capacity serves little purpose.
- The budget increase from $75K to $175K should provide money for a postdoc and a graduate student to address the design of a functionalization strategy for increasing hydrogen storage and for studying the spillover effect.
- More focus on model modification and deletions of current HIPCO process.
- Furthering the model to include other materials and dynamic modeling consideration.
- The results are very poor and PI doesn't present any convincing arguments as to how this will be overcome. These materials have been poor performers and heavily researched in the past. It is unclear what value if any this project is delivering.
- Concentrate on carbon foam, drop everything else. [According to one reviewer].

Project # STP-18: Development of Carbon-Based Materials and Characterization of Hydrogen Adsorption by NMR

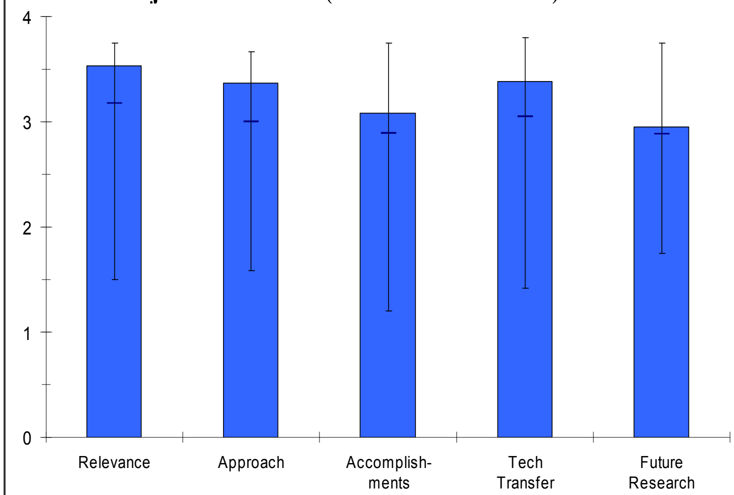
Yue Wu; University of North Carolina

[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

The University of North Carolina is using NMR techniques to support team members of the DOE carbon CoE in developing reversible carbon-based hydrogen storage materials with 7 wt.% materials-based gravimetric capacity, with potential to meet DOE 2010 system-level targets. In 2006, this project is conducting NMR measurements of hydrogen adsorption in boron-doped carbon nanotubes and boron-doped graphite, investigating if boron atoms are incorporated in the framework of nanotubes and graphite, and investigating effects of doping treatment on hydrogen adsorption in polyaniline.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project, under the umbrella of the carbon CoE, is well aligned with the DOE R&D plan.
- This project is intended to develop an NMR characterization tool for in-situ measurement of hydrogen storage in nanostructures.
- The reliable evaluation of hydrogen has been a contentious problem for the past ten years, and the work done by this group is impressive as a reliable measurement tool. However, more emphasis should be given to comparison of hydrogen capacity measurements between the NMR and other measurement techniques.
- Any analytical technique for measuring hydrogen storage in determining where, how many bonds are etc is always a valuable tool to the community.

Question 2: Approach to performing the research and development

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

- Good approach, involving a technique complementary to existing adsorption capacity measuring methods, giving also information on microscopic structures and dynamics. It supports the CoE members in developing reversible carbon-based hydrogen storage materials.
- Straightforward modification and improvements to existing NMR facility is planned to allow observation of hydrogen in nanostructures at pressures up to 100 atm.
- Approach is very promising from a scientific standpoint because it gives not only a measure of hydrogen uptake, but a determination of the importance of various binding sites as a function of temperature and pressure, once these aspects of the NMR capabilities of this group are implemented.
- Use of NMR technique to support CoE in developing carbon-based hydrogen storage materials.
- Is this setup only suitable for boron doped carbon structures?

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

This project was rated **3.1** based on accomplishments.

- Project appears on schedule—the technique seems to be selective enough to identify promising adsorption sites and measure site-specific H_2 adsorption isotherms in B-doped carbon materials considered by the CoE.
- The NMR facility improvements have been completed and initial measurements on provided samples have been conducted successfully.
- Needs more collaboration with other projects in the program to utilize/enhance these capabilities. This fundamental research type of work can be used to validate and understand results generated within the CoE.
- The progress of this group has been impressive with the boron doped nanotubes. The calibration standard has been developed and tested. Different binding sites have been identified and progress made in establishing some relative importance in binding. Preliminary work has been done on doped polyaniline with controversial results that could be important. More progress is needed in comparing the hydrogen uptake measurements by this group using NMR and other groups using other techniques.
- Not enough was presented on the success of the technique to identify where H_2 is. Is this a project to test NMR under various conditions and materials or to determine if boron doping is effective?

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

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

- Good interaction with members of the CoE involving mainly sample characterization work and comparison of in-situ high pressure NMR adsorption data with other techniques.
- Collaborations are in place with partners in the carbon CoE. This project will provide valuable support to the CoE partners who are developing new storage materials.
- More interaction is advised with other members within the CoE.
- A strong point of this program is not only the promise of the method for characterizing many types of samples, but also the possibility of working with many collaborators who study a diverse ensemble of different materials.
- Seems to be involved with good partners. Collaborations should increase if tool is seen as valuable by other PIs.

Question 5: Approach to and relevance of proposed future research

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

- Proposed work is sufficiently planned, builds upon present experience and expands current studies - very much dependent though on the plans and progress of the material suppliers who are the co-workers from the CoE. May need to also incorporate a more systematic study of errors associated with the technique.
- Plans for future work are a reasonable mix of improvements to the characterization tool and continued support for CoE partners.
- Start working on nanohorns and other carbon materials as soon as possible (sooner than proposed in the plan).
- Proposed research overall is very interesting and will advance the program. Since many of the groups focus significant effort on hydrogen adsorptions at 77K, it would be beneficial for this group to develop capability to make NMR measurements at 77K soon, consistent with their future plans.
- H_2 adsorption measurements of the boron doped systems should be carried out soon to show their real potential. There is no point of perfecting the doping techniques if the potential of achieving high RT adsorption is not there.
- Again the focus should be on NMR, not boron doping.

Strengths and weaknesses

Strengths
- Selectivity of the characterization tool.
- Extensive experience and expertise in NMR characterization.
- NMR in-situ measurements capabilities.
- The group has been developing a complementary method for hydrogen adsorption studies with the ability to obtain binding energies for specific sites. The technique can be applied to many kinds of samples across several programs. The group is aggressive in seeking out groups to provide samples for them to measure and interpret.
- Good techniques and lab facilities.

Weaknesses

- Limitations of the technique. Work program strongly dependent upon developments in the work of the material suppliers/partners in the CoE.
- Needs more collaboration and integration within the CoE.
- Many groups working on hydrogen adsorption on carbon based systems and other physisorbed systems are focusing their efforts on hydrogen uptake at 77K. This NMR group should put effort in gaining measurement capability at 77K, as outlined in their planned future work.
- Program is not focused in where it is going or what it intends to accomplish.

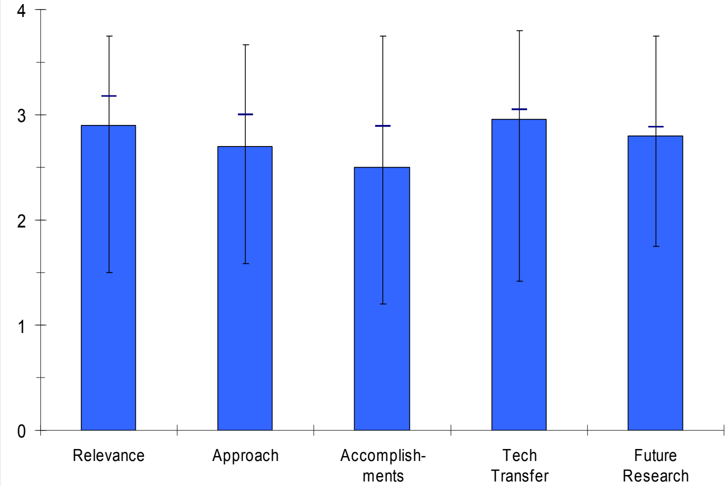
Specific recommendations and additions or deletions to the work scope

- Consider including in the work program a more systematic study of the sources of errors in the measurement technique, and validating measurements, before proceeding further to more challenging materials.
- Consider offering characterization support to other organizations in the DOE Hydrogen Storage Program.
- Applying the NMR measurements to other carbon based materials.
- The group should put emphasis on developing the low temperature capability and should put emphasis on comparing their hydrogen uptake measurements with those made at 77K by other techniques. This would give more confidence all around in the reliability of the various techniques.
- Needs to explain for which hydrogen storage materials this technique would not be suitable to make measurements.

Project # STP-19: Synthesis of Small Diameter Carbon Nanotubes and Mesoporous Carbon Materials for Hydrogen Storage
Jie Liu; Duke University
[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

Overall Project Score: 2.7 (5 Reviews Received)



Goals of this Duke University project include demonstrating small diameter single walled carbon nanotubes with potential to meet the DOE 2010 goal in hydrogen storage properties, as well as demonstrating the storage potential for mesoporous carbon materials with metal loading to meet or exceed the DOE 2010 goal for both gravimetric and volumetric capacity. Work is underway to understand the effect of diameters of nanotubes on their hydrogen storage properties; develop a method to precisely control the diameter of the produced nanotubes; understand and demonstrate the effect of metal loading on nanotube on the hydrogen storage properties; synthesize mesoporous carbon materials with high surface area; and study the effect of metal loading on mesoporous carbon on the hydrogen storage properties.

Question 1: Relevance to overall DOE objectives

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

- Good alignment to the overall DOE objectives working under the umbrella of the CoE.
- Effect of CNT diameter on storage capacity is already known. It is irrelevant since all nanotubes have poor storage capacity regardless of diameter.
- This project is obviously aligned with the President's hydrogen vision and the RD&D objectives. Storage is key to the hydrogen initiative and the carbon-based option is a part of the overall effort. However, it seems obvious that this option is further behind in its science base than the other two options being pursued and it is not clear that it can get much beyond the 2006 target of 6 wt.%.

Question 2: Approach to performing the research and development

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

- Systematic approach assisting in answering fundamental questions related to hydrogen storage in carbon materials; developing novel synthesis techniques that are able to yield tailored SWNTs for high hydrogen uptake.
- Nothing novel is being proposed?
- Looks good.

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

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

- Significant progress was demonstrated. Highlights of this work include the understanding of the relation between the carbon feeding rate and the diameter of prepared nanotubes and the development of a simple method for metal nanoparticles decorating of these materials.
- Nothing novel has been accomplished.
- It is difficult to understand what is accomplished during the period. 33% completion is noted in the summary slide. The tasks and specific objectives appear amorphous.
- Given the limited resources, this project could be better served if narrowed to one subject instead of 3.
- Doing extremely well.

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

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

- Extended on-going collaboration with a number of partners from the CoE concerning the material characterization.
- Are they really collaborating?

Question 5: Approach to and relevance of proposed future research

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

- Sound future plans that include the expansion of work to bulk synthesis, upscaling and systematic study of the impact of metal decoration on carbon materials hydrogen storage capacity.
- Based on other people's assumptions. This work has been done several times before without success.
- Logical progression.

Strengths and weaknesses

Strengths
- Novelty of the technique and fundamental understanding of the control mechanism of CNT diameter through carbon feeding rate variation.
- Knowledge and hard work.

Weaknesses
- Feasibility and cost considerations related to upscaling.
- The accomplishments/results are difficult to understand. Need to improve the presentation clarity by defining terms and baseline. Need to clearly state the baseline and benchmarks. The dispensing heat transfer requirement for cryo-compressed system at 100K is not defined or addressed (e.g., how much energy is required to cool room temperature hydrogen to store at 100K?).
- None obvious from the poster presentation.

Specific recommendations and additions or deletions to the work scope

- Concentrate efforts on the demonstration of large scale production capability for small diameter SWNTs and high surface area mesoporous carbon materials with right amount of metal loading.
- Terminate this project.
- Continue this science-based project as-is, but begin to think of ways to make larger quantities for tests by others.

Project # STP-21: Carbon-Based Hydrogen Storage

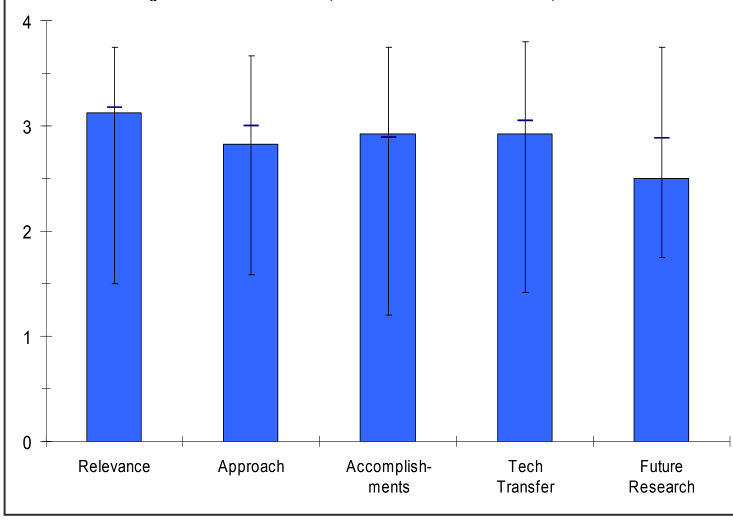
Ted Baumann; Lawrence Livermore National Laboratory

[Partner of the DOE Carbon Center of Excellence]

Brief Summary of Project

Designing new nanostructured carbon-based materials that meet the DOE 2010 targets for on-board vehicle hydrogen storage of 6 wt.% H_2 is the objective of this Lawrence Livermore National Laboratory (LLNL) project. Metal-doped carbon aerogels will be prepared, characterized and evaluated for their hydrogen storage properties. Mechanisms associated with hydrogen adsorption in these materials will be investigated using advanced nuclear magnetic resonance (NMR) techniques.

Overall Project Score: 2.9 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- The proposal to increase the hydrogen uptake by doping carbon aerogels with various metal particles is aimed at addressing the goals of the hydrogen storage problem. This is a high risk project that should be explored in the context of a high risk project. The light weight and high porosity is an attractive feature of the carbon aerogel materials. Carbon aerogels without metal additives have not been able to demonstrate sufficient hydrogen storage capacity, and the question is whether metal doping can lead to a large enhancement in storage capacity at room temperature.
- Project is limited by many of the drawbacks that CNT technology has but so far the material looks easier to synthesize and tune than CNTs.

Question 2: Approach to performing the research and development

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

- The required target capacity for the proposed system should be closer to 7-8 wt.% on system basis.
- The approach of controlling the nanopores and macropores to understand the interplay of these parameters with hydrogen uptake is encouraging. The use of different dopants to vary binding energies is desirable. The approach should emphasize that initial work will be done at 77K where quantitative studies can be done, rather than at room temperature where DOE has programmatic interest.
- Better approach than using carbon nanotubes since preparation of the materials is simpler and more scalable. Metal deposition also seems simpler and more controllable than on carbon nanotubes. However, overall the project is limited by many of the same drawbacks that CNTs have.

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

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

- Carbon aerogel samples have been prepared and characterized by a variety of techniques. Hydrogen adsorption measurements done by Ahn at Caltech shows that the hydrogen uptake of their materials depends on specific surface area so that optimization can be carried out. The addition of metal particles showed changes in the hydrogen uptake with specific surface area, but so far the effect has been to decrease the hydrogen uptake.

Measurements thus far have been at 77K, which is appropriate for exploring the pertinent effects, since uptake at room temperature is too low. Eventually, measurements at room temperature will be needed to compare the characteristic hydrogen storage behavior of these materials to others in the program.
- PI demonstrated good process in synthesizing and characterizing the aerogels. More work needs to be done to reduce precious metal loading and fully understand pore size effect on storage capacity.

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

This project was rated **2.9** for technology transfer and collaboration.

- Collaborations with Ahn at Caltech are very good. In the future there are plans to interact and collaborate with Wu in North Carolina on NMR measurements. More collaborating may be possible and desirable.
- Working well with C. Ahn.

Question 5: Approach to and relevance of proposed future research

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

- It is not clear if the current activities address the minimum storage capacity of 7-8 wt.% requirements.
- Plans for FY06 are to control pore size distribution and to study different dopants, both metal and boron, which are consistent with the present program of this group. Plans to work with Wu on NMR characterization represent a step forward.
- Proved that one can obtain high surface area aerogels. However it seems that the doped materials did not show any improvement over other materials with the same surface area probably due the inaccessibility of the metal sites. Accordingly one should solve this problem before going to higher surface area.
- Good to include go/no-go points on volumetric density and metal doping reduction.

Strengths and weaknesses

Strengths
- This group has great expertise in the preparation and characterization of carbon aerogels. They are also experts in doping carbon aerogels with metals and for characterizing the resulting changes in properties. They are adventurous in trying to explore new applications for their materials.
- Aerogels should be much easier prepared than nano tubes.

Weaknesses
- It is not clear if the current activities address the minimum storage capacity of 7-8 wt.% requirements.
- The group does not have much prior experience with hydrogen storage applications. They need to focus more on what the metal doping does for hydrogen storage and why.
- Aerogel seem to have the limitation of surface area similar to that of activated [carbon]. Heat transfer and volumetric density looks like it will remain a challenge as well.

Specific recommendations and additions or deletions to the work scope

- The team should measure relative benefit of a specific dopant in a carbon aerogel host as compared to other carbon hosts. A comparison should be made of room temperature vs. 77K uptake for carbon aerogels vs. other carbons both in their pristine and doped forms.
- Could these materials make good insulators for cryo vessels?

Project # STP-25: Electrochemical Hydrogen Storage Systems

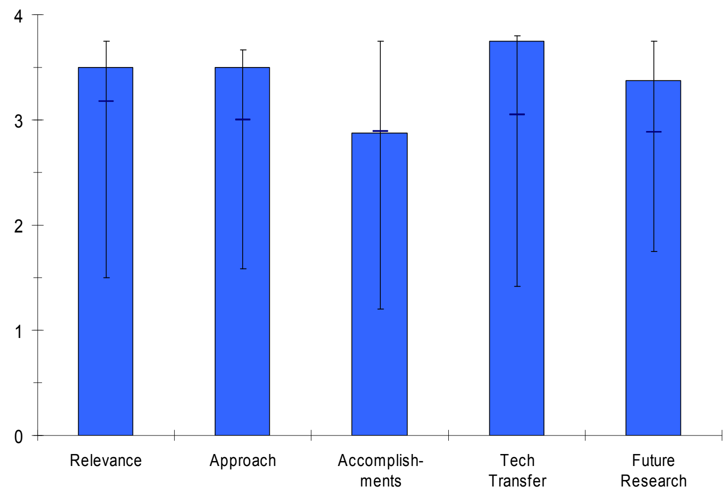
Digby Macdonald; Pennsylvania State University

[Partner of the DOE Chemical Hydrogen Center of Excellence]

Brief Summary of Project

Two strategies are pursued in this Pennsylvania State University project to advance hydrogen storage technology. In the first case, hydride hydrolysis/ regeneration is investigated by exploring the electrochemical reduction of B-O to B-H, while in the second strategy the electrochemistry of various polyhedral boranes is explored to ascertain if electrochemical transformations can be affected between various members that reversibly absorb and release hydrogen and hence could form the basis of a new hydrogen storage technology.

Overall Project Score: 3.3 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project aligns fairly well with the President's initiative, working on efficient regeneration.
- This work is directed at investigating electrochemical approaches to convert borates back to borohydrides, as well as possible new electrochemical routes to hydrogen storage.
- Regeneration of sodium borohydride can be done either chemically or electrochemically. PSU is looking at electrochemical routes to regenerate sodium borohydrate (borate to borohydride). Cost effective and energy efficient regeneration is required to prove the feasibility of using sodium borohydride as a on-board storage material. The project is highly relevant to the mission of the chemical hydride CoE.
- This program addresses a critical issue in developing a new method to regenerate B-O to B-H.

Question 2: Approach to performing the research and development

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

- Approach appears to be well organized and structured.
- Project appears to coordinate with the chemical hydrogen storage CoE.
- The electrochemical approach is excellent.
- The approach down to two alternative paths appears sound. One path is direct electrochemical reduction while the other path is through polyhedral boranes.
- The program is well-designed. Very good interaction with other CoE partners.

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

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

- Project appears to have made good progress in its borate and polyborane work.
- The voltammetry technique for the detection of $BH4^-$ in electrochemical reactions appears to be quite useful. The results suggest that the electrochemical conversion of borate to borohydride is quite difficult. The polyborane work appears to be making progress.

- Progress in the direct reduction route appears to be the determination that numerous attempts at direct reduction have not been successful so far. Alternative pathways through intermediates are being pursued. The other path has identified suitable solvents and reference electrode. Electrochemical transformations have been shown to occur in the B_{12} and B_{10} systems, but their exact nature is unknown.
- The initial electrolysis results are promising. The barriers are identified.
- Not clear how project is performing to DOE targets.

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

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

- This project shows very good collaboration, including national labs and industry.
- This project is well interfaced with the chemical hydrogen storage CoE activities.
- Collaboration appears strong between PSU and other CoE members.

Question 5: Approach to and relevance of proposed future research

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

- This project appears to have a strong plan for future work.
- The project has excellent plans for future work.
- Future plans are to further research promising leads in both paths. However, the go/no-go decision on borohydride regeneration is approaching without assurance that the electrochemical approach will be successful in that time frame.

Strengths and weaknesses

Strengths
- Project appears to have a solid plan.
- The PI and his team have excellent electrochemical expertise.
- Strong collaboration. Innovative approach. The initial results are promising.

Weaknesses
- It is not clear how this project is performing to DOE targets.
- Difficult thermodynamic barrier.

Specific recommendations and additions or deletions to the work scope

- In the future add this project's performance to the table showing DOE technical targets.
- The CoE and PSU should develop a detailed plan that maximizes the amount of information that this approach can bring to the go/no-go decision process.

Project # STP-26: Chemical Hydrogen Storage Using Ultra-High Surface Area Main Group Materials

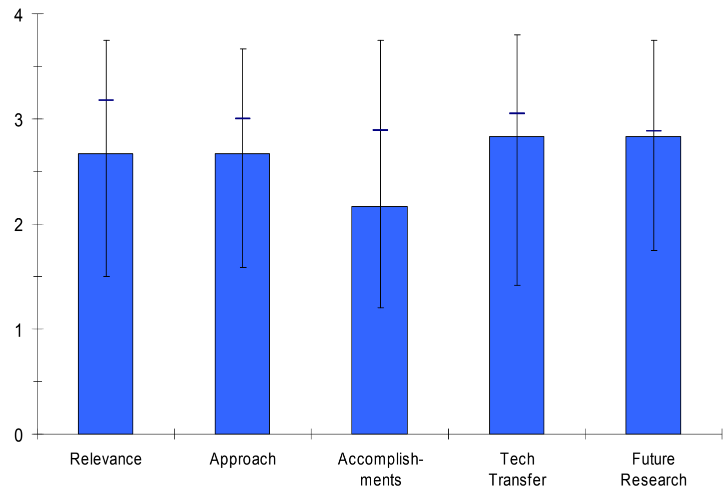
Philip Power; University of California-Davis

[Partner of the DOE Chemical Hydrogen Center of Excellence]

Brief Summary of Project

The goal of this University of California project is to identify hydrogen storage materials enabling DOE targets and increase the understanding of synthetic approaches and physical properties of main group element clusters, such as Si, B, Al, and alloys thereof, as well as BP and BN compounds. Over the past year, efforts have been directed towards designing simple routes to such compounds using mild conditions and studying weight and volume of the synthesized materials as well as the reversibility of hydrogen uptake.

Overall Project Score: 2.5 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.

- The emphasis on this project is to develop new, relatively simple routes to hydrogen storage materials, based on nanosized particles of light elements.
- This project intends to discover new, viable chemical hydrogen storage materials.

Question 2: Approach to performing the research and development

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

- The approach of attaching hydrogen or hydrogen-containing species to the surfaces of nanosized particles of light elements such as B, Si, and Al is an interesting one.
- Approach is directed toward the synthesis of new candidate storage materials using main group elements and "mild" synthesis conditions.
- Fully characterize the new materials including hydrogen take-up, release and regeneration.

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

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

- Nanosized Si and B particles have been successfully synthesized.
- Several nanoparticulate/nanocrystalline compounds have been synthesized and characterization is underway for these new materials.
- No definitive data on hydrogen take-up, release or regeneration was reported - apparently these measurements are currently underway. Composition and reactivity characterization has begun and some compounds appear to be promising.

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

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

- This project is a part of the chemical hydrogen storage CoE.
- UC-Davis is a member of the chemical hydrogen CoE.
- Specific collaborations with LANL, PNNL, U of Penn and U of Alabama were listed.

Question 5: Approach to and relevance of proposed future research

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

- Having successfully synthesized nanosized Si and B particles, the future thrust of the project is to attach as much hydrogen as possible to the surfaces of these particles.
- Planned future activities appear to be reasonable.

Strengths and weaknesses

Strengths
- This is a potentially simple approach for developing a new type of hydrogen storage material based on nanosized elemental particles.
- Builds on synthesis capabilities at UC-Davis.

Weaknesses
- It may be difficult to attach substantial amounts of hydrogen to the surfaces of the nanoparticles. The nanoparticles may not remain nanosized after hydrogen uptake and release cycling. The surfaces of the Si and B nanoparticles may be highly susceptible to poisoning, particularly with oxygen.

Specific recommendations and additions or deletions to the work scope

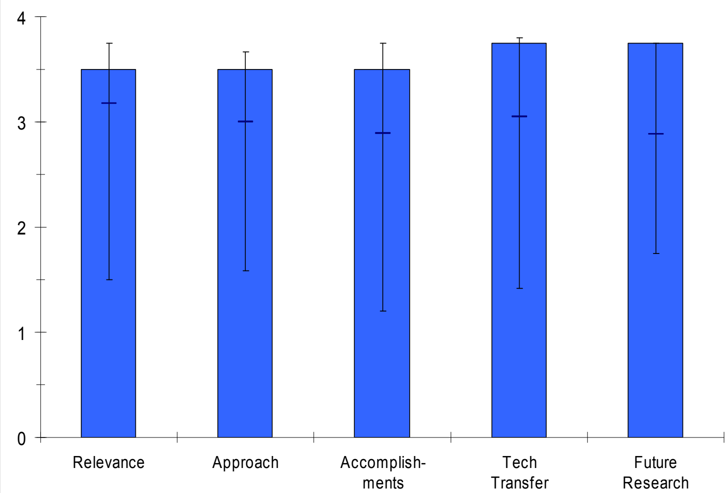
- Obtain preliminary assessment of hydrogen up-take and release as quickly as possible after compounds are synthesized in order to determine which materials should receive more extensive characterization. Narrow the number of potential candidate materials in order to focus on the more promising candidates.
- Project is heavily focused on synthesis and characterization of new materials. Suggestion is to conduct performance evaluations for hydrogen storage at earlier stages.

Project # STP-27: Main Group Element Chemistry in Service of Hydrogen Storage and Activation

Anthony Arduengo; University of Alabama

[Partner of the DOE Chemical Hydrogen Center of Excellence]

Overall Project Score: 3.6 (4 Reviews Received)



Brief Summary of Project

The objectives of this project are:

- Develop new chemistries to enable DOE to meet the 2010 hydrogen storage targets.
- Develop and implement imidazolium-based H_2 activation chemistry
- Develop and implement systems based on polyhydrides of main group elements: phosphorus, boron, nitrogen
- Develop and implement cyanocarbon systems for H_2 storage
- Provide computational chemistry support (thermodynamics, kinetics, properties prediction) to the experimental efforts of the DOE CoE for chemical hydrogen storage to reduce the time to design new materials and develop materials to meet DOE's 2010 hydrogen storage targets.

The University of Alabama has developed new cyanocarbon, carbene and carbenium ion chemistries to meet DOE 2010 hydrogen storage goals. New conceptual models have been developed for improving weight percent beyond 1:1 stoichiometry to meet DOE 2015 goals. Electronic structure methods were used to successfully predict reliable values of the thermodynamic, kinetic, and spectroscopic properties of compounds for chemical hydrogen storage. Experimental and computational work is ongoing.

Question 1: Relevance to overall DOE objectives

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

- This project is directed at the synthesis of new, unique materials for chemical hydrogen storage.
- The objective of this project is to develop new chemistries for hydrogen storage.
- Work is very well focused on developing new chemistries to meet storage targets

Question 2: Approach to performing the research and development

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

- High degree of integration with the other activities in the CoE. The conceptual approach is outstanding. The approach is well-balanced mix of theoretical work and experimental approach.
- The project has a very sound fundamental chemistry approach, with both computational and experimental elements.
- Chemistry based experimental/theoretical approach provides another tool for identifying potential new materials and in understanding behavior of current materials
- Synthesize new, novel organo-nitrogen compounds.
- Determine electronic structure to obtain thermodynamic and kinetic parameters.
- Develop models for hydrogen storage, release and regeneration.
- Characterize promising materials.

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

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

- Good progress narrowing down the choices. Comprehensive thermodynamic work. They had a fairly good strategy on some of the experimental and synthesis work.
- Good list of accomplishments in both theoretical and experimental studies
- Computational approach to study regeneration and other storage parameters of ammonia borane.
- Initial carbene (2 wt.% material capacity measured) and cyanocarbon (1 wt.% material capacity measured) compounds synthesized and preliminary characterization underway. Phosphocarbon compound has exhibited uncatalyzed hydrogen uptake at room temperature.
- Interesting initial results with the carbenes and cyanocarbons. Very interesting initial results on a new material, where hydriding and dehydriding can be activated by radiant energy.

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

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

- Active collaboration with a number of chemical hydrogen CoE members.
- This project is a part of the chemical hydrogen storage CoE.

Question 5: Approach to and relevance of proposed future research

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

- Planned experimental and computational activities appear to be reasonable.
- The proposed future work looks excellent.

Strengths and weaknesses

Strengths
- Builds on strong chemical synthesis and characterization capabilities and extensive related work on similar compounds.
- This project is very strong in its chemistry.
- Innovative approach for chemical hydride storage. This presents the possibility of onboard chemical hydride regeneration.

Weaknesses
- None.

Specific recommendations and additions or deletions to the work scope

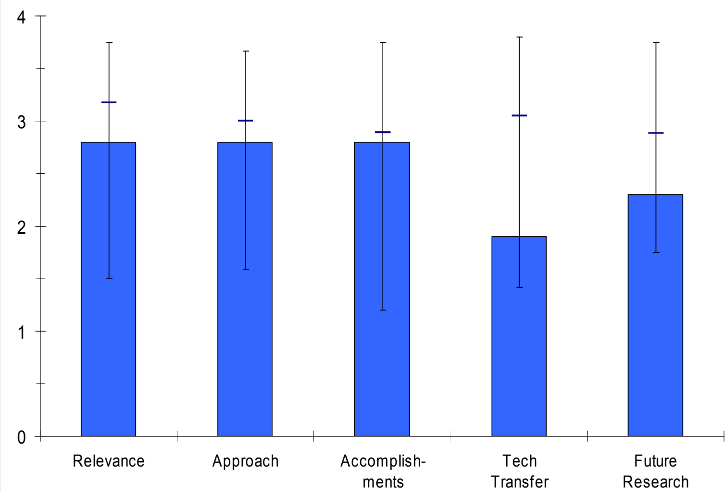
- This project is already 40% spent at this point in time. Perhaps the project scope needs to be reduced somewhat, to focus on the most promising hydrogen storage avenues identified to date.

Project # STP-37: Clean Energy Research Project: Advanced Metal Hydrides
Jim Ritter, presenting; Ralph White (PI) University of South Carolina

Brief Summary of Project

The University of South Carolina Clean Energy Project addresses research for hydrogen production, storage, and use. Currently, 5 tasks make up the Clean Energy Research Program initiated in FY04 and then continued in FY05. Hydrogen Production: Low Temperature Electrolytic Production – This task will focus on production by electrolysis of anhydrous gaseous HCl and by the electrolysis of gaseous SO_2. Hydrogen Storage: Development of Complex Metal Hydrides. This task will focus on the storage and retrieval of hydrogen in metal doped complex metal hydrides (alanates). Chemical Hydrides. The possibility of using sodium borohydride to store and release hydrogen will be investigated. Fuel Cell MEAs: Diagnostic Tools for Understanding Chemical Stresses and MEA Durability Resulting from Hydrogen Impurities. Fuel Cell MEAs: Durability Study of the Cathode of a Polymer Electrolyte Membrane Fuel Cell. This is a cross-cutting project.

Overall Project Score: 2.6 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project partially supports the hydrogen program vision in working on some of the technical barriers.
- Hydrogen storage is the key enabling technology critical for the success of hydrogen as a transportation fuel. Any work which can further understanding of the basic science of hydrogen storage is vitally relevant to DOE objectives, the President's Hydrogen Initiative and the objectives of the Multi Year R&D Plan. It is important, however, to ensure that the most capable researchers are identified to receive federal funds so that the most effective use can be made of taxpayer's dollars.
- The project is targeted at meeting the DOE 2010 hydrogen capacity targets.
- The development of high-hydrogen loading solid materials for storage is one of the most important components of the hydrogen program. This project appears to be addressing the right issue.
- Not clear how physiochemical pathway applies to DOE program
- Why still looking at Na alanate?

Question 2: Approach to performing the research and development

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

- Work has focused on metal hydrides, as previously recommended, may be duplicating work previously done by others.
- PI is looking at materials that have not been fully considered for hydrogen storage. This may be because historic evaluations have been thought to be limited by thermodynamics.
- The off-board regeneration of the $LiAlH_4$ with THF is interesting, if the hydrogen storage capacity can be increased.
- Working with more-or-less standard doped-alanates using solubility properties in THF presents a quite reasonable method to both study the systems and maximize the reversible hydrogen storage capacity. The incorporation of new materials that may result in higher hydrogen loadings is also good. However, the fact that high temperature is needed for this particular conversion should not be forgotten.

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

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

- Project has done a good amount of work, but it is unclear if the work is leading to overcoming DOE technical barriers.
- Thermodynamics of $LiAlH_4$ would not normally be expected to achieve levels of H storage at reasonable temperature and pressure that meet the program goals. The PI has investigated reactions with solvents that may be able to alter thermodynamics to make the material more suitable. However it is unclear how this can be translated into a practical system solution for vehicular transportation. There would have to be some way to accomplish the rehydrogenation and the addition/removal of the solvent without adding significant complexity/cost/etc.
- The results indicate a reversible hydrogen capacity of 4 wt.% at 160°C, via the route of regenerating the $LiAlH_4$ offboard using THF. Some interesting very new results with a new, undisclosed material were indicated.
- A lot of the data is discouraging from the loading temperature standpoint, but it is definitive in determining the limits of the system. The new (proprietary) material is presenting some very impressive reversible hydrogen capacity data. Use this material to gain some valuable information that can be used on other materials, but don't get bogged down with something that won't work for this program.

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

This project was rated **1.9** for technology transfer and collaboration.

- Project has some collaboration with others, but no industry relationship.
- Collaboration & partnering seem limited; there is no real discussion of how the collaborators interacted with the PI in this project.
- Although collaborations are presently somewhat limited, the PI indicates a strong interest to interact with or to join the metal hydride CoE.
- Does not appear to be any.
- Essentially no interaction with researchers in DOE hydrogen storage program

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Fair plan for future research is identified, but it should be more closely aligned with DOE goals and targets.
- The practicality of a vehicular system using these materials needs to be assessed as part of any future research plans.
- The future work appears to be focused on the new material system that has just recently been discovered which has a higher hydrogen capacity.
- Aside from continuing to explore these materials, none was presented.
- Future work not identified

Strengths and weaknesses

Strengths

- Researchers are better focusing on project than in past.
- PI is taking a new look at materials that traditional thermodynamics might have excluded.
- This project introduces the approach of regenerating complex hydrides off-board and provides some interesting results in this regard.
- Good technical knowledge. The approach seems to be working in maximizing reversibility.

Weaknesses

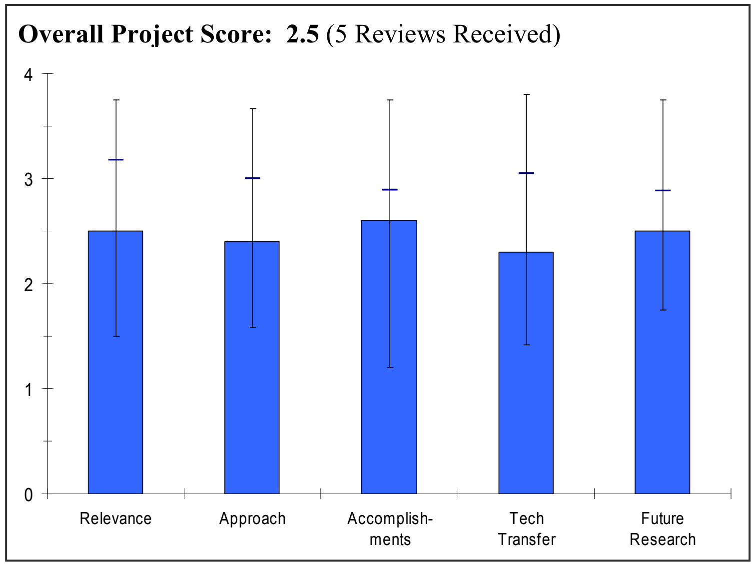
- Work in project appears to be somewhat misdirected as work may be duplicative of other work.
- There is skepticism that a viable hydrogen transportation, storage, and regeneration systems approach to vehicle needs can come from this work.
- This project would benefit from interactions with the metal hydride CoE.
- Are they (or anyone) ever going to get there? They did not adequately address partnering or future direction.

Specific recommendations and additions or deletions to the work scope

- Work with DOE to get project more closely aligned with DOE targets.
- None.
- They need to find the material that will meet the goals. Don't spend a lot of time on materials that won't. It's alright to develop technique on some of the low performers (even $LiAlH_4$ fits this category), but know when to stop.

Project # STP-43: Hydrogen Research at University of South Florida

Lee Stefanakos; University of South Florida



Brief Summary of Project

This multi-faceted University of South Florida project is investigating several hydrogen production techniques; hydrogen storage primarily in the area of advanced metal hydride compounds, and electrode improvements. This is a cross-cutting project started in 2004, with additional funds in 2005.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- A lot of what USF is doing is relevant to the program. They started with a large menu of projects that address various aspects of DOE hydrogen and fuel cell needs and have whittled the non-performers out of the mix.
- Project appears to partially address some of the goals of the MYPP.
- Hydrogen storage is the key enabling technology critical for the success of hydrogen as a transportation fuel. Any work which can further understanding of the basic science of hydrogen storage is vitally relevant to DOE objectives, the President's Hydrogen Initiative and the objectives of the Multi Year R&D Plan.
- Project appears to partially address some of the goals of the MYPP.
- The majority of the work presented here deals with some potentially interesting metal and mixed hydrides (Task 2.1). This area remains one of critical importance.
- The hydrogen storage activities of this project are targeted at identifying and investigating new materials consistent with the DOE hydrogen storage targets.
- It is important to ensure that the most capable researchers are identified to receive federal funds so that the most effective use can be made of taxpayer's dollars. It is important that this research be focused for maximum benefit rather than a broad survey of various technologies that are related to the needs of the hydrogen initiative.
- Most of work appears to be outside of current DOE storage program - lots of work on production, fuel cells, delivery. Likely those efforts are also not connected to DOE

Question 2: Approach to performing the research and development

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

- There was no discussion on how the materials that were chosen for synthesis and study came to be chosen. However, the materials that were chosen appear interesting enough to be considered. The remaining approach including synthesis and characterization via electronic structure calculations seems reasonable.
- The initial approach had elements that may not have been that well thought out. Fortunately, a number of these elements have now been discontinued and the current approach appears reasonably good.
- It is unclear what aspects of the research being reported are to be evaluated. The PI seems to be investigating a range of technologies in many areas. I am submitting evaluation for the two storage "tasks."
- Go/no-go criteria are not clear.

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

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

- The PI is to be commended for looking at $Zn(BH)_4$, a material that has not been thoroughly studied. While this compound could theoretically carry and release a high weight percent of hydrogen, and gravimetric results tend to indicate a significant reduction in weight, there is some question whether that is due to hydrogen release or the decomposition of B2H6. If the latter, this appears to be a dead end.
- 8.4 wt.% gravimetric capacity of $Zn(BH)_4$ may show some potential, but other materials looked at in this project don't appear to show promise.
- The fact that $Zn(BH_4)_2$ shows TGA data with 8.4% by hydrogen coming off at 100ºC is good. The fact that the reaction is reversible is good, but there is nothing showing that it is entirely reversible. 8.4% is just the upper limit - and that doesn't even include system weight. It may not meet goals even at best. It may still be a good test material, however.
- The project reports that synthesis of a $Zn(BH_4)_2$ material with a gravimetric capacity of 8.4 wt.% that shows reversibility at 100ºC (the amount of this reversibility is not indicated).
- Work on $LiNH_2$: Question claims of 7.5 wt.% given thermodynamics of the reactions. Further work is needed to confirm the results.
- Nanocomposite: little of value seen in the work or results.
- The new results for $LiBH_4$ complexes look to be worth following up.
- Very recent new results indicate a $LiBH_4$-based material system with an 11 wt.% gravimetric dehydration capacity at approximately 150°C.
- There doesn't seem to be much in the way of progress other than some initial characterization measurements validating the successful synthesis of some of these materials.

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

This project was rated **2.3** for technology transfer and collaboration.

- Collaboration & partnering seem limited; there is no real discussion of how the collaborators interacted with the PI in this project.
- Collaboration appears to be primarily with universities in Florida.
- Should consider developing stronger collaborative relationships with industry.
- It's good to have a large number of academic collaborators, but some industrial participation would really help.
- There appears to be relatively little research interaction of this project with the larger hydrogen storage community. Such interaction would be greatly promoted if other researchers can reproduce some of the results that have been reported.

Question 5: Approach to and relevance of proposed future research

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

- Future work for Task 2.1 seems to be reasonable and might be beneficial if the PI focuses on a select few materials and can expand the scientific knowledge base about these materials. Work in other task areas reviewed should be deemphasized (and PI acknowledges that some such work will cease in the future).
- Encouraging that comments from last review have resulted in focusing this project on advanced and nanomaterials, but any future work must align more closely to DOE targets.
- Watch out for following up with materials that won't reach the longer term goals regardless of what you do with them. Use them only for technique improvement.
- The future work on this project should focus on verifying, understanding, and optimizing the interesting results that they have reported.

Strengths and weaknesses

Strengths

- Project appears to be responding to previous review comments.
- The project personnel are very aware of DOE goals and are quick to discontinue projects that do not appear to have a chance to meet the goals.
- Some interesting results, if they can be verified by other researchers.

Weaknesses

- It is unclear what aspects of the research being reported are to be evaluated. The PI seems to be investigating a range of technologies in so many areas to the extent that one questions if any one research topic is getting sufficient attention and funding. 12 technical "tasks" are reported in this poster and it is unclear how much funding is allocated to any one technical area.
- Some of the materials being looked at in this project don't appear to have much potential.
- Too many projects to start with diluted the effort.
- Presumably, the purpose of the new effort on electronic structure calculations of complex borohydrides is to serve as a theoretical guide for the selection of potential new complex borohydrides for experimental study. This effort would benefit from interactions with similar activities in the chemical hydrogen storage CoE.

Specific recommendations and additions or deletions to the work scope

- PI ought to focus on one or two key areas of storage research and concentrate there, rather than a scatter shot approach to many aspects of hydrogen transportation.
- Testing is needed to determine if there is truly reversibility of the znbh4 at reasonable temperatures.
- Work with DOE and the Storage Team to get better alignment with DOE targets and goals.
- Watch out for diborane production, and make sure you know how to handle it and trap it if you get it.
- Need project focus.
- This project should send some of its $Zn(BH_4)_2$ and $LiBH_4$-based materials to SwRI for independent evaluation of the results reported.

Fuel Cells

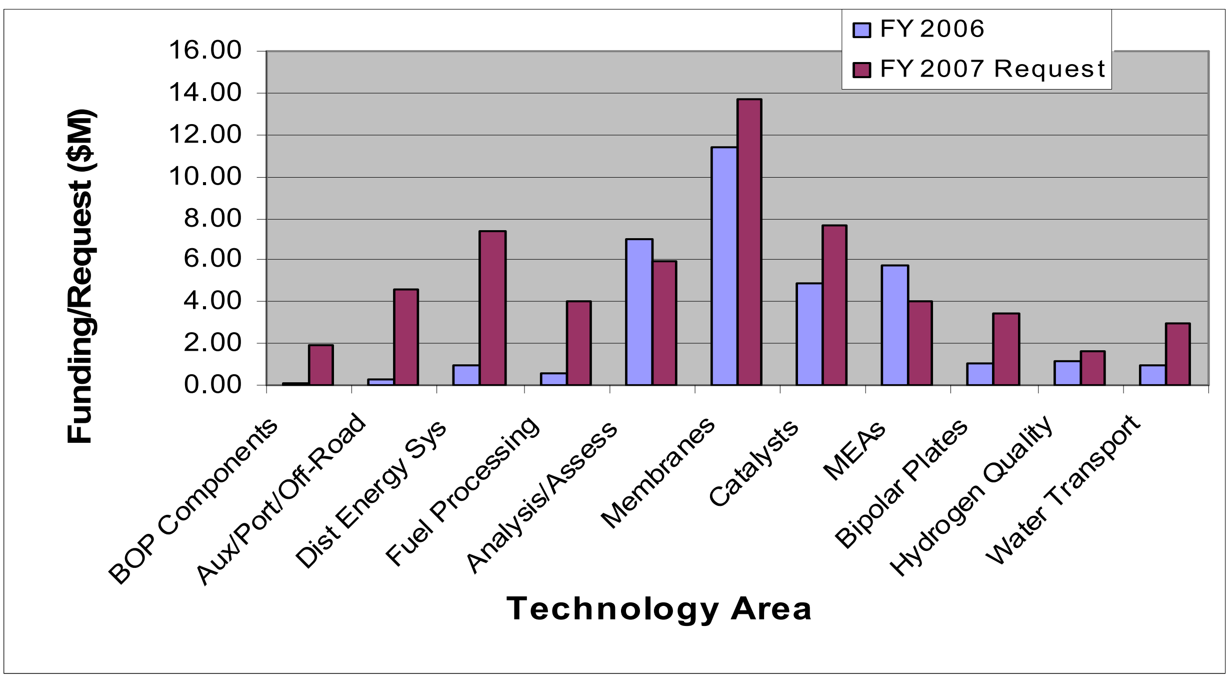
Summary of Annual Merit Review Fuel Cells Subprogram

Summary of Reviewer Comments on Fuel Cells Subprogram:

Reviewers consider fuel cell development to be a critical enabling technology for the success of the President's Hydrogen Fuel Initiative. Overall, the R&D portfolio was judged to be well managed, appropriately diverse, and focused on addressing technical barriers and meeting performance targets. Progress was considered good. Some reviewers thought that the funding for some industrial projects was too large. Others thought that some projects received insufficient funding to enable significant technical advancement. The current focus on partnering (industry, National Labs, etc.) was applauded and reviewers suggested that some projects might benefit from consolidation with other closely related programs – to establish a "critical mass" and better utilize resources and funding as many of the National Lab projects are already doing. Some reviewers thought that balance of plant was not covered adequately while others thought that components crucial to the ultimate success of PEMFCs for transportation, such as cathode catalysts, catalyst supports, membranes, and MEAs should be developed first, before significant effort and resources are invested in other areas.

Fuel Cell Funding by Technology:

The Fuel Cell Technology Subprogram continues to concentrate on the critical path technology of stack components (membranes, catalysts, bipolar plates, membrane electrode assemblies, etc.). Cost and durability of stack components continue to be a key focus of the subprogram.



Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the fuel cell projects were high to average, with scores of 3.7, 3.0 and 2.0 for the highest, average and lowest scores respectively. The scores reflect the technical progress that has been made over the past year. Key recommendations are summarized below. DOE will act on reviewer recommendations as appropriate for the scope and coherency of the overall fuel cell research effort.

MEAs: The scores for the MEA projects ranged from good to very good. The projects are completed with significant accomplishments. New awards from the recent solicitation/laboratory call will begin in the fall of 2006. E-Tek has shown good performance with fine gradient GDLs and progress with IBAD electrodes. 3M's ternary PtCoMn catalysts show high performance and ability; mass activities based on their NSTF catalysts and correlations with the whisker morphologies are impressive. The stability of UTC's PtIrCo catalyst is promising. LANL's LaB_6 support has a long way to go to reach practicality.

Catalysts: Scores for catalyst projects ranged from fair to very good; as a number of these projects conclude, new projects will be awarded in early FY07. Dr. Ross, a PI from LBNL, is retiring; he and his impressive work will be missed. Hopefully, he will continue to work in the field, perhaps as a consultant. Ways to produce Pt-alloy particles with control of exposed crystal faces in high-surface-area catalysts should be explored. BNL's work on gold overlayers shows promise to suppress potential cycling damage and should be continued. The reviewers thought great progress was made in development of non-precious metal catalysts. Many PIs have improved the activity of these catalysts, but need to continue to address the durability issues. Ballard should consider adding a small amount of a precious metal and looking for strong metal-support interactions.

Membranes: The membrane projects were ranked good on average with the exception of the 3M project which ranked among the highest. Projects were completed and new awards from the recent solicitation began. Overall cost implications and suitability of new materials and fabrication technologies for use mass production received consistent expressions of concern. Reviewers recommended that ANL, NREL, and SNL set some specific targets for critical properties that need to be demonstrated and a timeline to meet them. Plug Power should assess the long-term performance stability of PEMEAS membranes under start-up and shut-down cycles. LANL's examination of non-Nafion MEAs could be extended to other new membrane materials being funded by the DOE. The reviewers suggested that Arkema concentrate on conductivity testing and durability testing.

Bipolar Plates: These projects were consistently ranked as good. All projects were completed as new awards from the recent solicitation/laboratory call will begin in the fall of 2006. For carbon/carbon bipolar plates, most DOE targets have been met. For metal plates, techniques that minimize the effect of thermal stresses and mismatch in thermal expansion coefficients between outer layers and the bulk of the plate need to be developed. For both carbon/carbon and metal plates, the cost of materials and manufacturing at high volume production needs to be evaluated.

Recycling: The reviewers thought that the recycling projects were good. More cost analysis as well as further details on the recycling of recovered Nafion need to be presented.

Stationary: Funding limitations curtailed efforts in this area in FY06. The Plug Power backup fuel cell system is completed and is being tested at ANL and at two other sites. Two other stationary projects were ranked fair to good. Reviewers noted that analysis and reporting of the data taken is critical and suggested that similar projects be integrated.

Analysis and Characterization: These projects were ranked good to very good and they strongly support the fuel cell program. The modelers were encouraged to validate their models with real world data; as suggested in 2005. Fuel cell manufacturers need to supply more experimental data to the modelers. For characterization efforts, the PIs might consider causes of degradation and mitigation strategies. Additional stack testing, particularly with respect to freeze/thaw behavior, is recommended.

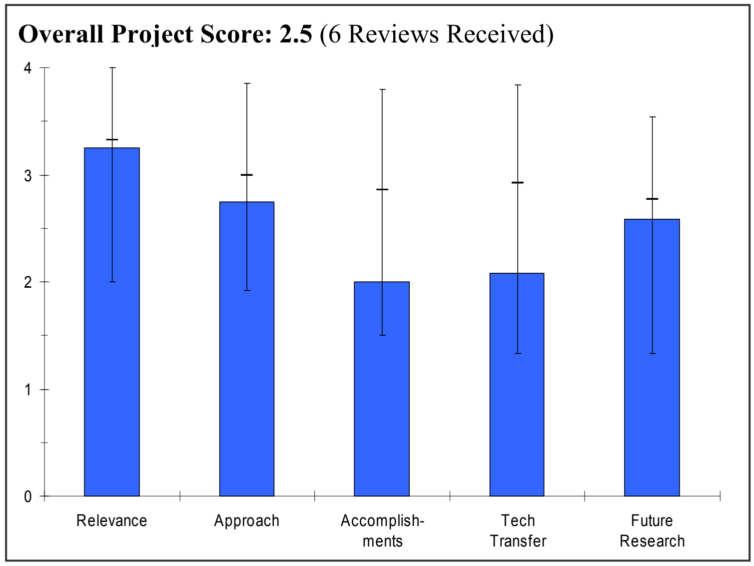
Portable Power, Auxiliary Power, BOP, Fuel Processing: Funding for these activities was suspended in January 2006 so these projects were not reviewed.

Project # FC-01: High-Temperature Polymer Electrolyte Membranes

Debbie Myers; ANL

Brief Summary of Project

Argonne National Laboratory is developing a proton-conducting membrane electrolyte for operation at 120-150ºC and low humidities to meet DOE's technical targets. ANL is investigating dendritic macromolecules attached to polymer backbones, which have a high density of proton-conducting groups to facilitate proton transfer with reduced need for water mediation.



Question 1: Relevance to overall DOE objectives

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

- This project's objectives are stated to be as broad as possible towards overcoming the barriers for fuel cell components. The project's staffing and resources are not nearly adequate, however, to take such a new fundamental approach for high temperature ionomers, and make it "real", that is make it impact the President's initiative in the 2010-2015 time frame.
- The overall project is aligned with DOE objectives. The tasks are structured appropriately to support DOE objectives.
- High temperature, low relative humidity membranes are critical components for PEM fuel cells. The barriers and an understanding of the challenges were well presented.
- The program of the DOE to support high temperature/low RH membrane materials aligns well with the President's hydrogen vision and RD&D plan objectives.
- Project represents long term DOE goals.
- Clear understanding of high temperature operation requirement is necessary for automotive and stationary. For automotive, maximum temperature is 120°C and dry for short time but not continuous, most of time operating temperature is around 60°C. Good to look at required "range" of temperature, including requirements of subzero temperatures.

Question 2: Approach to performing the research and development

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

- This approach appears to offer many opportunities for molecular design. But there is too much synthesis, polymerization, dispersion, film forming, process development, membrane property characterization, as well as eventually durability and fuel cell testing to be done before it would ever be known whether this approach has the potential to meet the requirements.
- Increasing the sulfonic acid concentration using dendrons is a good approach to enhance membrane conductivity. Desulfonation of the sulfonated aromatics > 80°C will limit high temperature application of the membrane. Higher concentration of sulfonic acids will accelerate the desulfonation reaction. Dendrons are expected to be unstable under peroxide degradation conditions.
- The use of dendrons to provide high surface area for proton transfer and are tethered to a polymer backbone is a nice concept. Oxidative stability of the dendrons attached to the water insoluble polymer background is of concern. A key question is: how will this dendronized polymer interact with the catalyst in a cathode?
- The potential of dendromers suggested seems high, however the approach thus far has been very poor. Poor selection of backbone properties (low T_g) initially used could/should have been avoided. Leaving groups of

backbone being "Cl" could be a potential problem for durability – "Cl" is a known accelerator of degradation. ~Ph-SO_3H has been demonstrated in the literature several times to desulfonate.
- Ability to tailor membrane properties is a good approach. Addition of modeling at Caltech is a good addition. Doesn't appear to a systematic plan on how to select compounds to tailor membrane properties.
- Good to leverage molecular level modeling to estimate basic performance. Leveraging this modeling work, implementation of the parametric design approach could work to characterize the polymer materials.

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

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

- This project is just not staffed adequately to have an impact on this problem in a reasonable time. There may be too many of these small discovery membrane efforts that will not be able to answer the hard questions in a reasonable time because it takes years and years of extensive resources to bring any new approach to the state ready for real-life evaluations.
- Fenton's test should be conducted on dendrons and the backbone material to which the dendrons are connected. The membrane should be subjected to hydrolysis conditions for prolonged period to evaluate the IEC stability of the membrane under high temperature aqueous conditions. The water content measurement method under 0% RH should be revised.
- The goal was a low RH, high temperature membrane yet conductivity measurements at these conditions have not been done for a project that started October 2001? An improved polymer backbone was produced, but now films can not be fabricated? Hence no membrane! No gas permeability measurements have been made. Improvements in water loss of the polymer at 150°C and 0% RH are noted. Membrane fabrication progress is too slow.
- Preliminary results of conductivity look promising. However, no membrane characterization has taken place yet – tensile strength, swelling, gas permeation. Characterization of material thus far has been very limited. Too much focus on modeling studies of Caltech – could be useful to guide research but not to make conclusions on the material which the PI did repeatedly. Membrane progress has been very limited – potentially due to lack of researchers.
- Progress appears to be slow. With no end date, estimated end date or even a Go/No-Go decision point, it is difficult to evaluate progress. Project doesn't seem sufficiently staffed based upon objectives.
- Implementation of molecular level modeling is a good accomplishment. However, in the area of polymer synthesis, significant outcomes haven't been seen. Identifying material design parameters and their sensitivity could work.

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

This project was rated **2.1** for technology transfer and collaboration.

- Only one University interaction and this came late in the project.
- More collaborative work should be done. Possibility of industrial collaboration should be explored.
- There is a nice use of modeling by Caltech to provide insight and guidance into the experiments that need to be performed. Perhaps this effort will guide the fabrication of a material that facilitates the "hopping mechanism."
- Only with Caltech at this point, with too much focus on modeling. Modeling work should only guide research, not lead to conclusions. Need to discuss with industry (polymer company) to overcome some of the serious short comings of this project.
- No industry collaborations.
- Good collaboration for molecular level modeling with Caltech.

Question 5: Approach to and relevance of proposed future research

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

- The modeling work should be validated experimentally, before it is relied on to guide the next materials to be synthesized. The material approaches seem to be broadening, rather than focusing, after 5.5 years.

- Fenton's stabilities of the spacers and dendrons should be evaluated.
- Several additional dendronized polymers with fluorinated backbones to improved stability as well as use of "spacers" to enhance stability appear to be promising, but doubts are raised as to if these will actually be fabricated into membranes and then tested are high temperature, low RH conditions.
- Need to evaluate the chemical/mechanical robustness of backbone and dendrimer very closely. At this point, serious drawbacks exist in both cases. Cost analysis should occur as dendrimers can be very expense. It is crucial that more data is generated on well designed membrane materials.
- Future plans seem rather vague, would appreciate more specifics so that progress can be judged.
- Material (polymer) characterization needs more detailed plan.

Strengths and weaknesses

Strengths
- Potentially rich technology area.
- Good approach of enhancing membrane IEC by increasing sulfonic acid concentration. Good synthetic strategy for synthesizing various dendron molecules.
- Concepts and modeling effort guiding experimental work is sound.
- Potential of dendrimers seems to be high. Possibility of using the intrinsic properties of dendrimers on the macro-morphology of the membrane could be very dramatic.
- Take modeling approach.

Weaknesses
- Not enough resources to effectively develop this technology in a meaningful time.
- In presence of water, the sulfonated aromatics undergo IEC loss under high temperature (>80°C). Therefore it's very likely that the dendrons will undergo IEC loss over time. The backbone and spacer structures may be unstable to Fenton's condition. Polyaromatic ionomers lose water content very rapidly ~ 80°C. Beyond this temperature, the membrane resistivity in increased significantly. Therefore, these ionomers may not be a good choice for high temperature membrane.
- Approach thus far has been poor. Poor initial evaluation of backbone/dendrimers used resulted in lost time and money in development. Some future approaches have significant short comings which can/should be avoided.
- Project is understaffed and as a result, the progress is very slow.
- Membrane fabrication is way too slow.

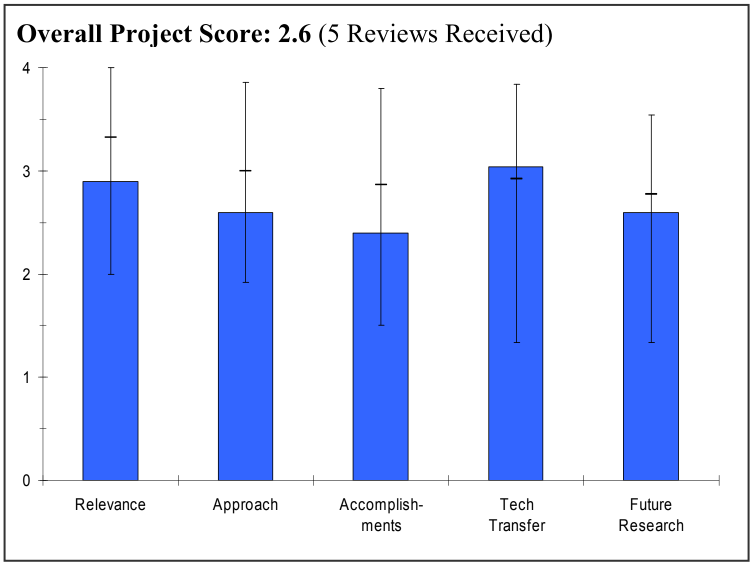
Specific recommendations and additions or deletions to the work scope

- The peroxide stability of dendrons, backbone material and the final membrane should be evaluated.
- Conductivity measurements must be taken under appropriate conditions i.e., low RH, >100°C. Lack of these measurements over 4 years should be cause for concern.
- It is of the highest importance that the PI have significant conversations with polymer chemists in the fuel cell industry. In the field of fuel cells, "C1" is a known contaminant and needs to be avoided if possible – not an acceptable leaving group on the backbone. Benzimidazole can adsorb onto Pt thereby killing the activity of the electrode. If the movement is high, electrode activity will be lost. Ph-SO_3H is known to be chemically weak and needs to be avoided. –COOH has been shown by DuPont to be "weak" spot of backbones and should be avoided.
- Set some very specific targets for three of the most critical properties that need to be demonstrated and a timeline to meet them. Then determine the resources needed to answer those questions in that time, and if it cannot be done, consider a Go/No-Go decision instead. Getting more feedback earlier, finding more partners to help guide the work.

Project # FC-02: Development of Polybenzimidazole-based High Temperature Membrane and Electrode Assemblies for Stationary Applications

Rhonda Staudt; Plug Power

Brief Summary of Project



This Plug Power project is identifying and demonstrating a membrane electrode assembly (MEA) based on a high-temperature polybenzimidazole (PBI) membrane that can achieve the performance, durability, and cost targets of stationary fuel cell applications. Initial screening of potential PBI-based chemistries and structures has been completed, and the top 5-10 candidate materials based on chemical and physical properties have been down selected. Plug Power is conducting rapid screening of candidate PBI materials in 50 cm^2 MEAs; performing detailed electrochemical characterization of MEAs made with selected PBI polymers; evaluating low cost acid-absorbing materials for phosphoric acid management within the system; continuing design and development of bipolar plates with PBI-specific flow fields; and continuing development of a PBI membrane-based MEA with advanced electrode structures providing high catalyst utilization.

Question 1: Relevance to overall DOE objectives

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

- The project supports the President's hydrogen vision and the RD&D plan objectives in terms of lifetime durability for stationary applications. Performance goals discussed as being unlikely, and cost goals have not been addressed in the presentation.
- This PBI MEA strategy is exclusive to the distributed power application (has limited or no relevance to the transportation application), however for Distributed Energy Generation applications, this project is well suited.
- A very relevant program for development of stationary energy conversion devices that use hydrogen fuel. The project is providing valuable data on the durability of these devices and addressing several of the barriers to implementation of the technology.
- Project aligns well to with respect to durability, but could use more focus on the cost barrier as well.
- This project has very good alignment with the objectives of DOE. Evaluation of the PBI-Phosphoric acid based membrane is very critical to the success of the high temperature membrane objectives.

Question 2: Approach to performing the research and development

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

- Issues of stress relaxation and phosphoric acid evaporation have been identified as limiting factors. Improvements have been made by incorporation of inorganic particles. The current level of improvement has not met long term goals; approaches to make further improvements were not presented or defended.
- Cost and Durability are key metrics which have not been addressed at this point in the project.
- The approach appears to be one of development testing, including materials testing under conditions that are relevant to durability. The mechanical testing is good as far as it goes, but the presentation provides little connection between material identity and behavior.

- Previous removal of transportation goals has improved the focus of this project.
- The approach is very well thought-out and well designed. Use of acid absorbing material as an acid trap is a good way of dealing with washed acid.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **2.4** based on accomplishments.

- Demonstrated minor improvements in stress relaxation. Significantly decreased phosphoric acid evaporation rates. Significant evaporation of phosphoric acid still exists. Level of detail given on seals gives no opportunity to address novelty or usefulness of this approach. Little insight has been given into why inorganic fillers serve to improve mechanical properties or evaporation rates.
- Durability may be misrepresented. Temperature effect of durability goes from 6.3 to between 12 and 19 microv/hr to 130 microv/cycle as temperature goes up from 120°C to 160°C to 180°C.
- The project appears to have had significant budget cuts that have significantly delayed progress. Several important deliverables are not spelled out or are delayed. The causes of these delays appear to be outside the control of Plug Power; however, I am a little concerned whether Plug will reach its goals..
- Not clear that current progress on durability will lead to achieving goal of 40,000 hours by 2010. Reducing or eliminating seals can be important to enabling high volume manufacturing of MEAs and fuel cells.
- The long term durability study should be done under dynamic cycling, not under steady state condition. Acid washings should be measured after start-up and shut-down cycles. Effect of acid washings on bipolar plate should be evaluated. The change in membrane IEC over time should be evaluated. The progress of the project seems to be slower than expected to achieve the overall objectives.

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

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

- Project has strong, well rounded team. Publications and presentations are low for funding level (1 presentation occurs twice).
- Good collaboration with other institutions. Did not see collaborations with other projects.
- There are excellent contributors to the program. However, there appears to be some blockage in the provision of important results such as cost estimates. This is threatening to undermine the credibility of the project.
- Close collaboration is apparent between PI and subcontractors and is important to the success of this project. Project may also benefit from collaborating with a National Lab such as Argonne that is also working on polybenzimidazole.
- This project has wide range of technical collaborations within academia and industry, which satisfies DOE objective.

<u>Question 5: Approach to and relevance of proposed future research</u>

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

- The proposed plan does not inherently lead to commercializable products.
- The project is nearing completion according to the schedule. The future activities are somewhat vague and represent final commercialization steps. The details of these appear to be withheld for commercialization reasons. It is therefore questionable as to what level of DOE support is appropriate in the future.
- Future work to further sealing work is important. It is not clear that future work goes any further in addressing cost barrier.
- Significant amount of work is still incomplete after 2.5 years of the project. Is it is possible to complete the stack work under given time? Significant acceleration of the work is needed for the completion of the project.

Strengths and weaknesses

Strengths
- Appropriate technology paths are explored.
- Plug Power presented very good durability studies that demonstrate technology viability and appropriate attention to show-stopping barriers.
- Seal elimination work. Strong collaboration with Rensselear Polytechnic.
- The stability of IEC of PEMEAS membrane at high temperature may allow this project to achieve 2010 DOE goals. The membrane performance remains relatively unchanged under steady state fuel cell conditions. PEMEAS and RPI team brings in years of experience in PBI-Phosphoric acid membrane system.

Weaknesses
- Cost studies should have been done earlier. More explanations for cost and performance feasibility, along with a stronger fundamental explanation for the results presented and how to apply them. Overall the approach presented shows little insight. A discussion of how the inorganic particles interact or how they could be further improved was never discussed.
- Impossible to relate the reported results to basic materials used. Project seems to have reached a point where the activities are now in the final commercialization.
- Project may have lost some momentum due to funding cuts.
- No information on acid washings under start-up and shut-down conditions. Use of acid trap will lead to parasitic loss when used on-board in an automobile system. The durability of bipolar plates under start-up and shut-down cycle is not addressed.

Specific recommendations and additions or deletions to the work scope

- This project appears to be a development program and hence a larger proportion of cost sharing appears to be more appropriate.
- The long-term performance stability of PEMEAS membrane under start-up and shut-down cycles should be assessed. To meet the automobile requirements, the acid washings should be quantified under dynamic temperature cycling.

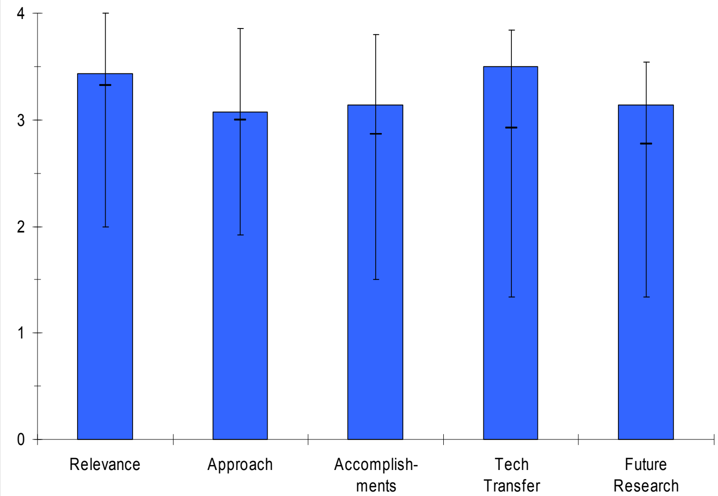
Project # FC-03: Non-Nafion Membrane Electrode Assemblies

Yu Seung Kim; LANL

Brief Summary of Project

The objective of this project is to develop low-cost, high performance and durable alternative membrane electrode assemblies (MEAs). This is being done by exploring MEA operating window dependence on architecture, performing evaluation of non-Nafion binder under H_2/air conditions, and evaluating H_2/air long-term (2000 h) performance of non-Nafion membranes under cycling conditions.

Overall Project Score: 3.2 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- There is good alignment of the project with DOE objectives. There are a good number of collaborators involved in this project.
- This project aligns well with the President's Hydrogen objectives. Understanding and optimizing HC membranes and MEAs are crucial for the success of fuel cells to meet the cost targets.
- Long-term FC performance for 4 membranes was reported for 4 MEAs in DMFCs, but this does not support the Hydrogen Initiative as H_2 has different durability issues than DMFC.
- Addressing automotive needs for fuel cells.
- Any new membrane materials efforts should include work to modify the electrodes as well. This is one of the few projects that address both new membranes and new electrodes which are key to achieving the current fuel cell targets.
- Project is focused on one of the most critical areas. Stable and lower cost electrodes will be required for long term success. LANL is perfectly suited to address this topic.

Question 2: Approach to performing the research and development

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

- The approach of working both water uptake is good. All four tasks have been focused correctly to accomplish the objectives. It is right to focus on the interaction between the membrane electrode catalyst and electrode ionomer.
- Approach is generally very good. Well thought out experiments – unclear why DMFC work is being carried out and does it always apply to hydrogen systems?
- No evidence is presented that such membranes can achieve conductivity targets using 1.5 kPa inlet water vapor pressure.
- Very good approach looking at individual components and the big picture.
- Attempt to isolate performance limitations seemed to fall short. More work is needed for a more thorough understanding of interfacial resistance, durability, and transport effects.
- Good to take parametric design approach and look at sensitivity with water-uptake. Membrane performance should depend on relative humidity. A clearer path needs to be identified to meet the target of alternative membrane development.
- The approach is OK, the catalyst layer (electrode) development appears to be OK, but the analyses and initial observations and conclusions need further refinement. Expectation is that this group will look at the fundamentals of such systems, and then design the appropriate experimental path.
- It is not clear that the actual mechanisms are being elucidated.

- This reviewer feels there are too many mechanisms in play which can lead to erroneous interpretations. 700 hours is not long enough yet under certain circumstances, a dry membrane can fail in hours.
- It is still a sulfonic acid system – water dynamics and transport phenomena are still water-based, regardless of the membrane chemistry.
- Sawtoothing is common – researchers should be aware of such behavior and what creates the response.
- Lower water content membranes at the same equivalent weight was a major difference between Dow and DuPont membranes.
- Can't exclude other phenomena such as catalyst/ionomer degradation, poisoning, etc.

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

This project was rated **3.1** based on accomplishments.

- The performance has been achieved by incorporating fluorinated bis-phenol, but it cannot be implemented practically, since we don't know about the long term effect of the bis-phenol in fuel cell operational condition. Non-Nafion ionomers should be examined with hydrocarbon membranes to evaluate the true compatibility of the electrode layer with the surface of the hydrocarbon membrane.
- Accomplishments have been very good. Good progress has been made in order to understand the needs for HC MEAs. Durability experiments have shown insight into possible mechanisms. Interfacial resistance measurements between membrane and electrode layer are key technology found.
- Achievement of improved interface of PFSA electrode to non-Nafion membrane is solid accomplishment.
- The team appears to have met all current milestones.
- Well coordinated and strong effort. The approach can be used as a model for the implementation of other non-Nafion materials.
- Identifying proper non-Nafion ionomer which provides less sensitivity and taking a good systematic approach.
- Project needs some refinement in the testing methods. A better screening experiment is needed to determine the key variables rather than making assumptions.
- Further refinement is necessary before any scale-up activity is planned. It is too early.
- Peroxide should still be a factor under these conditions – need to better understand the impact (and quantity), especially for these new membranes.
- There is nothing that states a conventional electrode designed for Nafion PEM will be the same electrode which works for this new membrane.

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

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

- Significant collaborative efforts have been put in this work. Good selection of different teams (Government, Industry and Academia) and knowledge bases has been accomplished.
- Good coordination with other groups. Would be interesting to see if this type of technology can be applied to other HC systems, i.e., Polyfuel, Arkema, Hoku, etc. Is this technology applicable to all HC membranes or just BSPH-type?
- Very strong team, but the amount of money for such a large team seems too small.
- Lots of collaborators listed, but it was not clear what they were doing or how the technology was being applied/transferred.
- Appears to be acceptable for this stage of the project.

Question 5: Approach to and relevance of proposed future research

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

- The proposed future work seems to be rational.
- Good future work. Well thought out experiments and well executed.
- The work on block copolymers may yield important insights for many other developers of new membranes.

- With an open ended project it is important that a timeline be proposed. New ionomer design needs to be incorporated into phase separation studies. Not enough detail to evaluate whether future work will lead to the optimized MEA in the most rapid time.
- A more thorough understanding of performance and durability is needed. The source of the recoverable vs. non-recoverable performance loss should be identified.
- The project needs some refining and an additional level of experimental detail based on elucidating the mechanisms.

Strengths and weaknesses

Strengths

- Good approach towards the study of hydrocarbon membranes. Development of non-Nafion ionomers for electrode ink to achieve good membrane electrode layer contact. Good understanding of MEA water content dependence on performance.
- Have shown conclusively the importance of optimizing HC MEAs in order to obtain good performance and acceptable durability. Very important information especially for the success of HC membranes in fuel cell applications.
- Researchers and their institution are capable of carrying out all aspects of the project. Systematic study of water uptake vs. IEC is powerful.
- Very strong team of extremely competent and qualified individuals.
- Strong effort in studying non-Nafion membranes and electrodes. This is a challenging and important problem, and even a moderate success can be used as a model for subsequent efforts with other non-Nafion membranes.
- Systematic approach to characterize new materials.
- LANL has great facilities and know-how. The fuel cell team is still good, and LANL (including this work) can work the "hard" and more challenging themes. The group seems to be creative.

Weaknesses

- Fluorinated additives are being used to achieve desired water content of the ionomer. The long term implication of using fluorinated additives is not known. Cost projection of the membrane using fluorinated bis-phenol is not known.
- Unclear if water uptake versus swelling is the important parameter for understanding durability – should be reexamined to confirm water uptake/swelling is the same. Assumptions are being made to understand HFR increase (increase in interfacial resistance) – it would be important to see how/why this can be done. Should focus on hydrogen – if DMFC work is done, it needs to be repeated in an hydrogen cell.
- No evidence is presented that such membranes can achieve conductivity targets using 25 or 1.5 kPa inlet water vapor pressure.
- Project needs more team coordination and focus.
- Role of collaborators was not clear. More analysis of failure mechanisms and performance loss/limitations is needed.

Specific recommendations and additions or deletions to the work scope

- Quantitative validation is needed to confirm that the increase in HFR is related to the increase in the interfacial resistance between the membrane and electrode layer. Confirmation that water uptake can be considered the same as swelling in regards to durability. Focus on hydrogen cells.
- Focus and extend OCV and start/stop tests to interface-optimized MEAs using Nafion in electrodes and BPSH, 6F, 6FCN membranes. Though difficult, try to obtain access to a wider variety of non-Nafion membranes. Measure fluoride emission rate on 6F type membranes in OCV and start-stop. Report performance with inlet water vapor pressures between 25 and 1.5 kPa. Work to improve performance with inlet water vapor pressures between 25 and 1.5 kPa.
- This same approach should be applied to other new membrane materials being funded by the DOE. A key development of this project should be design rules for incorporating non-Nafion polymers into fuel cell electrodes.
- Determine the true cost and fuel cell durability of the membrane possessing fluorinated bis-phenol additive. Assess the feasibility of using some non-fluorinated material to achieve similar water uptake advantage.

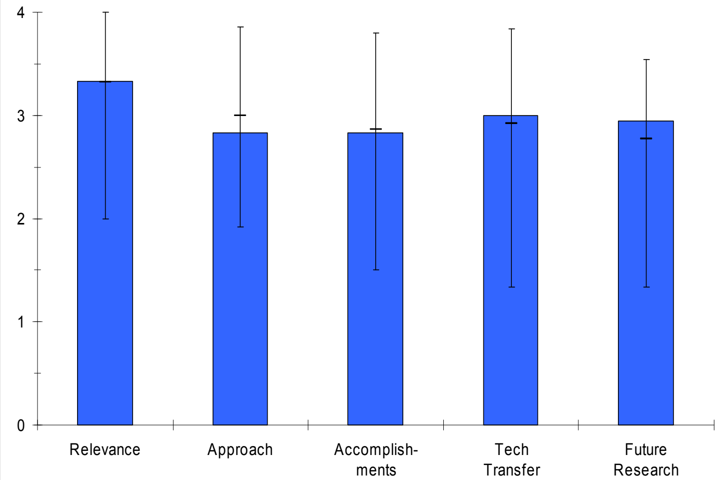
Project # FC-04: Advanced Fuel Cell Membranes Based on Heteropolyacids

John Turner; NREL

Brief Summary of Project

The goal of this research at the National Renewable Energy Laboratory in partnership with the Colorado School of Mines is to develop the methodology for the fabrication of a 3-D cross-linked, hydrocarbon-based membrane using immobilized heteropolyacids (HPAs) as the proton conducting moiety. HPAs and their salts exhibit high proton conductivity at low humidities (below 25% RH) and at elevated temperatures (well above 100°C). NREL will apply its understanding of the structural, chemical, and thermal properties/stability and proton conductivity towards meeting the DOE goals for advanced membranes.

Overall Project Score: 3.0 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project is relevant to DOE objectives.
- Immobilization of highly proton conductive materials is a good direction to proceed.
- High temperature low relative humidity membranes are critical components for PEM fuel cells. The barriers and an understanding of the challenges were well presented.
- Project aligns well to cost and durability barriers.
- New membranes are absolutely required to inverse PEM durability & decrease cost.
- This work is an alternative approach of developing low-cost fuel cell membranes for low relative humidity operation.

Question 2: Approach to performing the research and development

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

- Hpas are water soluble and they can wash out very easily under fuel cell operational condition.
- PMG material, which is being used as host polymer matrix, may not be stable under oxidative fuel cell condition. It may survive in solar panels where peroxides are not formed.
- Immobilization by chemically attaching HPA with polymer is better approach.
- The development of cross-linked hydrocarbon membranes using immobilized heteropolyacids is proposed. A very worthwhile goal. The approach to achieve this goal is well thought out.
- Like many of the new high temperature membrane approaches, the breadth of the material sets and processing options is getting very large very quickly.
- Not clear that this heteropolyacid work is different than past HPA work.
- Elegant, innovative concepts. Excellent organizations of options for available resources. Need to focus on a few options only.
- The project activities are too diversified respect to the budget. The PI needs to focus on acquiring fundamental understanding to identify the opportunities and the challenges of using heteropolyacids for fuel cell membranes.
- There must be more analysis regarding on the cost projection of heteropolyacid-based membranes.

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

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

- The conductivity of HPA composite membrane drops at 80 oC.
- The conductivity at 120°C is 2 order less than Nafion under the same condition.
- No fuel cell performance given.
- Considering the project start date (FY 2005) and the funding level ($150K/yr),much work has been accomplished. Stabilization of the HPAs has occurred and membrane films have been fabricated. The films are flexible and the HPA does not leach out of the membrane. Fenton's reagent test performed shows some concerns about leaching out of the HPA. Preliminary conductivity measurements accomplished albeit at only low temps 27 to 80°C and 100% RH. Higher temperature and low RH measurements are needed.
- Given the short time it has been operating, results to date are encouraging. But the PIs have only scratched the surface of what will have to be done to allow making a judgment that there is a path to success with this approach.
- This project states to be working on improving durability and reducing cost, yet performance is shown.
- Poor conductivity threatens viability of this approach. Too much NMR & TGA analytical data & too little data analysis. Need more evaluation of data.
- Significant progress on immobilizing the heteropolyacid and characterizing the membrane has been made.
- The PI needs to do more evaluation of membrane conductivity at low relative humidity (RH) conditions. The use of heteropolyacid is to eliminate the humidity dependency of fuel cell membranes. Even though the values of conductivity measured at the saturation condition are relatively low, the more important aspect should be the characterization of the humidity dependency.
- More membrane characterization works are required such as gas crossover rates, mechanical properties, chemical stability, water uptakes etc.

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

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

- Limited technology collaboration.
- More collaboration will be better.
- Strong interaction with Colorado School of Mines and also relationship with 3M through School of Mines interaction for the recently funded High Temp Membrane Program (Topic 1). Poster FCP-06.
- May want to consider adding an industry member to the collaborative effort.
- The project has appropriate collaboration between a National Lab and a university to acquiring scientific understanding of the use of heteropolyacids in hydrocarbon membranes. Having another partners for fuel cell testing will be beneficial. anthropomorphic

Question 5: Approach to and relevance of proposed future research

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

- Planned immobilization techniques are appropriate.
- Proposed future work is sound. Increasing thermal stability is a must the range for stability should be raised to 150°C not just 120°C (an operating temperature). A better procedure for measuring conductivity is needed. Gas permeability measurements should be included. What about water uptake and membrane swelling issues?
- With limited resources, prioritization and focus on only the key issues is warranted.
- Future plan appears to address the issues and questions identified through the research, but may not have enough time to complete the work.
- Plenty of work left to be done.
- The proposed activities are forwarding to the right direction, but it is too diversified considering to the budget. It needs to be focused on understanding specific issues rather than evaluation of different materials.

Strengths and weaknesses

Strengths
- HPAs are well known for their proton conductivity at high temperature. Incorporation of this material in membrane properly may allow the membrane to operate at higher temperature.
- Good synthetic approaches are being taken for immobilizing HPA in polymer matrix.
- The goal of developing a composite hydrocarbon membrane using HPAs as the conducting moiety is very sound. The teaming relationships are very good and true collaboration is occurring.
- A tremendous breadth of materials and processes are available. Technical members are strong.
- NREL and CSM appear to be working well together.
- Excellent approach to a key PEM issue.
- This project directly addressing the goal of developing low-cost, no-humidity required fuel cell membranes.

Weaknesses
- The conductivity of HPAs seems to decrease when incorporated into polymer matrix.
- Compared to Nafion, very low conductivity at 80°C, 100% RH.
- It is very unlikely that the conductivity will increase by 3D cross linking of the polymer.
- The DOE goal is high conductivity at low RH and high temperature. While the membrane may be capable of this,a better procedure for measuring conductivity is needed. Gas permeability measurements should be included.
- When not adequately staffed, these kinds of material development projects may meander forever.
- Collaboration only between NREL and CSM.
- Too few resources – need to work fewer options and make more progress on those few materials. No discussion of system performance parameters!
- Not enough budget has been assigned.

Specific recommendations and additions or deletions to the work scope

- Increasing thermal stability is a must. The range for stability should be raised to 150°C not just 120°C (operating temperature). A better procedure for measuring conductivity is needed. Gas permeability measurements should be included. What about water uptake and membrane swelling issues?
- Set some very specific targets for three of the most critical properties and a timeline to meet them. Then determine the resources needed to answer those questions in that time, and if it cannot be done, consider a Go/No-Go decision instead.
- Should either increase funding or narrow the alternatives.
- Rather than evaluation different materials, the project has to focus on understanding the binding mechanism of HPAs and the conductivity mechanism in the membrane.
- Work with the hydrocarbon membrane people to learn about what to expect in high temperature membranes. Else establish collaborations with someone in the MEA industry to understand what to expect from high temperature membranes. Until we have some real high temperature membranes, specifications cannot be made.

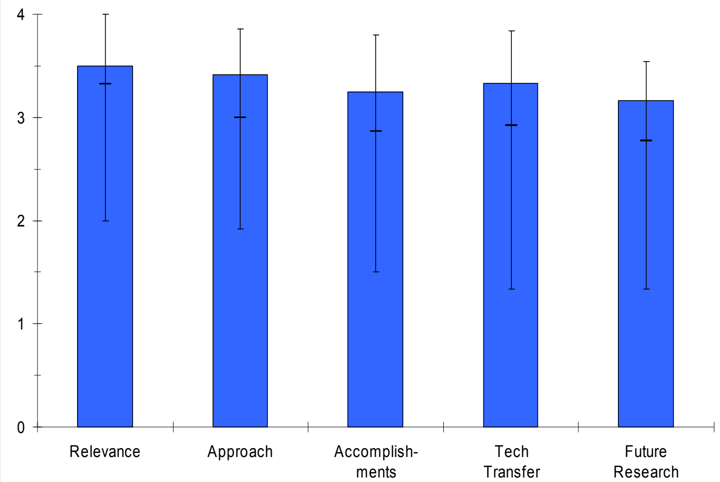
Project # FC-05: Enabling Commercial PEM Fuel Cells with Breakthrough Lifetime Improvements

Gonzalo Escobedo; DuPont

Brief Summary of Project

This DuPont project is utilizing both experiments and modeling to develop a better understanding of potential mechanisms that can lead to membrane failure, including H_2O_2 formation; radical formation; attack of polymer weak sites; material properties degradation; localized stress which promotes cracks/fissures; and crossover failure occurrences. Mitigation strategies such as peroxide prevention, peroxide decomposition, polymer stabilization, membrane reinforcement, and edge seal design and optimization are being investigated to improve membrane durability. The project will optimize each and incorporate them, in total, into fuel cell products.

Overall Project Score: 3.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Lowering the cost and improving the durability of membranes are key criteria for the President's Hydrogen Fuel Initiative (HFI).
- This project targets the HFI's major barrier and is completely relevant to a major component of the HFI.
- Good relevancy to DOE objectives and goal of durability. Clear definition of operating (assumed) conditions is necessary, and it should be consistently used in the durability experiments.

Question 2: Approach to performing the research and development

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

- The study of peroxide and mechanical stress together yields deeper insight into failure mechanisms. Chemical modification, reinforcement and mitigation mechanisms are reasonable approaches for reducing the identified failure mechanisms. Characterization using dielectric spectroscopy is reasonable. Study of model compounds is appropriate for accelerated tests.
- Approach is very reasonable. Model membranes have demonstrated "weak" aspects of membrane. A UTC model seems to be applicable to PFSA-type materials and insight can be gained. University of Southern Mississippi (USM) analytical work seems to be providing insight on membrane degradation mechanisms.
- The approach is refreshingly appropriate for fuel cell research. The combination of chemical degradation mechanisms elucidated by model compound studies with appropriate mechanical and dielectric relaxation studies provides a very insightful picture of what is happening. The application of the results to fuel cell and stack testing is impressive and most appropriate. Combined with e modeling effort the project demonstrates an outstanding approach that has been lacking in fuel cell research in the past.
- It is not clear how the results on the other model compounds at USM relate to the work at DuPont.
- Systematic approach to develop new material.
- Good approach showing progress towards goals.

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

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

- Little progress for the year for a project funded at this level. Conflicting interpretation of loss of mechanical properties (suggesting chain scission) and end group attack (stated as being only chemical attack mechanism). Improvements in chemical attack resistance and membrane lifetimes is compelling.
- CS/reinforcements show significant improvements in membrane durability. USM post-mortem results look very interesting. UTC mitigation technique has shown success with the DuPont material.
- The accomplishments are impressive and very satisfying. The progress is excellent on a very difficult problem.
- Good progress on membrane development with reinforced type membrane. It is necessary to show model validation data. Also it is necessary to investigate the corellation between bubble formation and real-world failure mode.
- Results of drive cycle tests with new materials are very encouraging.

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

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

- Few presentations or publications for a project of this size; a number of the cited presentations were not for this period of performance. Team has necessary strengths; however USM results seem overrepresented for fraction of budget.
- Very high. Collaboration with both USM and UTC seems to be very good. Work from all parties is leading to success of the program.
- The partnerships are appropriate and fully engaged. Excellent example of collaborative research.

Question 5: Approach to and relevance of proposed future research

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

- Proposed research feeds into program well and can bring further insight. Cost analysis is of particularly high value. At this stage of the project, modeling of combined chemical and mechanical effects is unlikely to yield fruitful results.
- USM post-mortem work does not seem to correlate with improvements seen in fuel cell experiments done by DuPont. A strong effort will be needed to interpret the differences seen between USM and DuPont results and should be followed closely to improve membrane durability. The PI did not state what will be done in order to find agreement between the findings of DuPont and USM.
- Project is close to completion. Future plans involve tieing last loose ends.

Strengths and weaknesses

Strengths
- Have shown meaningful lifetime improvements. Bubble formation in membranes following Fenton's reaction is very interesting.
- DuPont is a world leader in the field of polymers and by using their model materials, they have learned the "weak" spots of their membrane and modified them. Furthermore by introducing the reinforcement and then using the mitigation techniques of UTC, major improvements in durability were achieved. Analytical work by USM has provided even more insight.
- Very strong approach that involves an appropriate mix of chemistry, physics, material science and engineering.
- Interesting findings on the role of the side chains in the response of the model compounds to Fenton's reagent. The combination of methods to reduce the FER appears to be effective.
- Systematic approach of membrane material development.
- Leverage model base engineering.
- Well coordinated team effort.

Weaknesses

- Lack of cost analysis, contradiction of chemical attack mechanisms and observed mechanical properties/ dielectric spectrum, and lack of discussion of mitigation mechanism (while some aspects proprietary, is this based on architecture or additives? Some generalities would be useful to understand if adverse interactions exist or if the cost would be likely prohibitive).
- Do the improvements meet the DOE targets for performance and durability for 2010? 2015? If not, what will DuPont do? DuPont needs to show durability as a function of cell potential versus time instead of always FER. FER is only applicable to PFSA type membranes and does not contribute to the industry. DuPont has not talked at all about cost – how much will this cost? Will it reach the targets?
- The seeming lack of correspondence between the USM studies and the DuPont combination of stabilization methods suggests that a closer integration of the work could be beneficial.
- Not sufficient data to support model validation.
- Accelerated fluoride emissions test results were short and it is questionable whether the conditions are truly accelerated.

Specific recommendations and additions or deletions to the work scope

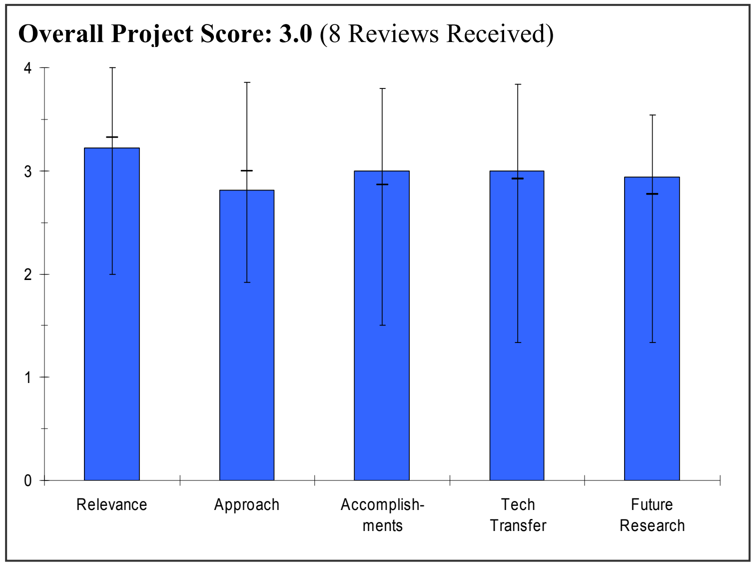
- Cost analysis first, delay UTC modeling work.
- DuPont needs to obtain an understanding of the results of USM and determine why such degradation is seen and how to mitigate that. If not, the membrane optimization will not be complete. DuPont needs to consider whether this material will meet the DOE targets or if more work needs to done. DuPont needs to disclose (or discuss) the cost of these materials. As we all know, Nafion is not cheap and this approach will only add more cost.
- Project close to completion.

Project # FC-06: Development of a Low-cost, Durable Membrane and MEA for Stationary and Mobile Fuel Cell Applications

Scott Gaboury; Arkema Chemicals

Brief Summary of Project

The objective of this Arkema project is to develop low-cost, high-durability polyelectrolyte membranes by optimizing chemistry and process, validating scale-up, developing membrane electrode assemblies (MEAs) based on these membranes, optimizing the MEA for new membranes, validating MEA performance, and validating the MEA performance in single cells and in full stacks.



Question 1: Relevance to overall DOE objectives

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

- Little economic and technical data were shown on why this approach will reduce cost and durability of membranes.
- This project will likely reduce membrane cost and improve durability but questions about this membrane's tolerance of dry gases are unanswered.
- Development of new membranes that are cheaper and more durable is important .
- Project addresses key barriers of cost and durability.
- Good alignment of the project with DOE objectives.
- This project corresponds well to the multi-year R&D plan.
- A durable membrane has applicability for both stationary and automotive applications.
- Blending Kynar with polyelectrolytes is NOT that novel.

Question 2: Approach to performing the research and development

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

- It is not clear that this approach can result in a stable viscous mixture. Test durations are not sufficiently long.
- It appears that this research group has developed their own durability test, but their membranes must be compared to other membranes by the same metrics, i.e. Fenton's test or cycling through OCV or testing at OCV, or simply running a fuel cell.
- More conductivity data needs to be performed at low %RH. The dry cathode experiment is not very impressive considering that the cathode makes water and that the anode is presumably humidified.
- It is a little difficult to evaluate the durability work as explicit chemistry is not described. The results appear to be going in the right direction but the approach could use a bit more of a chemicals and materials emphasis. Fast screening techniques are fine for manufacturing but do not promote fundamental understanding.
- Eliminating apparent dead end with M31 material is good.
- M40 material durability work should continue to be pursued.
- Basic properties of the polyelectrolytes should be explained.
- PVDF is a low T_g material. It gets softened at 120^oC. High temperature application could be an issue.
- PVDF binder softening may lead to the leakage and failure of the gas separating membrane and induce cross-over.

- This approach may be ok for low temperature (<80°C) fuel cell application, but questions arise for 120°C fuel cell application.
- Kynar blend adds mechanical stability. Arkema is taking a systematic approach to improving performance, durability, and operating range.
- The simple blending of Kynar and polyelectrolytes has been conducted in the past, yet the researcher made no references to the pros and cons of the past efforts. How is the Arkema effort different?
- There is no substantive discussion of the resulting composite membrane re: structure; nor is the nature of the polyelectrolyte clarified.
- The presenter implies that these systems are lower cost than perfluorinated systems. Without quantification this assertion is not defensible.

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

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

- The progress since the last review seems to be slow. Many issues remain unanswered.
- 0.1 S cm^{-1} at 100% RH is readily obtainable by other materials. Cost is a huge advantage though and if these systems could be made to perform under truly low %RH conditions with real durability then this would be a very impressive project. The membrane survived 120°C but did it still conduct at 120°C?
- Progress appears to have been made for cost and durability.
- Current durability performance of 2100 hours at steady state appears to be well below the 2006 DOE target of 9000 hours.
- Gen-D (M40) material is a significant accomplishment.
- The proof of concept for the high throughput casting method is a good step towards large scale manufacturing process.
- Arkema identified degradation mechanisms, implemented mitigation strategies and achieved >2500 hours without degradation in *ex situ* tests. Performance is comparable to state of the art Nafion.
- Higher temperature performance was achieved; but the assertion of higher conductivity in water does not translate readily into higher performance in a stack.
- The researcher needs to clarify the differences in chemical structure (at least qualitatively) as he progresses from Gen A to Gen D of the polyelectrolyte.
- Long-term stability of the composite structure (re: durability of the polyelectrolyte within the PVDF) is not clearly explained; e.g., was there a tie-layer or a grafted interlayer polymer involved?
- How does *ex situ* sulfur loss correlate with *in situ* sulfur loss?
- The reduction in weight degradation is a significant accomplishment.

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

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

- The partner team seems to be good; however it is not clear which specific tasks each team member is focused on.
- Good collaborations with universities and industry.
- Good team of collaborators are engaged in this project.
- Collaborations include universities, fuel cell stack/system developer, and National Lab.
- Except for future plans, no current tech transfer or collaborations were evident,.
- It would be useful if the researcher made comparisons with other degradation studies of Kynar blends or with other polymeric systems.

Question 5: Approach to and relevance of proposed future research

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

- The PI did not explain how corrective action of current weaknesses will be implemented.
- More proton conduction and durability testing under harsher conditions needs to be performed with these systems.
- Continuation of M40 material work is good.
- How will they test end of life durability?
- Temperature effect of the membrane should be studied.
- Need to conduct TGA and DMA tests on membrane.
- Future work addresses remaining issues such as low RH and high temperature.
- Future plans are pedestrian and appear to be hand-offs to other companies for further testing and evaluation.
- How will the researcher distinguish between membrane performance and electrode formulations.

Strengths and weaknesses

Strengths
- The PVDF platform is an excellent scaffold for new ionomers that may need more structural integrity.
- Morphology work is a good addition.
- Focusing on M40 material.
- Easy fabrication technique.
- Good control on IEC and mechanical properties of the membrane.
- Good collaboration partners.
- Use of PVDF and polyelectrolytes allows the decoupling of conductivity and mechanical properties.
- Excellent results in the polyelectrolyte/Kynar blends.

Weaknesses
- They never explain why they are not conducting standard durability tests. They should not do only their own tests in isolation.
- There is a lot of fundamental understanding that can be learned from this research however that was not included in the presentation.
- Not much work apparent yet on end of life durability for the M40 material with respect to typical drive cycles.
- Low T_g binding material, PVDF, may not be stable under 120^{o}C operation temperature.
- Peroxide stability of the polyelectrolyte material should be evaluated.
- Thermal effect on phase separation between the polyelectrolyte and PVDF should be evaluated.
- Need to include more information on composite structures, degradation mechanisms, polyelectrolyte chemistry, and stack testing data.
- Need more information on the manufacturing scale-up, uniformity of composite structure from a doctor-blade casting process and the definition of the electrode structure (will it be of the composite material or will it be Nafion-based?)
- Degradation tests need to be conducted over longer times. Extrapolations of short duration tests may not be adequate.

Specific recommendations and additions or deletions to the work scope

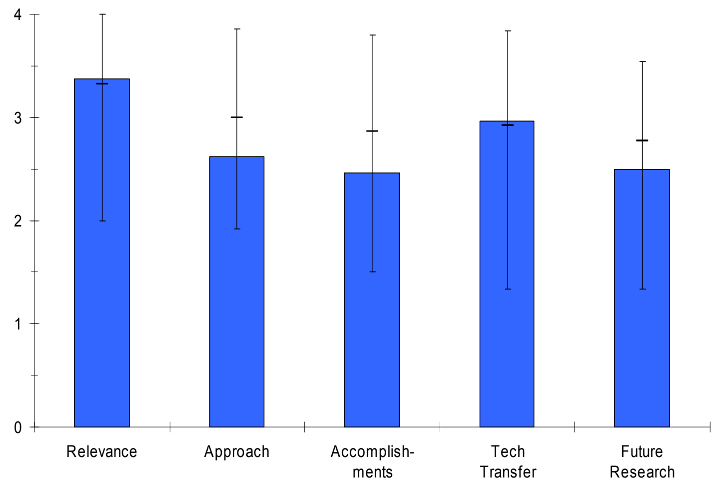
- Include more background on the economic and technical rationale for their approach over Nafion.
- Concentrate on conductivity testing and durability testing. Would be good to see membranes tested *ex situ* under much drier conditions. Not too soon to develop good electrodes for these materials as this is an important problem. Otherwise scale up is premature.
- Present durability performance to DOE targets for typical drive cycles as well as steady state.
- Gen-D (M40) material should be assessed under aqueous hydrolysis condition and Fenton's degradation condition.
- Thermal effect (~120^{o}C) on the membrane should be studied.
- Use the Fenton's test to check durability.
- Compare Arkema systems with past PVDF blends.
- Take more than just an Edisonian approach in blend formulation.

Project # FC-07: Hydrocarbon Membrane

Christopher Cornelius; SNL

Brief Summary of Project

Overall Project Score: 2.7 (8 Reviews Received)



This Sandia National Laboratories polymer electrolyte membrane (PEM) and catalyst coated membrane (CCM) development effort is an alternative approach to address the physical property limitations of perfluoronated PEM materials such as Nafion. The limitations include its poor mechanical properties at temperatures above 80°C, high methanol flux in direct methanol fuel cells, loss in proton conductivity at elevated temperatures, and high material cost. This has resulted in a considerable amount of research to correct these material property deficiencies with an alternative polymer electrolyte. While Nafion is currently one of the state-of-the-art polymer electrolytes for fuel cells, several alternative polymer electrolyte materials have demonstrated better fuel cell performance characteristics. The polymer family of polyphenylenes represents a class of thermoplastics that has the potential of being used as a PEM within a fuel cell. These types of polymers are known for their excellent thermal and chemical stability, while maintaining organic solubility making it possible to form mechanically robust films. The chemistry afforded by the parent Diels-Alder polyphenylene represents a system that has tunable chemical structure and properties.

Question 1: Relevance to overall DOE objectives

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

- The project is relevant to the goals and objectives of DOE.
- Clearly identified technical relevance and interactions/collaborations.
- Development of HC membranes is aligned well with the Hydrogen Fuel Initiative.
- Represents long term DOE goals.
- MEA is being designed with new ideas being incorporated in both membrane and electrode.
- Project is focused on one of the most critical areas. Stable and lower cost membranes will be required for project success. Sandia is well suited to address this topic.

Question 2: Approach to performing the research and development

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

- This approach is very common.
- Sulfonated polyphenylenes are susceptible to hydrolysis under high temperature fuel cell condition.
- There is a high possibility of desulfonation of sulfonated polyphenylene, which will lead to loss of IEC of the membrane.
- The use of the SDAPP is a sound approach re: HC membranes.
- The enumeration of the different expected properties is well developed, especially with the understanding of the role of electrode versus that of the membrane material.
- The comparison with Nafion systems is well-done.
- From the presentation, it appears the technical approach is focused on SDAPP binders and not membranes. Optimization of performance through binder design has greater focus than on membrane characterization and durability.

- SDAPP-type membrane materials do seem to provide improvements in FC performance and lower gas cross-over.
- Preliminary data illustrates that adding ionomer to electrode is not going to be easy – need to incorporate this problem into the approach.
- Short on details about blends, control of permeability, porosity, electrochemical activity, etc. Also, many of the parameters are coupled, complicating the data analysis.
- Clearer definition and metric of interfacial resistance is necessary.
- It is not clear to this reviewer how these membranes are any different than prior hydrocarbon-based systems developed over the last 30 years – all seem to be highly aromatic and sulfonic acid-based.
- Such systems may impact stability with lower RH but there weren't any time-related results to demonstrate stability to peroxide, etc.
- Still need pressurized systems to maintain water management and facilitate proton transport.

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

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

- The need for non-Nafion ionomers for non-Nafion membranes is well proven.
- At high temperature (120°C, 50% RH) polyphenylene material is performing a little better than Nafion, but it is not doing that great.
- The cost of dione material is not given.
- No studies have been done on durability of the PEM material.
- Focusing on the roles of the electrode versus the membrane materials is appropriate.
- The speculation of a more "porous" SDAPP structure versus Nafion-based electrodes is intriguing.
- The researcher has a solid insight into structure-property relationships, unlike the other presentations which take a more Edisonian approach to structural variations.
- Too much focus on binder design, however good progress.
- SDAPP membranes show improvements in fuel cell performance and hydrogen cross-over. A large amount of work was spent using SDAPP as the ionomer in the electrode to minimize interfacial resistance between the membrane and the electrode, however, after all this work, when Nafion was used as the ionomer, performance was better. Very little membrane characterization and no electrochemical studies (CV) done at this time.
- Strong results in both membrane and electrode, but the rationale is not clear. A more systematic approach is needed. The membrane-electrode interface needs to be better characterized.
- Performance of baseline membrane (Nafion) is too low. Interfacial resistance could be an issue. Further investigation of this issue is necessary to fix it.
- The early stage results are promising.
- Degradation and lifetime tests need to be carried out (early life)
- Electrodes and membrane interfaces must be better understood, Sandia should focus on such fundamentals.
- Suggest using additional electrochemical tests to assess the MEA.

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

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

- Lack of interaction with industrial partners.
- More collaborative partners are needed.
- The slides clarify the roles of the collaborators well.
- Very impressive list of collaborators.
- Interaction seems to be high. Clarification of who is doing what is needed for the DOE to assess the contributions of each collaborator.
- Interacting with numerous institutions and companies.
- Role of partners and collaborators is not clear.
- PIs should consider to providing developed material to fuel cell developer/automotive OEM to evaluate under real-world conditions.
- Appears to be acceptable for this stage of the project.

Question 5: Approach to and relevance of proposed future research

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

- Too broad.
- Not enough time for evaluating four different tailor-made hydrocarbon ionomeric materials for electrode and PEM utilization.
- There is no information on types of inorganic materials to be used in organic-inorganic composites.
- Future themes were well-defined, but the details are lacking.
- Much work to be done in the field. Recommend the team show timeline or prioritization of future work.
- Too much focus on reducing the interfacial resistance when mass transport losses seem to be causing more losses. Strong need to understand what is happening in the electrode (electrochemical/gas transport) before future research is carried out. Need to correlate AFM with membrane properties.
- A more systematic approach is needed. "Optimizing" morphology assumes that they know what a good morphology would look like. A study of structure-property relationships could tell you this, but it cannot be stated a priori.
- Needs further improvement of fuel cell performance in a systematic way.

Strengths and weaknesses

Strengths
- Understanding of DOE objectives and underlying challenges.
- The steps taken to solving the problem are adequate.
- The researcher gave a solid presentation and has a sound understanding of structure-property relationships.
- SDAPP material shows promise as a membrane for FC applications. Analytical work (AFM) provides vivid images and could provide great insight.
- Strong collaboration with other institutions.
- Few projects address both membrane and electrode, so this is relatively uncharted territory. However, this is a challenging problem that needs to be addressed, and the results are very relevant to new materials development. A well executed program has the potential to impact many efforts in future years.
- Capability of membrane synthesis.
- Sandia has great facilities and know-how. The team is quite qualified to address these challenges.

Weaknesses
- More work is needed in materials study.
- Collaborations should be established to acquire better ionomeric material.
- There is no clear attempt to check on durability of the HC systems.
- There is a need to better understand the costs of these HC systems as compared to perfluorinated structures.
- Should focus near term research on key durability issues which have been displayed by hydrocarbon membranes.
- Very little membrane characterization – tensile strength, swelling, etc. No electrochemical studies of electrode surface – CV, EIS, specific FC conditions to understand electrode structure. Too much focus on using SDAPP as ionomer in electrode layer without understanding of the mechanisms.
- A more systematic approach is needed. Performance improvements are impressive, but the source of the different effects needs to be identified.

Specific recommendations and additions or deletions to the work scope

- The future work is too broad. More focused approach is needed.
- Continue the excellent collaborations.
- Use cyclic voltammetry to help elucidate structure-property relationships.
- Expand on the analyses, e.g., by operating at various oxygen pressures, to clarify theories on SDAPP "porosity" versus that of Nafion-type electrode layers.

- There is a strong need to quantify the pros/cons of using SDAPP in the electrode layer – as of now, there is no benefit. Helox experiments need to be run to investigate mass-transport phenomena. Experiments need to be run to look at the interfacial resistance specifically. CV experiments need to be run in order verify how SDAPP affects the Pt surface. Serious holes that need to be filled before further work can be done with a logical approach. AFM work must be correlated to membrane properties in order to gain understanding of which morphology is beneficial.
- Need H_2O_2 stability data.
- Need to add to approach – integration of membrane and electrodes, how to make electrodes out of SDAPP.
- With so many parameters being varied, a design of experiment approach could help sort through the many effects.

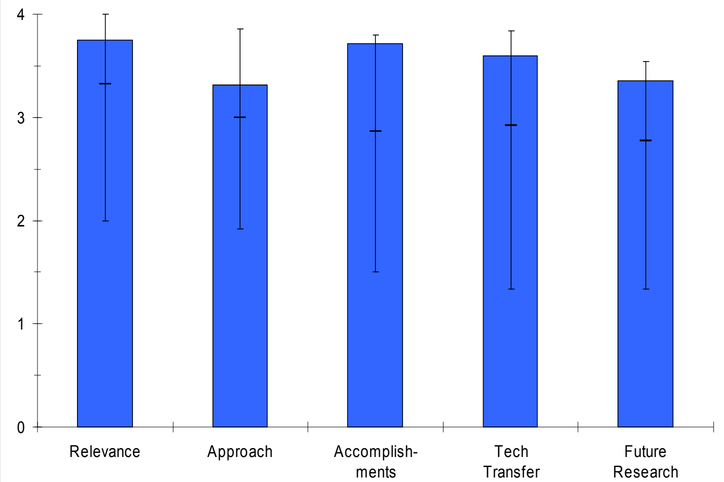
Project # FC-08: MEA and Stack Durability for PEM Fuel Cells

Mike Hicks; 3M

Brief Summary of Project

During this project, 3M will determine root causes of membrane electrode assembly (MEA) failure modes and develop an MEA with enhanced durability and maintained performance that can be manufactured in a high volume process, is capable of meeting market required targets for lifetime and cost, and is optimized for field-ready systems. The system demonstration will be for 2,000 hrs. The focus is on MEA component development, MEA characterization and diagnostics, and defining a system operating window. The overall objective is to develop a pathway/technology for stationary PEM fuel cell systems for enabling DOE's 2010 objective of 40,000 hour system lifetime to be met.

Overall Project Score: 3.6 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The development of fuel cells with lifetime improvements is necessary for reaching the President's Hydrogen Fuel Initiative. The project investigates GDL, membrane, MEA and stack issues with membranes being the focus.
- This project is highly relevant to the DOE Hydrogen Program. Increased MEA durability is critical to the success of fuel cells in stationary applications. The DOE goal for 40,000 hours durability should be considered a minimum requirement for success. Much of the work has applicability to the automotive sector. The cycles chosen for the accelerated testing provide stress to the MEA and are likely to be relevant for predictions of lifetime in stationary conditions. Load cycling that more closely resembles driving profiles would stress the MEA even harder and be more relevant to automotive applications.
- The focus on durability (> 40,000 hrs.) is clear and in line with the DOE program objectives.
- The realistic goals of high-volume manufacturability and low-cost are also appropriate.
- The determination of membrane-electrode assembly failure mechanisms is critical to determining the materials limitations and operating conditions accelerating failure.
- Great focus on durability, but this is the only barrier addressed.
- Project is directly relevant to the Hydrogen Fuel Initiative and fully supports MYPP.
- Project directly targets a principal barrier (durability) to the successful commercialization of stationary fuel cells.
- Project addresses one of the major barriers for Fuel Cells, durability. While this project is directed toward stationary power, the project is applicable to both stationary and automotive applications
- The PI and his team are addressing each critical technical problem which needs to be solved to commercialize PEMFC.

Question 2: Approach to performing the research and development

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

- Study of model compounds is appropriate for accelerated tests. The accelerated tests and models presented are necessary for estimating fuel cell lifetimes without long test times. Investigating reinforcement and edge effects is sensible, as these have been identified as being related to membrane failure. The synergy of GDL, MEA and stack work with membranes is not apparent.

- Accelerated testing and statistical analysis are important components of the approach. The approach is comprehensive. The work with model compounds should result in improved insights into changes in membrane characteristics upon aging. However, it must be shown that the results apply to actual membranes.
- The step-by-step approach is systematic and the linkage of structure developments to optimized system operating conditions shows the researcher's practical stance.
- The approach is holistic and very comprehensive, and the pace of the delivery points to a methodic approach to tackling the challenges.
- This project has a good, balanced approach. The approach could be improved by incorporating post-mortem microscopic characterization to correlate failure location with conditions encountered at this location.
- Approach addresses durability very thoroughly.
- At some point it would be beneficial to understand the cost implications of achieving the durability identified in this project.
- Project is sharply focused on pathways and technology to enable achieving DOE 2010 objective of 40,000 hr system lifetime.
- Project incorporates innovative dual approach of combining 1) optimization of MEAs and components for durability with 2) optimization of operating conditions to minimize performance decay.
- Approach addresses important technical barriers. 3M haa developed accelerated tests AND a correlation to real time tests which allows them to predict lifetimes from accelerated tests. New work starting with nonuniformities may be important and could feed in to requirements for manufacturing.
- The PI has a very keen understanding of fuel cell technology. Each technical barrier is being addressed very comprehensively.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated **3.7** based on accomplishments.

- Chemical decomposition due to peroxide has been demonstrated on model compounds. Edge failure has been shown to be mitigated by a specific (unstated) approach. GDL and stack studies either have not been demonstrated or gave uninteresting results. Segmented cell results show predictable performance in terms of current distribution at low stoichiometric conditions. Non uniformity modeling studies have not been validated at this time; reported results for uneven catalyst layers are interesting, if verifiable. Conclusions regarding reinforcements are suspect and potentially misleading.
- 3M appears to be making good progress in improving MEA durability. Based on accelerated aging tests, including load cycling, 3M has predicted better than 20,000 hrs. 3M and Plug Power identified edge failures as a major cause of early failures and indicated that design changes along the edges of the active area have eliminated the problem.
- The researcher showed a set of excellent accomplishments (from test equipment for component evaluations, through membrane / MEA developments, to system tests and lifetime modeling.)
- The GDL capillary pressure measurement development is a good example of technical collaboration with CWRU; the "lack of correlation between reinforcement and physical property durability" is well done; but this data set needs to be checked out.
- The rapid aging with ion-exchange with Fe cations and the evidence of premature edge effects are useful findings, as is the work on decay mechanisms, using model compounds.
- In general, this project has yielded very useful data on the various causes of membrane and MEA durability.
- Saratoga system test of MEA is valuable to make sure component level durability gains are maintained when incorporated into a system.
- Durability progress is very good.
- Impressive list of technical accomplishments from durability testing and procedure development to identification of membrane degradation mechanisms and operating strategies to minimize performance decay.
- New MEAs with 3M ionomer have achieved approximately 4x better durability.
- PI indicated significant improvements in durability are achievable through demonstrated pathway towards 20,000 hr MEA lifetime with 3M PEM MEAs under accelerated near OCV load cycle tests.

- Overall, progress in this project has been good. 3M has demonstrated lifetimes in accelerated tests that extrapolate to lifetimes which meet the 2005 goals. Development of GDL characterization techniques is important work. Work with reinforced membranes and lack of correlation of durability with mechanical strength appears at odds with others experience and may need more investigation to determine what the differences are and if relative humidity cycling is the appropriate test.
- 3M and the PI have shown historically very good progress that is well presented and easily comprehended.

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

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

- Case Western contributions in the area of small molecule analogues are very useful. Role of Plug Power and University of Miami less clear as non-uniformity studies are much less interesting than the membrane work presented. Significant presentations, but little in the area of publications.
- Very good collaboration between the partners is evident. The work at Case is leading to improved understanding of membrane degradation, and the Plug Power work is important third-party validation of 3M's approach.
- This talk very clearly identified the collaborators and the roles each played in the different sub-projects in this very comprehensive, overall, study.
- Outstanding collaboration with the collaborators contributing in their areas of expertise.
- Strong collaborative team.
- Leveraging the expertise of all members well.
- Project has established reasonable coordination mechanisms.
- It is not clear as to the extent of collaboration with other organizations conducting MEA and stack durability research. For example, would there be synergistic advances if this project coordinated more closely with the LANL durability activities led by Rod Borup?
- Collaboration with Plug power and use of the Saratoga system provides benefits and experience with stationary systems. University involvement at Case and U of Miami.
- 3M has a great mixture of academic (university) and other commercial developers.

Question 5: Approach to and relevance of proposed future research

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

- Project looks to finish/expand current tasks.
- Good plans for future activities.
- Again, this section was well-presented and well-focused (MEA and stack development with a key fuel cell partner; MEA degradation tests along with attempts to correlate structure and properties, statistical lifetime predictions.)
- The focus of fluoride release is important, but the need to determine whether the fluoride comes primarily from the electrode layer or the membrane is paramount.
- The description of the future plans was too vague.
- Future durability working on lifetime predictions is very good.
- May want to consider estimating cost of achieving this level of MEA durability.
- Future work schedule is logical, building upon past progress.
- No discussion was provided as to technical contingencies or off ramps if unexpected show stoppers arise.
- Future work builds on previous work and is directed at bringing the project to completion.
- The future program is well planned and follows-on to the present program. Keep them FUNDED!

Strengths and weaknesses

Strengths
- Chemical control and modifications are possible.

- 3M is attacking the problem of durability in a comprehensive manner. The work at Case is leading to a clearer understanding of the changes in membrane characteristics upon aging.
- This summary presentation is broad and very comprehensive, although some of the conclusions need to be re-checked: reinforcement and the "lack of correlation" to durability?
- Microscopic analysis of MEAs post-mortem would be a nice addition to determine failure mechanisms.
- Analysis tools are used.
- Project appears to be very focused.
- Very solid project approach and strategy.
- Impressive list of technical accomplishments to date.
- This project can draw from other concurrent projects at 3M (catalyst and advanced MEAs for enhanced operating conditions) and bring in the newest findings from them. This project has developed/advanced accelerated test protocols that are of use to the fuel cell community as a whole and has performed a lot of the work providing an understanding of membrane chemical degradation mechanisms.
- Technical team and company management.
- Great cooperation and coordination with partners.
- Super understanding of technology.

Weaknesses

- Conclusions regarding reinforcements and the data taken by GM (Gittelman) have been overstated and presented out of context. The type of reinforcement investigated involved soft PTFE which does little structurally. In Gittelman's work, the correlations with water uptake seem to suggest water uptake is more important for failure, and other reinforcement types could help this problem. Additionally, for situations where tear is important, any such reinforcement is likely useful. The statements given in this presentation regarding reinforcements may end up being very misleading to those who watched the presentation.
- There are a lot of data that indicate reinforced membranes do improve durability in contrast to 3M who concluded that the reinforced membrane did not improve MEA durability. The different conclusions may well result from the great variability in testing protocols and conditions. From the presentation it was not apparent that the definition of the accelerated testing profiles was made after considering possible failure mechanisms.
- A revelation of the 3M chemistry would help bring greater clarity to why these ionomers show improvements over traditional Nafion.
- More work is needed on the peroxide effects.
- Project only addresses one barrier.
- It is sometimes difficult to determine what work was performed under this project and what work was performed under the concurrent projects in catalysts/advanced MEAs.

Specific recommendations and additions or deletions to the work scope

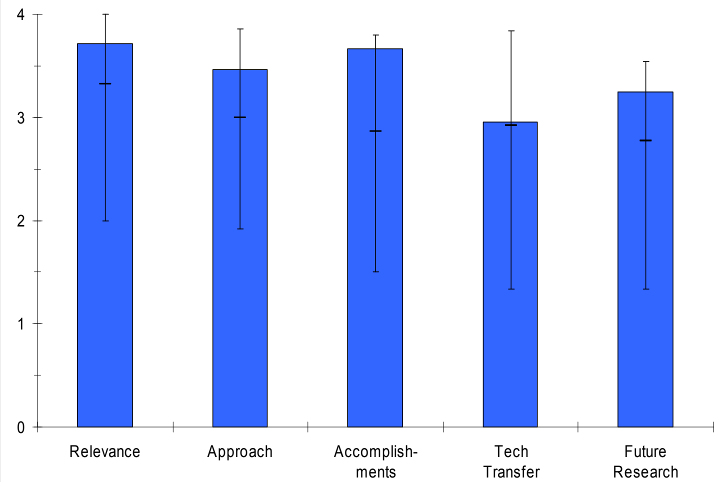
- Focus on membranes, decrease work on GDLs and MEAs/stacks.
- Standardized testing conditions and protocols need to be defined so that data from different experiments can be compared on a similar basis. The USFCC work in this area should be able to guide future work in the durability.
- Well done! Just focus on your critical assumptions and issues that you have recognized.
- Add in some effort to evaluate cost.
- Increase collaborations especially with other DOE supported fuel cell durability projects.
- This was one of the best presentations I have ever attended.

Project # FC-09: Low Pt Loading Fuel Cell Electrocatalysts
Radoslav Adzic; BNL

Brief Summary of Project

The purpose of this Brookhaven National Laboratory project is to develop low platinum-loading fuel cell electrocatalysts. The objectives are to demonstrate the possibility of synthesizing novel electrocatalysts for O_2 reduction with monolayer level Pt loadings and conduct long-term tests in fuel cells with them, commence a study of mixed Pt-late transition metal monolayer electrocatalysts, further characterize a PdCo electrocatalyst for O_2 reduction and a $PtRu_{20}$ electrocatalyst for H_2/CO oxidation, and to gain a deeper understanding of the mechanism of their catalytic action.

Overall Project Score: 3.5 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The PI and his team are focusing on the key problems: cost of electrode catalysts and endurance.
- Approach for reduction of PM content in FC electrocatalysts which has demonstrated ability to significantly increase the mass activity in ORR reaction.
- New addition was modification of the nanoparticles surface to improve durability.
- The project is well-targeted to reducing electrocatalyst loading, enhancing catalytic activity, and increasing durability – hence, definitely relevant to the DOE plan objectives.
- The researcher presented well and systematically, although he mumbled at times.
- The project constitutes a very well thought-out approach to one of the most important DOE objectives: high activity cathode catalysts to reduce fuel cell costs.
- The development of improved (higher activity, longer life, and lower cost) electrocatalysts is imperative for fuel cell commercialization in automotive applications.
- These activities are relevant to meeting the DOE objectives. If successful, this project could result in a critical breakthrough on the cost goals for fuel cell catalysts. The alloys being examined may have applications beyond the PEM fuel cell designs being considered.

Question 2: Approach to performing the research and development

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

- The keen understanding of the PI has his team and their expertise in surface behavior and fundamental electrochemistry making good progress.
- Well thought and systematic approach based on fundamentals.
- Well-done! The presentation was comprehensive and its focus on mixed metal systems was deliberate and systematic.
- The researcher was very clear in his delineation of the different types of structures he was investigating (Pt on Pd, then onto mixed Pt metals on Pd, then core-shell nanoparticles.)
- The correlations of structure with chemical activity were clearly presented, including the focus on O_2 reduction kinetics and mass activities.
- The project's approach is a carefully thought-out pathway to the demonstration of high catalyst mass activities.

- The need for checking the resistance of active catalysts against potential cycling damage has been very well addressed.
- The project has been characterized by excellent and rapid transfer from single-crystal results to supported catalyst RDE testing and has made a good start on MEA testing.
- The presenter did not explain the basis of the choice of elemental materials, e.g. What fundamental considerations suggest positive results using Ir, Au, and Fe?
- The presentation would be more effective if the presenter showed photographs or presented overviews of the sample materials and test fixtures.
- The approach taken by the PI appears to be practical and sound. Tolerance to methanol and a synthetic reformate is mentioned. Definition of the reformate and cognizance of impurities likely to be in transportation grades of fuels was not touched upon.

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

This project was rated **3.7** based on accomplishments.

- The work on surface stabilization is very impressive and has made a significant advancement in fuel cell science.
- Excellent progress in new compositions and durability improvement of Pt-based catalysts and new support for increased performance.
- The findings of increased mass activity and the intriguing conclusions on the behavior of Au nanoclusters on stability and durability are impressive.
- The fine-tuning of the Pt substrate in the mixed metal structures as well as the interpretation of the XANES data for chemical stability in DMFC systems represents solid work.
- The combination of theory (re: dual pathway kinetics) with experimental data rounds up a very solid body of work, which results in catalytic structures almost at DOE performance goals – good work.
- Excellent RDE demonstration of new catalysts with high activity per mass of precious metal.
- A good start was made on testing the durability of these new catalysts against potential cycling effects which are likely to be the greatest challenge that they face.
- The gold overlayer work has shown impressive, and very surprising, suppression of potential cycling damage, through an approach that may have wide applicability.
- Development of very nice data-driven theory, and thoughtful application of existing theory to explain results and guide new directions.
- The responsiveness to last year's comments about cyclic testing was excellent.
- The presenter should explain the sufficiency of the test conditions, in particular the voltage range selected, and the temperature, and convince the audience of the direct applicability to the fuel cell operating environment.
- The comparisons of activity and surface area integrity with and without gold were very compelling.
- The results with the Pt_3Fe system offer a high degree of confidence that catalyst loading and cost targets can be met.
- The work on Pd_2Co for DMFC should be a part of a different presentation – it is distracting and lower in priority.
- The technical progress is very significant and appears to show great progress towards several alternate catalysts to platinum.

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

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

- It is not clear that their good work is being transferred to other scientists and developers.
- Collaborations with P. Zelenay at LANL are clearly identified; but the other collaborations were not well-presented.
- The work with Plug Power should be further explained.
- Productive collaborations are in place and more are being brought on line.

- A previous reviewer's comment that collaborations with companies whose core business is the making of catalysts would be most valuable has an important element of truth.
- The collaboration with LANL continues to be productive.
- The process of technology transfer is not clear. When would such technologies be available in commercial components and integration in stacks?
- The results are very favorable – BNL should seek qualification by a component developer/supplier.
- There was no calculation or projection of comparative costs – a succinct statement of cost impacts would be helpful for further confirming relevance and commercial importance.
- The apparent collaboration with 3M, GM, and Plug Power is encouraging. These entities will maintain an "applied" science perspective which will ensure commercial relevance.

Question 5: Approach to and relevance of proposed future research

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

- It appears that the PI and his team have a reasonable plan for the future.
- Would be nice to include more FC test data and durability studies in MEA configuration.
- The steps forward are acceptable; but a further push to further verification in functional stacks with "established" fuel cell companies are important.
- These market demonstrations are needed to show reduction to practice for these academically exciting developments.
- Future emphasis on stabilization by gold clusters is appropriate, as this surprising effect has a "too good to be true" aspect and must be checked very carefully.
- Plan to concentrate on systems that improve the activity per total mass of all precious metals is wise.
- It would be good to maintain a fair amount of the emphasis on fundamental understanding that has yielded substantial progress to this point, even as this type of catalyst gets closer to real application.
- A clear vision of future work was not presented.
- The proposed future research path appears rational. Longer term stability testing is warranted. Hopefully, this work will include determination of any degradation by-products and if and how the by-products might leave the cell.

Strengths and weaknesses

Strengths
- Good fundamental understanding.
- Strong fundamental work.
- The study has led to appreciable progress towards DOE goals in reductions in Pt metal loadings, increases in activity and durability, as well as novel structures.
- The work presents a good balance between experimental data and plausible explanations of structure-property relationships, including modeling of certain kinetic pathways.
- The finding re: Au nanocluster effects on Pt activity and durability is intriguing and must be further explored.
- The project has generated ideas and followed those ideas to new catalysts that give very encouraging results.
- The project has productively blended theory and experiment.
- The project has successfully brought ideas forward from single crystal experiments to real supported high-area catalysts in a short time.
- This project offers critical confirmation that DOE's targets for electrocatalysts are achievable.
- The technical strength of this project is self-evident.
- Collaboration with LANL in the past on MEA testing of the materials developed as part of this project was beneficial for validation and application demonstration.

Weaknesses
- Not sufficient technology transfer.
- Given the exciting progress on structure developments, there needs to be a greater urgency to "reduce to practice" by scaling up these structures for use by some fuel cell stack developers.

- More collaborations are needed to show whether the promise of these findings could be translated into the commercial marketplace.
- The effects of contaminants have to be further explored and more external collaborations could lead to such experiments.
- In the past, substitution of Pd for Pt was perhaps perceived as more of an advance than it really would be; this situation appears to have been rectified now.
- Make sure that the reference data is as solid as the data on the innovative systems.
- Avoid drawing conclusions from RDE data at too low potentials where the mass transport correction is large.
- The presentation slides in general contained too much information, and were very difficult to read – use one plot on a slide instead of three.
- Basic material considerations were not adequately explained, i.e. Alloying element choices were not rationalized, and the results were not fully reconciled with theory.
- Test data on operating prototype stacks, effects of commercial grade fuels and the impact on fuel cell cost should be included in the approach.

Specific recommendations and additions or deletions to the work scope

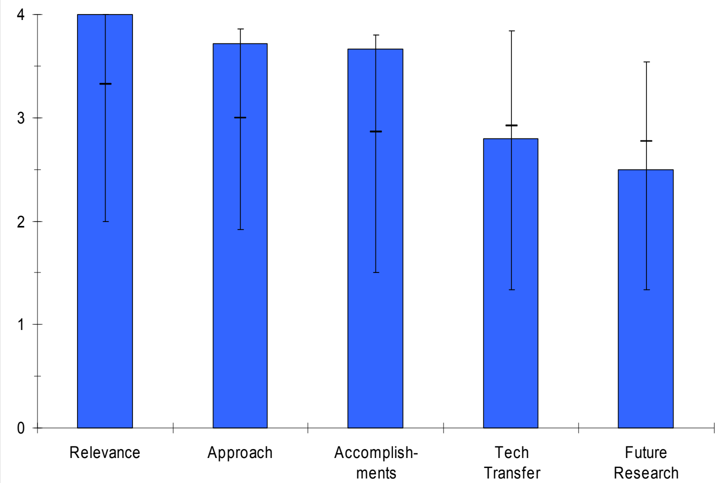
- It should include closer integration with others.
- Check on the structural and functional stability of these novel structures in real fuel cell stacks with real-life feeds of hydrogen and air (or even reformate feeds.) The effects of contaminants must be considered.
- Higher temperature operations, including cycling at higher potentials as well, could provide insights into whether such novel structures exhibit structural stability and durability.
- This project has enough useful cathode leads to follow to keep it very productive.
- Progress in transportation fuel cells requires that the best electrocatalyst people concentrate on cathodes; avoid being overly distracted by the scientifically fascinating but technically less critical issues on anodes.
- If a cost assessment for Pd_3Fe with Pt monolayer is compelling, this work should be highest priority.
- The results with gold alloying are very convincing, and these should be reconciled with theoretical calculations.
- The work on DMFC materials is defocusing, and should be re-examined in terms of relevancy, especially in light of FY 2006 budget considerations.
- Expand the operating temperature range for the catalyst. Temperatures ranging from -40 to 120°C will cover the present envisioned PEM range.

Project # FC-10: New Electrocatalysts for Fuel Cells

Phil Ross; LBNL

Brief Summary of Project

Overall Project Score: 3.5 (6 Reviews Received)



This Lawrence Berkeley National Laboratory (LBNL) project is developing new catalysts for both anodes and cathodes following a unified concept of platinum group metal (PGM)-based bimetallic nanoparticles with a "grape" structure (a PGM "skin" with base metal core). The choice of PGM and core metals for the anode and cathode is based on computational screening of PGM core-shell nanostructures using newly developed (under BES funding) Monte Carlo simulations. LBNL is pursuing new synthetic chemistry to synthesize nanoparticles with a "grape" structure, continue focus on Re as metal core with Pt and Pd as PGM, optimize AuPd as an alternative to Pt in anodes, and conduct fundamental studies of the crystallite size effect for the oxygen reduction reaction in acidic electrolytes on carbon supported Pt and Pt alloy nanoparticles.

Question 1: Relevance to overall DOE objectives

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

- There is no doubt that improvements in electrocatalyst activities are critical to cost reduction of catalysts for fuel cell stacks.
- This project directly addresses routes to the improved cathode catalyst activities that are central to attaining DOE's fuel cell cost targets.
- The development of improved (higher activity, longer life, and lower cost) electrocatalysts is imperative for fuel cell commercialization in automotive applications.
- Improving the activity and stability of electrocatalysts is very important, especially to meet automotive targets.
- Fundamental project critical for further improvement of the state of the art in ORR electrocatalysts.
- This type of project is highly relevant to achieving DOE and industries goal towards commercialization of PEM fuel cells.

Question 2: Approach to performing the research and development

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

- This project has done very fundamental research that very astutely focuses in on the central problems in fuel cell electrocatalysts, so it should have a high level of impact in industrial systems.
- An excellent blend of careful experimentation and theory (both within the project and drawn from the literature) has contributed to the success of the project.
- The objectives of the work are very clear, but how they are being realized was not clearly articulated.
- The explanation of analytical methods (Auger, Ion Scattering) was very lucid.
- Details of the annealing process, including photos of test articles and equipment, as well as processing conditions and timescales, were not elaborated.
- The first half of the talk focused on Pt_3Co, but the second half on Pt_3Ni – the difficulty of preparing the latter should be explained.

- There was mention of Monte-Carlo calculations – more explanation of this study, which is critical for reconciling theory and results, is requested.
- An excellent mix of theory and experimental work, which leads to true fundamental understanding. Phil's work on single-crystal model systems has been very useful in furthering understanding.
- Combination of modeling and single crystal studies combined with characterization of supported catalysts.
- The approach generates interesting results. The possibilities are intriguing. However, nothing was indicated on how this work might be commercialized.

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

This project was rated **3.7** based on accomplishments.

- Project is coming to a close, but the closing activities are still very good. A final conclusion slide would have been very helpful for a lengthy research project.
- The alloy single crystal activity and characterization results vividly point out important pathways to higher activities in practical catalysts.
- Confirmation of structural changes at surfaces combined with excellent data were extremely compelling.
- The Pt_3Ni system and its performance offers tremendous promise for achieving DOE's automotive targets for catalytic materials in PEM.
- In addition to surface analyses, a full compositional analysis would be beneficial.
- Only real new result shown was the PtNi results. Nevertheless, these were quite impressive. Understandably, the emphasis for the past year has been in documenting the work that has been done.
- The demonstrated 10-fold improvement of mass activity as function of the type of crystalline phase.
- The progress made appears to be quite significant.

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

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

- The only criticism of this project is that it would have been great if the project had more catalyst industry partners. The real technology transfer is in the research staff joining industry and another National Lab.
- Collaborations with MEA makers, fuel cell developers, and automotive OEMs have been highly effective.
- Catalyst makers have been influenced by this project's publications and presentations but might have benefited from more direct collaborations.
- There was mention of collaborations with GM and 3M, but it seemed to have occurred in the past. What is the outlet for the latest results? What work is needed to validate the technology, scale-up, and realize commercial components?
- Third party assessment of costs would help establish the value of the developments.
- There is some interaction with industry (e.g., GM), but more interaction with catalyst companies vs. end users would be beneficial for an academic program working on electrocatalysts.
- Two industrial "partners" were mentioned, GM and Cabot. No information was supplied to indicate any active collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Plan to get everything written up is good.
- PI's plan to retire will leave a big hole in the fuel cell community.
- It is to be hoped that PI's retirement will include some continued exciting science – now fully on his own terms.
- This project is wrapping up; however the study of the Pt_3Co system, in analogy to the Pt_3Ni system, should be more fully investigated.
- It would be disappointing if research in this area was decreased or side tracked. Some indication of how a future PI might proceed would have been appropriate.

Strengths and weaknesses

Strengths

- The findings are impressive. The activity improvements are very good. The development and research process is quite logical.
- The project is characterized by careful experimentation on very well-controlled systems.
- This work has blended experiment and theory in an unusually effective way.
- The project has created insights that are critical to the development of more active and stable supported electrocatalysts.
- The objective of realizing optimal utilization of precious metals is exceedingly clear, and the latest results confirm the achievability of this goal. This offers high confidence that DOE targets will be met.
- Excellent mix of theoretical and experimental work. Elegant experimental results. Advancement of fundamental understanding.
- Technical expertise and innovative analytical techniques.

Weaknesses

- It is a little disappointing that this project did not have catalyst manufacturers involvement. GM and 3M are not catalysts suppliers. However, it was good to see one researcher joining Cabot Superior MicroPowders so one will assume some technology transfer will occur.
- The recent work of the project has not carried the single crystal ideas all the way through to high-surface-area supported catalysts.
- There is sound experimental and theoretical work published in technical journals, and very exciting results in terms of reduced costs, but the pathway to a commercial outlet is not at all articulated.
- Lack of proposed follow-on research.

Specific recommendations and additions or deletions to the work scope

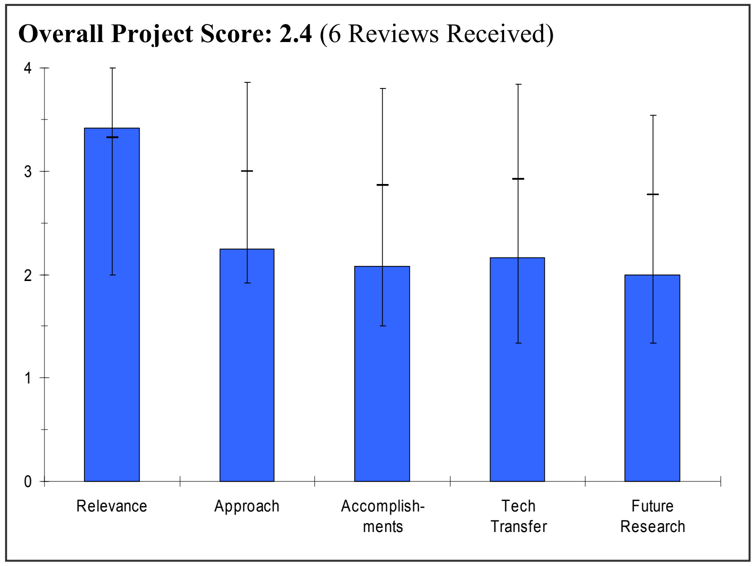
- I assume that Dr. Ross will be retained as a consultant and this should be very helpful for the technology transfer part.
- Explore ways to produce Pt-alloy particles with control of exposed crystal faces in high-surface-area catalysts.
- Develop characterization tools capable of determining surface structure (nanoparticle habit) and 2nd-layer composition for high-surface-area catalysts.
- Preparation of Pt_3Co in analogy to Pt_3Ni is the logical next step.
- Analysis of comparative costs (including materials and processing steps) would help reinforce relevancy, and inspire next steps toward adoption by component developers.

Project # FC-11: Development of transition metal/ chalcogen based cathode catalysts for PEM fuel cells

Stephen Campbell; Ballard

Brief Summary of Project

Ballard Power Systems is developing a non-precious metal cathode catalyst for PEM fuel cells that will be as active and as durable as current PGM-based catalysts, at a significantly reduced cost. This project develops composition using sputtered thin films, develops dispersed, supported catalysts, and evaluation/demonstration in fuel cells and stacks.



Question 1: Relevance to overall DOE objectives

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

- This project sought an active, stable Pt-free cathode catalyst which could solve the cathode catalyst cost problem.
- Whether DOE should have put so much money into completely Pt-free approaches is questionable, but is probably outside the scope of this review.
- Project is well focused on DOE goal to identify less expensive catalysts for PEM fuel cell. Unclear how targets A, B and C can be achieved
- This project is very relevant to the objectives of the HFCIT program of addressing the key issue of cathode electrocatalysis.
- Alternative catalysts are important.
- PGM reduction is a key need and this project addresses the need.

Question 2: Approach to performing the research and development

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

- This project is neither fundamental enough nor applied enough to have maximum impact – the middle ground has not proven particularly fertile here.
- Testing was done in sulfuric acid rather than in more weakly-adsorbing perchloric acid, which is much more relevant to PEM fuel cells.
- Project approach is suitable for the planned goals. Unclear why switching to ternary system should raise OCP.
- This project appears to be well suited for a design of experiments approach.
- Chalcogenide catalysts studied in this project appear to lack required performance and performance stability. This approach shows little promise for reaching performance targets..
- At this stage of the project, testing should have been also carried out at higher than ambient temperatures.
- This project is unlikely to yield useful results.
- The justification for selecting the materials was not explained.

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

This project was rated **2.1** based on accomplishments.

- The project has demonstrated good stability, if not particularly impressive catalytic activity, for several new materials.

- Little progress in identifying promising systems.
- The results are not commensurate with the amount of funding spent. More materials systems and some degree of accelerated testing of the electrocatalyst would benefit this project.
- Although a fully unambiguous evaluation of the catalysts' performance is not possible in the absence of current density data, the results presented by the PI indicate poor ORR performance of the catalysts.
- Stability of the catalysts is unsatisfactory.
- Demonstrated increase in the OCP values is a plus.
- All results to date have shown no measure of success with the chalcogenide catalysts. The project objectives appear to be in a constant state of flux. The work is mainly (90% according to the presenter) carried out at UBC/CWRU, and it is entirely unclear what Ballard has contributed.
- This is a difficult challenge. An explanation of stability data on CoS_2 would be beneficial.

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

This project was rated **2.2** for technology transfer and collaboration.

- The work seemed to show a good working relationship between UBC, CWRU and Ballard.
- Will these results be used by anyone?
- This work may have provided useful background for Ballard's proprietary work on precious-metal-enhanced catalysts using materials such as these as supports, though from the outside it's not clear which effort made the real innovations.
- Good collaborations with Universities.
- Roles of the participants was not defined nor mentioned during the presentation. There is no indication of what CWRU and UBC bring to the project.
- The roles of the two partners and that of Ballard's have been unclear. More and stronger partners would help the progress.
- In view of the poor results to date and the unlikely development of a useful chalcogenide catalyst there isn't anything to transfer at present.
- Good collaboration with Universities.

Question 5: Approach to and relevance of proposed future research

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

- Either more fundamental mechanistic work or a shift to the addition of very small amounts of precious metals might be more productive than the planned continuing tweaking with non-precious-component ternary systems.
- Future efforts should include identification of catalyst active sites.
- Given the enormous challenges that the project is facing, the presented research plan does not guarantee sufficient progress in the future.
- No sensible approach other than trying some of this and that are offered. Trial and error without a sound basis of understanding can only lead to continued lack of progress.
- Is the investigation on binary systems exhausted such that it is necessary to introduce a third variable?
- Go/No-Go point is useful. Besides the OCV, other criteria were not identified.

Strengths and weaknesses

Strengths
- The project gave evidence of careful experimentation.
- Development of procedure for particle size distribution from HRTEM images.
- Interesting approach.
- The project takes on much needed work.

Weaknesses
- Slow progress in getting new experimental results.
- Results less than expected for the amount of funding allocated. This project would benefit from accelerated testing tied to better TEM to look at the catalyst stability.
- Path forward is unclear.
- No peroxide formation data.
- Why worry about normalizing the activity of non-PGM materials with respect to active area, especially where the mechanism is not the objective? The ultimate goal is activity greater than that achievable with Pt.

Specific recommendations and additions or deletions to the work scope

- Consider dropping the plans for additional non-precious-component ternary work in favor of adding a small amount of a precious metal and looking for strong metal-support interactions, if this doesn't overlap excessively with proprietary projects.
- Complement RDE measurements with RRDE measurements. Evaluate volumetric activity in fuel cell and compare with DOE target even if OCP is low
- This project is in a dire need of a major breakthrough to justify further funding; it should be terminated otherwise. There should be no stack testing at this stage.
- End the project.
- The characterizations did not appear to explain the data or direct future plans.

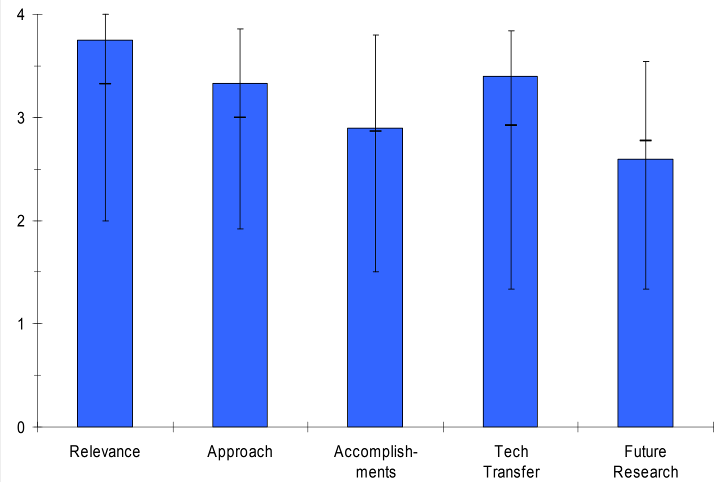
Project # FC-12: Novel Approach to Non-Precious Metal Catalysts

Radoslav Atanasoski; 3M

Brief Summary of Project

This 3M project is developing and demonstrating non-precious metal cathode catalysts to lower cost (goal of 50% less vs. target of 0.2 g Pt/peak kW) and to reduce the dependence of proton exchange membrane fuel cell catalysts on precious metals. 3M is identifying opportunities for system cost reduction through breakthroughs in the catalyst area of fuel cells, utilizing cost-effective, scalable fabrication processes. Sample tasks include investigation of Fe-N-C as a model catalytic site, vacuum 1- and 2-step synthesis processes, combinatorial approach in identifying potential catalysts, nanotechnology processes, and fabrication and characterization of MEAs.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project is part of DOE's attempt at finding a stable, active Pt-free catalyst that could remove any concerns about catalyst cost.
- The project is relevant to the RD&D plan in the context of discovering new alloys that improve catalyst activity and durability.
- The project is of paramount importance to realization of the DOE 2010 target goals for the design of novel high surface area non-precious nanostructured cathode materials.
- Catalyst cost reduction is one of the key DOE targets.

Question 2: Approach to performing the research and development

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

- This project has followed logical approaches to the development of active non-Pt catalysts and has nicely benchmarked activities against both Pt and state-of-the-art non-Pt systems, though see the box below for a qualifier regarding a Tafel extrapolation.
- The use of 50 cm^2 MEA's in early testing lends an important element of reality to this project which is often lacking in non-Pt catalyst work.
- The low surface areas available from the vacuum approach draw into question its applicability to non-Pt systems, even though significant relative progress (10^3 fold) has been made.
- The approach is rather broad and loosely defined (i.e., "vacuum processes" and "nanotechnology"); consequently it is difficult to gauge the technical value of their approaches. The researchers appear to be well aware of the technical targets and focused on meeting them.
- It is very difficult to judge the technical merits of the approach when the materials are named "A, B, and C". The RRDE measurements should be used to screen the materials prior to fabricating them into a full MEA, rather than vice versa. With the current approach, some materials may be disregarded due to poor activity caused by intricacies in making an MEA rather than being judged on intrinsic catalyst activity.

- The PI used state-of-the-art analytical tools of surface preparation and surface analysis to form well characterized surfaces.
- The PI uniquely combined modeling and UHV studies to illuminate the electronic effects.
- The rotating ring disk electrode method is important addition to a real fuel cell testing.
- The PI must find correlation between physicochemical properties of surface atoms and stability/reactivity in an electrochemical environment under PEMFC operating conditions. To stimulate and complement the experimental studies, the PI must further develop theoretical and/or computational methods, based on reliable quantum chemical means, to perform band-structure calculations.

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

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

- Good progress has been shown in somewhat exceeding state-of-the-art non-Pt activities in full 50 cm^2 MEAs, though no major advancement of the field has yet been made.
- Use of a Tafel slope extrapolation to give a pure kinetic activity at 900 mV seems a bit questionable (if you're already not just kinetically limited at 900 mV, the catalyst has serious other problems).
- Overall, this project was a good effort on a very challenging topic. Progress was made without the ability to obtain significant interaction with an experienced collaborator with whom they had planned to work (through no fault of the project organizers).
- Actual materials not presented, which complicates understanding of the issues and results. Considerable performance loss over the first ten hours with the "nanotechnology" catalysts. Fuel cell derived results are for 58 psig oxygen, which is generous compared to conventional conditions. New higher surface area substrates are intriguing, but it is difficult to appreciate their importance with so few details. Considerable progress towards the activity targets have been accomplished, but durability continues to be a discouraging aspect.
- The interim performance milestones of 0.08 A/cm^2 at 0.6 V and 0.1 A/cm^2 at 0.7 V are too low for a project that has been funded for since 2003 and to meet the 2010 DOE targets.
- The IR correction for Figure 10 is 35 mV at 0.032A/cm^2, corresponding to an area specific resistance of 1.1 ohm cm^2. Normal fuel cell resistance would be <0.2 ohm cm^2. If this could not be lowered, it would prevent attainment of stack efficiency targets.
- Performance on catalysts has improved from the previous report.
- Production of peroxide is too high and may affect both stability of the membrane and catalyst itself.
- These types of catalysts are inherently unstable in acid solution and the project needs a stronger durability component.
- To advance the catalytic/stability performance the nature of active sites must be illuminated in more details.

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

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

- Effective collaborations are in place with National Labs and universities; since the PI is in a company with diverse fuel cell capabilities; many industrial bases are already covered.
- Technology collaborations are strong.
- Technology transfer is not applicable for this project as 3M has the capabilities for commercialization. Good collaboration with universities and Brookhaven.
- The PI has clearly indicated contribution from 3M and from collaborators/partners. 3M is uniquely positioned to transfer the knowledge from fundamental studies to the "real" products.

Question 5: Approach to and relevance of proposed future research

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

- Plans seem basically good, but it is not clear that plans are in place to increase durability of state-of-the-art non-Pt catalysts.

- Future work primarily involves understanding current materials (e.g., characterizing active sites). Paths for improving performance are not described. Durability is a critical issue, but is downplayed in the proposed future research.
- The goal of the future work should not be to achieve the 2005 DOE targets!
- Activity still requiring 2.6x improvement for 2005 target and 7x improvement for the 2010 target suggests that surveying other approaches is needed rather than optimizing the current system.
- The proposed future research is rather general. The PI needs to identify, more precisely, what experimental and theoretical methods he is planning to use in order to resolve stability and activity issues.

Strengths and weaknesses

Strengths

- The project has demonstrated the ability to make 50 cm^2 MEAs out of this challenging class of non-Pt catalysts.
- From its slow start, the project has shown major improvements by reaching the state-of-the-art for this type of non-Pt catalyst.
- The project has strong contributions from the collaborators.
- The collaborative team and characterization of the materials using spectroscopy.
- A comprehensive approach covering all aspects needed to create, characterize, control and understand a new generation of non-precious nanostructured cathode materials for low temperature fuel cells.

Weaknesses

- The very significant improvements made in the vacuum approach may have been good effort wasted in a direction that can't work with non-Pt systems due to the small surface area available.
- It's not clear that the project has yet advanced significantly beyond the non-Pt state-of-the-art (which was real data, not an extrapolated Tafel line), either in activity or in communicated new insights; it may now be poised to do so.
- Durability is at least as important as activity but receives significantly less coverage.
- Extremely low catalytic activity even after working since 2003 on this class of catalysts.
- These types of materials are very unstable in hostile electrochemical environments and the success of this project relies more on stability of catalysts than on catalytic activity. The amount of peroxide produced during the ORR is too high and must be completely eliminated.

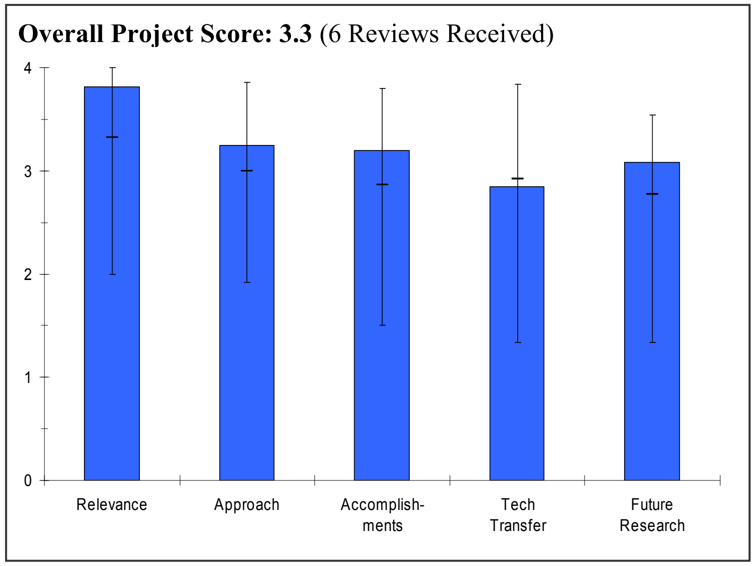
Specific recommendations and additions or deletions to the work scope

- Increased attention to durability issues, even while the activity is still low, could accelerate long-term progress.
- Need basic understanding of what is limiting the performance of this class of catalysts.
- Bring in organometallic chemists to supply ligands which might be added to, or replace some of the nitrogen from the existing C/N matrix to tune electronic, steric factors.

Project # FC-13: Novel Non-Precious Metals for PEMFC: Catalyst Selection Through Molecular Modeling and Durability Studies

Branko N. Popov; University of South Carolina

Brief Summary of Project



The University of South Carolina is synthesizing novel non-precious metal electrocatalysts and metal-free electrocatalysts with similar activity and stability as Pt for the oxygen reduction reaction (ORR). The PIs are focusing on high activity for the ORR, mass production methods, corrosion resistance, low cost, and improved understanding of reaction mechanism of oxygen reduction. The active reaction catalytic sites are optimized with respect to carbon support, presence of surface oxygen groups, nitrogen content, surface modifiers, pyrolysis temperature, porosity, pore size distribution and the concentration of the non-metallic additive "X" in the catalyst matrix. Supporting tasks include theoretical molecular modeling, electrochemical characterization, structural studies (XPS, EXAFS, XANES), identifying the correlation among the catalyst composition, heat treatment and catalytic sites for oxygen reduction, and demonstrating the potential of the novel non-precious electrocatalysts and the metal-free catalyst as substitutes for Pt currently used in membrane electrode assemblies.

Question 1: Relevance to overall DOE objectives

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

- Development of new catalysts addresses the two key issues of fuel cell cost and durability.
- Through addressing the key issue of cathode electrocatalysis, this project is very relevant to the program objectives.
- The project is a necessary step for moving away from Pt and lowering cost.
- Outstanding.

Question 2: Approach to performing the research and development

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

- The initial approach, based on electrochemical characterization, has been logical and technically sound.
- With only a little more than one year left in the project, the PIs need to down-select and focus on the most promising pathway, and to put additional emphasis on catalyst stability.
- Research focuses on interesting materials, especially metal-free carbons.
- Testing the effect of several different factors on the performance of the Co-based catalysts has revealed interesting properties of the catalysts in the Co group.
- There seems to be little, if any, benefit from the modeling component in this project.
- Given poor "real-time" stability of the catalysts, there is no need for accelerated durability testing at this stage of the project.
- Good systematic approach that includes support in modeling and structure at other universities.
- It could be more efficient to focus on one approach. Metal-free catalysts, if not active enough, have no special attraction in this scenario.

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

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

- Significant progress has been made during the past year. Activity and selectivity has been improved greatly.
- Progress to date does not suggest that the targets will be achieved in the remaining timeframe of the project.
- Initial performance of several materials studied in this project is promising.
- Neither Co/C nor Co-X/C catalysts show sufficient performance durability.
- Effect of surface modifiers needs to be demonstrated using unmodified surfaces as a reference.
- Activating carbon by nitrogen incorporation appears to be a promising endeavor. It is unfortunate that the nature of "X" could not be communicated. The strong reduction of the peroxide generation is impressive.
- Very systematic narrowing of materials and optimization has led to good progress in activity.
- The activity achieved is very impressive. T he durability needs to be addressed.
- The activity of Co-based catalysts has been increased but it is still considerably lower than that of Pt/C. The number of electrons per O_2 should be calculated at higher E from Koutecky-Levich plots. At 0.5 V, a diffusion control is operative and at such over potential it is easy to get a 4-e^- reduction.

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

This project was rated **2.9** for technology transfer and collaboration.

- The modeling and structural studies with Case Western and Northeastern complement the experimental work well.
- More industry involvement is recommended to develop synthesis processes amenable to large-scale manufacturing.
- Benefit from the modeling done at CWRU is unclear as is the role of Northeastern University in this project.
- Industrial/National Lab Involvement would help the progress of the project.
- An industrial collaborator is needed to help this project to establish benchmarks and guidance on development.
- Collaboration with other universities seems to be useful and supportive.
- Developments are still in early stages for tech transfer.

Question 5: Approach to and relevance of proposed future research

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

- Proposed approaches to improving the durability of Co-based catalysts are insufficient. In particular, without a better understanding of the causes for the observed fast catalyst performance degradation, it is not obvious at all why supporting catalysts on graphitized carbon should help the stability (applies also to metal-free catalysts).
- Well formulated approach to developing the concepts further.
- Future plans to support durability and MEA optimizations are supported by the data.
- Not clear why ZrO_2 encapsulation is proposed?
- Should focus on selected systems and fewer variables.

Strengths and weaknesses

Strengths

- The initial approach, based on electrochemical characterization, has been logical and technically sound.
- The project team is highly qualified and experienced in carrying out the electrochemical characterizations.
- Selection of uncommon catalysts, metal-free in particular, represents a strength of the project.
- USC is building a good knowledgebase on Co and metal-free catalyst work.
- Interesting innovative approach.
- Systematic approach and good progress. Very organized presentation.
- Synthetic effort seems good.

Weaknesses

- Durability studies have been rather limited – much more is needed.
- Poor stability of the catalysts and no clear path towards improving it.
- The Xs need to be revealed to allow better technical evaluation of this project in the future.
- Contrary to the PI's claim, 5% peroxide yield is too high to be practical for PEFC systems.
- It is not clear in the presentation regarding how this project is making measurable progress toward the contribution of lowering costs, increasing durability, etc. of the fuel cell stack.
- A better selection of variables for optimizing of catalysts seems necessary.

Specific recommendations and additions or deletions to the work scope

- More industry involvement is recommended to develop synthesis processes amenable to large-scale manufacturing.
- With only a little more than one year left in the project, the approach needs to down-select and focus on the most promising pathway, and to put additional emphasis on catalyst stability.
- Unless the PI can demonstrate a major improvement to the durability of Co-based catalysts, the "Co part" of the project should be deleted from the scope.
- Catalysts need to be tested for possible Pt presence, especially following the so called the "post-treatment". The nature of the "post-treatment" should be revealed.
- Catalysts testing at above-ambient temperatures is needed.
- It would be nice if all the researchers working with Co as a catalyst will collaborate with each other. It appears that they are sometimes repeating each other's work to learn similar lessons. Fewer presentation slides and a few slides on how this research is making measurable contribution toward reducing cost and durability of the stack year to year would be appreciated.
- Expand modeling efforts.
- Recommend continued support.
- More insight into the extent and impact of changes in MEA/cell design would be desirable.

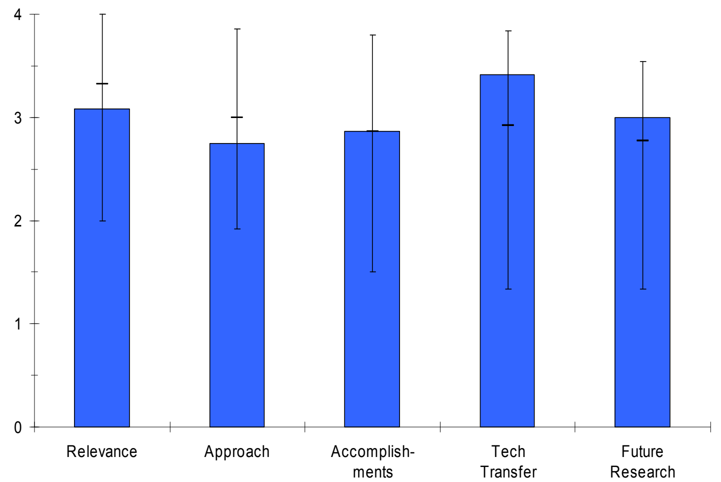
Project # FC-14: Non-Platinum Cathode Catalysts

Piotr Zelenay; LANL

Brief Summary of Project

This Los Alamos National Laboratory project on non-platinum catalysts for polymer electrolyte fuel cells focuses on the fuel cell performance, performance stability and the mechanism of oxygen reduction reaction (ORR) on non-precious metal/heterocyclic polymer composites, such as carbon-supported Co-polypyrrole composite. The project also targets the design, synthesis and fuel cell testing of surface chalcogenides, in particular Se-decorated Ru nanoparticles (Se/Ru), including replacement of ruthenium by non-precious metals in the core of the nanoparticles.

Overall Project Score: 3.0 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- This project went after the Holy Grail of fuel cell research, a durable non-Pt cathode catalyst which could potentially reduce fuel cell costs.
- It is unlikely that the no-Pt, rather than low-Pt, route is more efficient route to acceptable fuel cell catalyst costs, but the decision to pursue this path was DOE's, not that of the organizers of this project.
- The high-loaded (3mg/cm^2, replacing 0.4mg/cm^2 Pt) Ru on chalcogenide catalyst part of this project is inconsistent with the intent, if not the exact wording, of DOE goals for direct H_2 fuel cells, as it would lead to a net increase in cathode catalyst costs once Ru prices were driven up by the development of a significant new use for that metal; it makes some more sense for direct methanol fuel cells.
- Zelenay et al. are making progress on developing non-platinum catalysts through two approaches – Co-N and RuSe complexes. Although RuSe is a non-platinum compound, it still contains a precious metal (Ru) so it is not clear if the project is truly focused on low-cost, available catalysts.
- Co catalysts, if successful, would remove one of the cost constraints.
- The project objectives, if successfully met, would definitely assist in meeting the hydrogen vision.
- With the price of Pt and its availability and the price target of FCs to be competitive with ICEs, Pt replacement is a must.
- Project addresses DOE goal to design low cost non-precious metal catalysts for the oxygen reduction reaction.

Question 2: Approach to performing the research and development

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

- Approach for the metal/conducting polymer/carbon catalyst was generally good, though plotting W/cm^2 is a bit misleading when the maximum value occurs at such a low potential that the cell efficiency would be unacceptable, and were the films really as thin as claimed? For such systems, one always wants to actually measure the electrode layer thickness.
- Approach for the Ru/chalcogenide system (previously the subject of considerable study) still hasn't broken free from its direct-methanol roots – it seems improper to use 10x as much of another platinum group metal to replace 1x Pt for a H_2/air fuel cell.

- Ru/chalcogenide data should be reported against the A/mg precious metal target instead of against the A/cm^3 non-PGM target, as Ru price would rise considerably with any high-volume use.
- The overall approach of the LANL team is thorough, combining physical and electrochemical characterization. The main challenge with their approach is distinguishing themselves from the many other researchers who have tried similar materials. For instance, Zaikovskii et al. published a report this year in J. Phys. Chem. B ((vol. 110, p. 6881) on RuSe nanoparticles, with an approach and results almost identical to that of the LANL team. There are numerous other reports on metal and metal oxide – PPY complexes (see for instance Nguyen Cong, El Abbassi, and Chartier, JECS, 220, v 149, p A525).
- The LANL project never really explains how their program is different from the other publications on related materials. More importantly, all of the related papers are stuck in activity about where LANL is – how exactly do they plan to make up the other 70% of activity needed to reach DOE goals? They have one RuSe material that is Fe-doped, but Fe is not a good idea for fuel cells due to its propensity to catalyze the Fenton reaction.
- The rating of only a 2 does not reflect in anyway the quality of the work being done. It is meant as a reality check on the difficulty of trying to develop a NPM catalyst that is equivalent to Pt in all the ways required for commercially successful components. Some aspects of the research have a very good probability of adding to the general knowledge that will ultimately overcome some of the barriers in time to be effective.
- The approach is good.
- Project approach is very well thought out.

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

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

- The durability of the 10%Co-PPY-XC72 would appear to represent an advance over the typically dismal stability of non-Pt cathode catalysts, but the experiment should have been performed at a practical voltage at or above 0.6V instead of 0.4V – durability can depend quite strongly on potential.
- The claim of a high A/cm^3 value for the Ru/chalcogenide catalyst is inappropriate in spirit if not in exact wording of the targets, as this target was intended for catalysts containing only non-precious components and the quoted achievement was for a high Ru loading.
- The A/cm^3 for the Co-PPY-XC72 is a modestly good achievement in a short time, particularly if the film thickness was accurately estimated as ~10 microns. Was the loading $0.2 mg/cm^2$ of Co, or $0.2 mg/cm^2$ of the complete Co+polymer+carbon catalyst? If the former, the film was probably much thicker than the <10 microns orally quoted in response to a question. The open XC72 structure typically sets the electrode layer thickness, and 0.4mg XC72 typically gives a 12 micron thick layer. $0.2 mg/cm^2$ Co in a 10% Co catalyst would have $2 mg/cm^2$ total catalyst, of which say 1 mg is XC72, which would give a 30 micron layer thickness. If the layer is actually 3 times thicker than assumed in the A/cm^3 calculation, then the quoted A/cm^3 would be three times too high. 4.9/3 = 1.6 which is still near state-of-the-art non-Pt A/cm^3 of 2.8.
- LANL has a mixed bag of accomplishments. They cite materials stability, but they do not do rigorous cycling of their materials, such as by pushing to 1.4 or 1.7 V in the RDE (to replicate the environment that the cathode sees when an actual fuel cell is switched on). The materials are stable within the demonstrated conditions, but it's not clear if they are stable beyond that. Otherwise, they have shown improvement in the materials that they characterized.
- Considering project cost and age, good results have been obtained.
- Again, the low rating does not imply a less than stellar rate of progress towards the original objectives. The LANL group has made outstanding progress and their work continues to be very solid and reliable. But the exact wording of the rankings does not allow giving a higher number since the barriers to replacing Pt are extraordinarily high.
- The project has brought more focus this year. However, the success with the Co should be exploited further while the Ru work should be focused on decreasing the amount of Ru to below $0.01 mg/cm^2$.
- Project shows significant accomplishments concerning nanocomposite stability and durability.
- Good progress in catalyst characterization.

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

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

- The technology is at a far too early stage of development to be transferred, so the lack of technology transfer beyond the partners (which includes and industrial partner for the DMFC work) is not a cause for concern.
- This is a highly integrated program. LANL, in general, is very good about collaborating and sharing data.
- Great team.
- Perhaps the fundamental work on the Ru side has to be left to Universities.
- Good collaboration with universities. PI needs to increase collaboration with industry.

Question 5: Approach to and relevance of proposed future research

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

- Future plans look OK, though DOE might consider retargeting the project to the likely more productive direction of very low PGM-loaded catalysts.
- The LANL team is heading down a fairly well traveled path. It's not clear how they will move ahead where others have failed.
- Ru catalysts are unlikely to exceed the performance of Pt, and with similar limited supply as Pt, are unlikely to surpass Pt in cost/performance.
- Proposed Co work is in less-explored area with greater scope for breakthrough.
- Recommend trying as quickly as possible, to establish fundamental limits of any of their approaches. These entitlement values (whether ORR activities, or mass transport over potential, or peroxide decomposition susceptibility, or toxicity, etc.) should be used to prioritize the likelihood of success of the approaches, and any related technologies the research community may be trying to develop.
- Prioritization is needed. Focus on Co and other transition metals, less on Ru.
- Future work focused on DOE goals.
- DOE targets for non-precious catalysts represent a difficult technical challenge.

Strengths and weaknesses

Strengths
- Some nice materials were developed in the Co/polymer/carbon composites.
- Highly talented, able team.
- The LANL group continues to be a major source of excellent research. They should continue to focus whenever possible more on the fundamentals of the barriers facing catalyst development, with a keen eye to what material systems have the most likelihood of meeting all the 10-12 requirements for a successful electrocatalyst.
- Good novel approach, promising results on the Co side.
- Strong combination of characterization methods for catalyst evaluation.

Weaknesses
- It would be good to pay attention to the intended spirit of targets and not just the literal meanings of the words -- we should all be able to recognize the difference between a precious metal-based target and a target intended for systems that are essentially without cost compared to PGM's.
- For non-Pt work in particular (and comparison to the A/cm^3 target), it is very important to measure the actual thickness of catalyst layers.
- No overt weaknesses, but philosophically, if they were to "try to develop catalysts at a National Laboratory with the preconceived notion that they would lead to a commercialized product," that would detract from their strengths.
- Reporting performance below 0.6 V (0.5V for nonprecious metal catalysts), durability included, is useless. Measure the actual thicknesses of the cathode layers to calculate the current densities.
- No collaboration with industry to evaluate catalysts in full-size fuel cells.

Specific recommendations and additions or deletions to the work scope

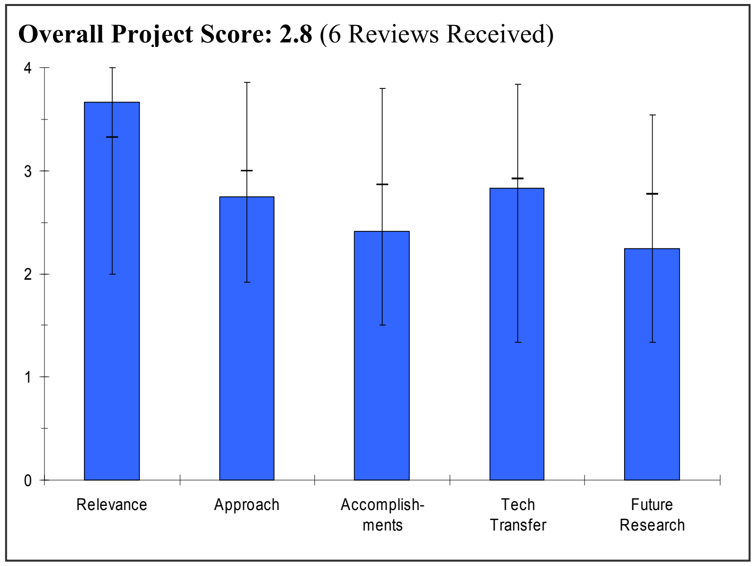
- Unless Ru loadings for Ru/chalcogenide system can be reduced more than 10x, consider abandoning it for direct H_2 systems.
- High Ru/chalcogenide could make sense as cathode for direct methanol if a membrane with low MeOH crossover could be developed; without this, a low selectivity for methanol oxidation just means unacceptable methanol emissions in the cathode offgas, no?
- There are several inconsistencies in the data. Fuel cell measurements on the Co-N complexes are made at 0.4 V, where they are generating peroxide. Such a fuel cell would also only have an efficiency of about 27% so it is not clear why measurements are being done at that potential.
- RDE/RRDE figures should be more clearly and consistently labeled with power density, temperature, electrolyte, sweep rate and reference electrode.

Project # FC-15: Low-Platinum Catalysts for Oxygen Reduction at PEMFC Cathodes

Karen Swider-Lyons; NRL

Brief Summary of Project

The objective of this Naval Research Laboratory (NRL) project is to attain the DOE goals to achieve 0.2 g Pt/rated kW before 2010 by focusing on lowering the amount of Pt in the fuel cell cathode. NRL is using oxide-based supports for Pt and other metals to leverage oxygen dissociation by oxides, metal-support interactions (MSIs) with Pt, and ionic mobility of oxide supports. Objectives for FY 2006 include devising mechanism(s) to explain catalyst activity, and improving synthesis and reproducible materials.



Question 1: Relevance to overall DOE objectives

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

- Project is well aligned to the goal of reduction of PM content.
- This project's objectives cover the critical need to reduce Pt usage in fuel cells.
- Improving the activity and stability of electrocatalysts and lowering Pt loading is very important, especially to meet automotive targets.
- This project supports Pt reduction and therefore cost reduction.
- Very important: Cost reduction. Performance. Durability. All have potential for improvement.

Question 2: Approach to performing the research and development

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

- The proposed idea is novel and has potential, seems that there is some uncertainty in the synthesis methods and in ability to reproduce samples.
- The search for improved cathode catalyst activity through strong metal-support interactions is one of only a few plausible routes to very high mass activities.
- A previous approach of attempting to accomplish significant ionic conduction in the support itself appears to have been properly dropped for the recently-examined materials.
- Specific activity (per Pt area) has now been recognized as a useful parameter in the earliest stages of catalyst development, but it is important not to lose sight of the fact that for precious-metal catalysts the economically-important factor is the mass activity.
- There is not enough focus on trying to obtain a fundamental understanding of mechanisms. There are some proposed mechanisms trying to explain the support interactions with Pt, but it appears to be "waving hands." Admittedly, the ORR mechanism is very complex and difficult to study, which is why one should focus on trying to explain activity improvements based on what is known about ORR catalysts. For example, is there a change in the electronic structure of the Pt or spacing when Pt is put on these supports? A good model in this area is Dr. Ross' work, where good hypotheses for increases in activity are offered.
- If MSIs can be leveraged to reduce Pt content, that is worthwhile. Acidic phosphates and basic oxides have been selected. Trying to understand the mechanism is a good strategy.
- Generally good although details seem a bit disorganized. Little cost discussion. Reasons for strong interest in gold were not clear.
- Approach is sound, but it would be rounded out if more fuel cell tests were carried out.

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

This project was rated **2.4** based on accomplishments.

- Progress was made in achieving improved mass activity, not clear how this can translate into improved materials for application in MEAs.
- Progress has been slow but real, with modest gains of specific activity reproducibly demonstrated at useful potentials.
- The very large metal support interaction gain demonstrated for gold in acid is of tremendous academic interest, but the enhanced activity is still at too low of a potential to be of any practical utility.
- Good quantity of results considering the level of funding. The results are interesting, but nothing really useful yet, since no practical new catalysts have been discovered nor has a clear improvement in mechanistic understanding been provided. Nevertheless, this is an interesting approach and now that many initial "hiccups" have been overcome, there is reason to be optimistic that future results will be even better.
- Technical progress has been slow perhaps as a result of non-reproducibility. This has been (hopefully) resolved.
- Mechanisms have been proposed. No data is presented (proposed future work).
- Overall not very impressive results. Hard to sort out "old" work from current work. Not clear that much has been accomplished in current work.
- The results this year were weak. The data on the Pt-TaPO would lead one to believe that it is a poor electrical conductor. This coupled with the low proton conductivity would indicate TaPO should be set aside for a second generation system.

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

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

- Collaboration with GM beneficial, started interaction with Ballard.
- Samples are slowly being made available to industry for testing, and meaningful dialog with at least some industrial scientists has been achieved.
- Interactions with GM have been useful and adding another "end-user" (Ballard) is good. However, some interaction with catalyst companies and/or MEA companies vs. end users would be beneficial for a lab program working on electrocatalysts.
- Good collaboration with GM and National Labs.
- Little apparent interaction with collaboration. ETEK role not really explained. University of Hawaii and Ballard roles not known.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Plan is adequate as long as it is systematically executed.
- Stated plans did not seem to propose much in the way of new types of materials to be explored.
- Plans need to emphasize the continued search for stable, high mass activities.
- It is not clear how the low surface area issue with the TaPO will be resolved. The one pathway that was tried was not effective and it is not really clear how the barriers to this approach, to date, will be overcome. Once a good performing catalyst has been made, will stability (e.g., potential cycles) be established? How will durability of these new materials be addressed?
- See recommendations below about communicating the objectives on the work with Au.
- Mechanistic studies are proposed. What tests will be conducted to confirm/refute the mechanisms?
- Generally reasonable but not very well defined. No details on possible future technology transfer.

Strengths and weaknesses

Strengths
- New interesting ideas.
- Growing patience to keep at the experiments until they are reproducible and right.
- Unique approach which has promise and is not currently being pursued by others. Focus on materials that are thermodynamically stable in the environment of interest is great. Good results to date, especially for the amount of funding provided.
- Using the support to increase activity is an established approach in heterogeneous catalysis.
- Some what unique approach. Could add to knowledge base if done well.

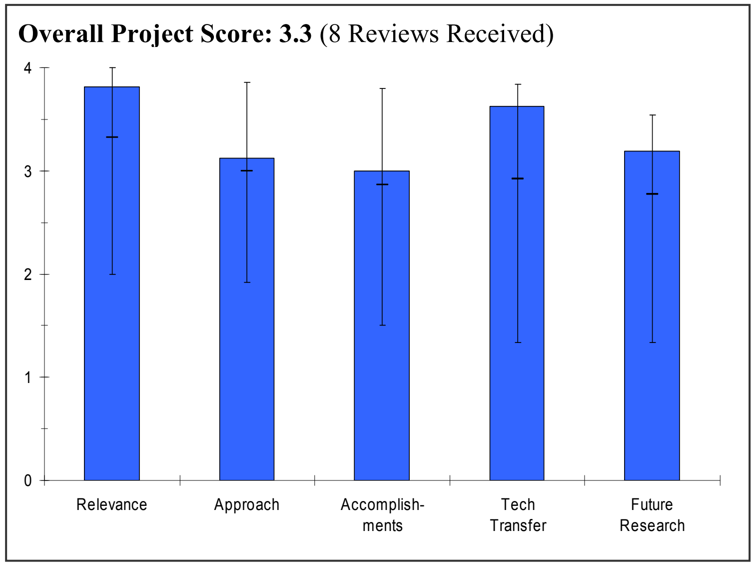
Weaknesses
- Execution and identifying what are the key process/structure parameters that matter for the performance.
- Mechanistic discussions still seem to be largely speculation rather than driven by a convincing body of data.
- Not understanding of activity effects, to date. For example, why does Ta:P ratio of 1:1 have the best activity? Is that even being investigated? Is there a hypothesis?
- Not clear how MSI has been or will be confirmed.
- Doesn't seem well organized. Little of the work or proposed work is shown to support performance increase. No discussion on costs issues associated with TaPO.
- This project needs a new focus. The continued focus on TaPO does not appear to be going anywhere. While the phosphates are an intriguing idea, there must be some other system(s) that is more promising that a Ta based one.

Specific recommendations and additions or deletions to the work scope

- Be careful not to let gold become too much of a time sink as long as all of its activity is below 600 mV (present activity should perhaps not be described as "works like a champ").
- It is not at all clear why Au catalysts are being studied here. I thought the reason was that it was a means to determine if there were support-catalyst interactions, i.e., it is a good "model system" since it has minimal activity in acid electrolytes. However, the summary statements on slide 19 are made about the abundance and stability of Au (relative to Pt), which implies that it is being investigated as a real commercial system. This should be made clear, is Au just a "model" or a real option? Presumably, it is the former. If, in the future, very large activity increases with Au are obtained, then it is OK to switch to stating that it is a real possibility.
- Why is "P" not circled on the table shown in slide 17 Isn't this one of the leading materials in this project?
- Try to organize better. Better explain emphasis on gold. Better relate results and planned work to cost, durability, and performance objectives.
- Redirect the entire project to a new materials system.

Project # FC-16: Development of High-Performance, Low-Pt Cathodes Containing New Catalysts and Layer Structures
Paolina Atanassova; Superior MicroPowders

Brief Summary of Project



This is a four year project led by Cabot Superior MicroPowders to develop and apply a combinatorial powder synthesis platform based on spray pyrolysis for discovery of high performance, low-Pt cathode electrocatalysts for PEM automotive fuel cells. This project will use the platform for electrocatalyst composition discovery and microstructure optimization under conditions that can be scaled for commercial powder production, and will deliver high-performance cathode electrocatalysts and membrane electrode assemblies (MEAs) with lower Pt content to meet the DOE target of 0.6 gPt/kW. Specific objectives include completing the development of rapid testing equipment (DuPont Fuel Cells); completing high throughput synthesis of ternary alloy compositions in a discovery mode; scaling up the best performing compositions, further optimizing MEA electrode structure; testing long-term stability of new electrocatalysts; and delivering electrocatalysts and test MEAs to stack manufacturers.

Question 1: Relevance to overall DOE objectives

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

- Reducing the Pt loading without performance loss is in line with DOE objectives.
- Extremely relevant to reducing the cost of PEMFCs.
- Low Pt loading is key to overcome price barriers.
- The project is focused on reducing PGM content of cathode without loss of performance or durability. Cost reduction and durability improvement are both critical to achieving DOE targets.
- Very relevant to cost, performance, durability issues.

Question 2: Approach to performing the research and development

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

- The combinatorial method being used has a major advantage over others in that the materials being screened are produced by the same process that would be used in large scale production of "real" catalysts.
- Considerable experience and capabilities in synthesis of new materials. Oriented towards scale-up and high production right from the start. Selection of compositions is logical but somewhat uninspired. A very large parameter set, but early narrowing of the selections may overlook unexpectedly beneficial compositions.
- High-throughput synthesis and testing is a very beneficial approach for rapid progress.
- Spray pyrolysis allows a variety of catalyst compositions, a rapid synthesis method and is easily scalable.
- The advantages of this unique preparation method over conventional catalyst preparation routes need to be pointed out.
- A lot of the work has been focused on the composition of the alloys. The effects of process parameters do not seem to be sufficiently addressed. How much room for improvement does the optimization of the process parameters offer?

- Structural effects are not discussed, e.g.. to what extent and with what effect does metal segregation occur during the heat treatment step?
- Rapid catalyst screening method for small scale fuel cells is a very valuable effort.
- Combinatorial screening has allowed rapid evaluation of many catalyst alloy combinations and identification of several promising systems. Quantitative performance-based decision points were used.
- Approach allows mass screening of many candidates. Durability not really discussed. Scale up issues could be important.
- Developing another method for producing catalysts (spray pyrolysis) seems a good strategy. Long-term testing will show if the combinatorial route for catalyst development is good.

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

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

- 21 ternary combinations were screened with some sweet spots found. These were selected for further testing and it seems that at least one produced a halving of Pt loading without any performance loss. The composition was not revealed. However, reports from elsewhere, e.g. GM, have already clearly established (in stacks) a halving of Pt loading using Pt-Co alloy catalyst. Unfortunately, the ternary compositions did not include any of the valve metals, Ti, Zr, Nd, Hf, W or Re, which would have made the results more interesting.
- Showing ongoing performance improvements. Achieving stable 2-3 nm particle sizes is a positive contribution, but very little durability data was provided on these compositions. Apparently other compositions (slide 14?) are sustaining kinetic activity, but may not be the same materials.
- Excellent progress in reducing PGM loading per kW.
- Huge amount of samples have been investigated.
- Performance of spray pyrolysis does not appear to be superior to current state of the art. If this is merely an alternative to conventional preparation routes, it is not necessarily an improvement.
- Durability improvement appears to be related to better availability on the carbon supports and does not seem to be related to the method of synthesis used in this project.
- Performance and durability of "discovered" alloys match those of conventional platinum at half the loading.
- Performance improvements are very good but more durability work is needed. Scale up results are very limited.
- The activity of the catalysts has been enhanced, and the initial stability test looks promising..

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

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

- It appears the Cabot and DuPont collaboration worked extremely well.
- Collaborations are quite strong.
- Excellent collaboration with DuPont.
- A good collaboration with both vertical integration and diversity. This is a good thing to see.
- Good collaboration with industry, component manufacturer and OEM involved.
- Hydrogenics (stack testing) seems to have been rather passive. Stack tests could be used for rapid screening. (different types of catalyst tested in one stack) to broaden the statistical basis.
- Collaborations include a university for TEM analysis, a stack/system fabricator, and an automotive OEM.
- Potentially very good. Partners could be more involved.
- Outstanding.

Question 5: Approach to and relevance of proposed future research

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

- Until they reveal the composition, it is not clear they have actually found anything new here. Factor of two gains from alloys are essentially already "on the books". Perhaps there will be a gain in process cost and hence the catalyst made by the spray pyrolysis method will be cheaper than from other methods.

- Last year of project. Corrosion resistant carbon results, shown on the future research slide, are very promising.
- Project under current contract is ending very soon. The proposed future work for the remaining few months is adequate to fulfill the goals of the project.
- Given the remaining duration, plan seems ok.
- It would be interesting to compare the cost of spray pyrolysis to the wet chemical route.
- Remaining effort provides a logical conclusion to the project.
- Generally good. More emphasis on scale up and cost issues for best candidates would help.
- Long-term stability tests critical and should be completed soon to assess the usefulness of the synthetic approach.

Strengths and weaknesses

Strengths
- Expertise in spray pyrolysis process to mass-produce high surface area materials.
- Project managed to screen a very large array of potential compositions with a high confidence level. Demonstrated catalyst production at larger scale rates.
- The strengths of this project and approach are the combinatorial approach and the scalability of the catalyst fabrication process.
- The high throughput synthesis process is a good development. It is good to see a common catalyst test protocol.
- Rapid, variable and scalable synthesis method.
- Rapid pre-screening.
- Excellent partnership.
- Ability to experimentally screen many candidates in a controlled and meaningful manner.

Weaknesses
- Not clear what the fraction of catalyst cost is due to synthesis chemistry and therefore what cost reduction can result from the process if there the catalyst is not truly new.
- While the screening is of value to establish baseline data sets, for the results shown, no particularly interesting or effective compositions were found.
- It is not clear in the presentation how this project contributed to fuel cell stack cost reduction and durability goals. Need further information if they used a high constant flow to generate a polarization curve.
- Performance over-emphasized, sometimes at the expense of durability.
- Too much focus on composition, effect of structure neglected.
- Focus on GDE only. This may limit the catalyst utilization. PM utilization may be better in catalyst coated membranes.
- So far too little emphasis on pursing durability issues.

Specific recommendations and additions or deletions to the work scope

- The source of the improved stability of the Pt alloy catalyst specific surface as compared to Pt should be clarified. Is the improved stability of Pt alloy surface area due to lower starting specific area of the alloy catalyst or an intrinsic property of the alloy vs. pure Pt?
- For a project that is coming to an end, a closing presentation maybe more useful to the audience. Tell us what contribution the work made in terms of cost, durability, etc. PI should report how close they came to fulfilling the original goals you promised to the DOE. Tell us what did you discover in your project that we should consider for other projects.
- Need to investigate the effect of structure on catalyst performance.
- How much improvement in performance and durability can be created by varying process parameters? Relate that data to other commercial catalysts with comparable composition/structure.
- Leaching tests ought to be included. How stable are the alloys in presence of liquid water/acid environment?
- Include at least preliminary cost discussions for promising candidates. Include durability as a higher priority issue. Push the short stack testing.

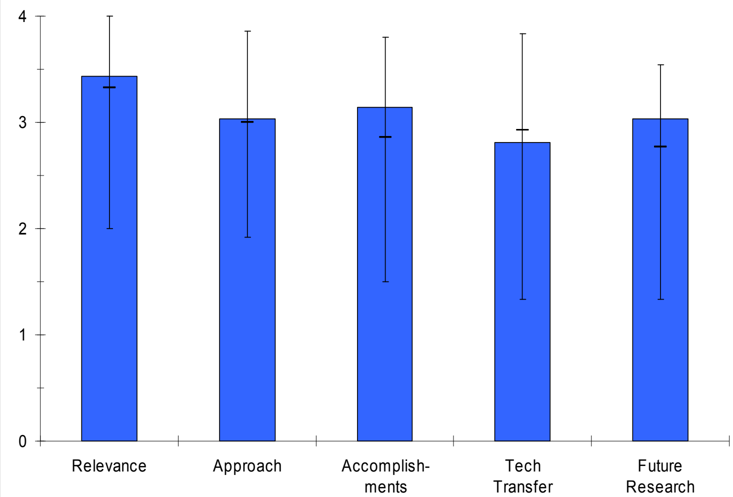
Project # FC-17: Electrode Stability

Xiaoping Wang; ANL

Brief Summary of Project

The objective of this Argonne National Laboratory project is to elucidate rates and mechanisms of loss of electrochemically active surface area (EASA) of polymer electrolyte fuel cell (PEFC) platinum electrodes. We are systematically investigating the dissolution behavior of Pt electrocatalysts in an aqueous, non-adsorbing electrolyte, mimicking the conditions encountered in the PEFC, as a function of potential, potential cycling, alloying, temperature, and other variables that are of interest in the automotive applications of PEFCs.

Overall Project Score: 3.1 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- How do the test conditions relate to real world FC cycles?
- How does cycling affect I-V curves for real fuel cells?
- Although stability of Pt in fuel cells is important, it is also an area where a lot of knowledge already exists. However, automotive applications present operating conditions that are very different (e.g., highly cyclic) relative to what has been studied in the past. In addition, PEM fuel cells have a different type of electrolyte and use different catalysts (e.g., high wt% Pt on C) than what most previous studies have focused on. Pt loss does not appear to be a major problem, but it does warrant some further studies.
- Improved understanding of Pt solubility as function of operating potential and temperature is important for improved durability.
- Project addresses DOE goals on increased durability and electrode performance of PEM fuel cells.
- Too far from real fuel cell world.
- Electrode stability is the key issue for FC successes.
- Reduction of performance degradation is high priority.

Question 2: Approach to performing the research and development

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

- It is not clear that the investigators are fully versed in the work that has been done to date. For example, are they aware that a model of Pt dissolution, including under cyclic conditions, was recently published (Darling and Meyer in JECS)? Are they aware that Pt is a catalyst for C corrosion (see, e.g., L. Roen, C. Paik, and T. Jarvi, "Electrocatalytic corrosion of carbon support in PEMFC cathodes," Electrochemical and Solid-State Letters, Vol. 7, No. 1, pp. A19-A22, 2004) and therefore will be a significant factor in their work at high potentials with Pt/C?
- Systematic approach based on model structures and combined with studies on Pt/C catalysts.
- Unclear how RRDE technique can be used to measure very small current from Pt dissolution.
- Project approach is suitable to the goals.
- The project has to focus either on the fundamentals of Pt stability or to adopt to a high throughput approach to encompass the FC reality of Pt corrosion. More detailed and in depth knowledge and critical literature review could be beneficial.

- Very interesting and somewhat unique approach.
- Not clear that same important variables (e.g. humidity, temperature, cycling rates, etc.) are being included.

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

This project was rated **3.1** based on accomplishments.

- What is the reason for the difference between potentiostatic and potential cycling dissolution rates?
- OK, for just starting out. But, it is not clear that the team has done a thorough literature review.
- Good progress in identification of conditions that lead to platinum dissolution and particle growth.
- The project is new, however a better focus will help.
- Project has impressive results for the time since started.
- Good evidence that cycling through large potential is major factor in degradation.
- Pace seems somewhat slow relative to FY06 milestones. In particular, if the test apparatus for elevated temperatures is just now being constructed, achieving the 09/06 milestone of temperature effect on dissolution of Pt and Pt-Co (on carbon support) seems in doubt. Plots of dissolved Pt vs. Potential don't have error bars. At >1.2 V, the PIs conclude that dissolved Pt decrease for polycrystalline Pt, but continues to increase for Pt/C. In the absence of error bars, perhaps the differences are not all that great.

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

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

- No interactions with FC manufacturers.
- Interactions with other institutions were mentioned but not explained.
- Could be improved by collaborating directly with researchers who have already done some studies of electrode stability on cells and/or stacks. For example, is Mahlon Wilson from LANL actively involved in this project or is there just an "interaction" by using LANL data?
- The project has to expand collaborative effort.
- Good collaboration with National Labs.
- It needs real life test conditions (potential range, Nafion, O_2 vs. N_2, etc.) from stack manufacturers.
- Very little so far but possibly not much opportunity.

Question 5: Approach to and relevance of proposed future research

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

- Why higher upper limit? Do commercial fuel cells operate at 1.5 V?
- A major goal of this project should be to generate a standardized set of stability tests that can be used to assess the different alternative catalysts that DOE is funding. Ideally, these tests could be conducted on new catalysts (e.g., Pt alloys) on standard carbon supports, as well as for new supported-catalyst systems. This means that the stability tests should challenge both the catalyst (e.g., accelerate dissolution with potential cycles) as well as the oxidation of the supports (e.g., potential holds at high potentials). This would be a very welcome addition to guide and assess all of DOE's advanced catalysts projects.
- Important addition in the future work plan is the study on alloy catalysts. It may be beneficial to expand on several compositions and catalysts and to be able to elucidate effect of composition and preparation method on the alloy stability and solubility of elements incorporated.
- Project should switch from analytical stage to understanding mechanisms of Pt dissolution and agglomeration. Since Pt agglomeration rate may depend on support, it would be better to use carbon support for Pt and Pt_3Co from the same manufacturer.
- The plan does not seem to include looking for other important factors such as rate of cycling and humidity.
- Planned future work seems to give only lip service to modeling effort to extrapolate results to real systems (FY05 reviewer comment). Does ANL really plan to do this? Need to address how to make the work more representative of real systems.

Strengths and weaknesses

Strengths
- Good electrochemistry.
- Bringing access to good analytical tools to bear on a relatively important issue.
- Systematic investigation of the influence of different factors on rate of Pt dissolution.
- This work is needed.
- Impressive implementation of difficult and sophisticated laboratory work.
- Excellent team of investigators.

Weaknesses
- Too far from realistic conditions.
- It is not clear how much and what coordination with other National Labs is needed to prevent duplication.
- Is this project adequately funded for the work plan? $350 K seems like a small effort, amounting to a little over 1 person-year with not much left over for materials. Should try to leverage additional DOE/BES funding.

Specific recommendations and additions or deletions to the work scope

- Coordinate with FC manufacturers and catalyst developers to focus research better.
- Include studies with different wt% of Pt on C (e.g., 50 wt %), since this is the type of catalyst being used in PEM cells. Additionally, characterize the changes in the carbon support that occur, as well as measuring the loss of Pt. There will be changes in C surface area, oxidation state, and quantity (i.e., loss to CO_2).
- Include studying stability of cathode catalysts while cycling the anode between H_2 and O_2. This is the real start/stop condition. If done, the results will be very interesting (e.g., see, C. Reiser, L. Bregoli, T. Patterson, J. Yi, J. Yang, M. Perry, and T. Jarvi, "A reverse-current decay mechanism for fuel cells," Electrochemical and Solid-State Letters, Vol. 8, No. 6, pp. A273-A276, 2005).
- It would be interesting to complement platinum dissolution rate and HRTEM measurements with cyclic voltammetry to establish relationship between microscopic and macroscopic results for EASA.
- Similar work may be going elsewhere in other labs. Stability is a serious issue and establishing one large project is worth considering. Include industry in the coordination effort, if not in a leadership position.
- Consider potential gradients (rate of cycling) and humidity as possible significant variables.

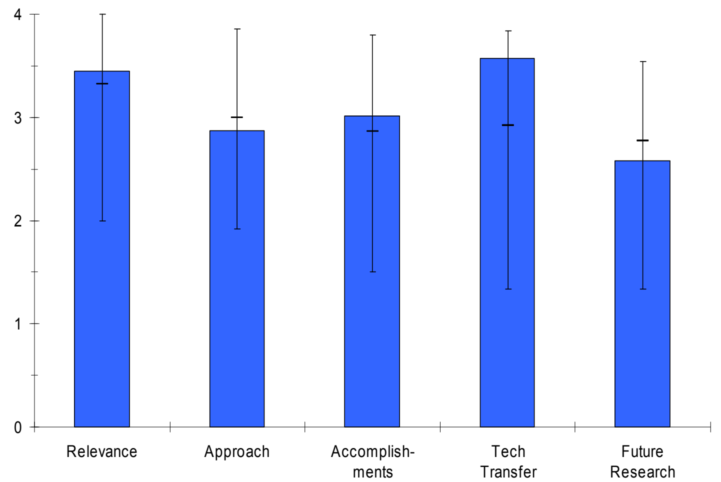
Project # FC-18: Integrated Manufacturing for Advanced MEAs

Yu-Min Tsou; E-TEK

Brief Summary of Project

De Nora North America and its team are developing new ELAT structures and cathode alloys that allow an overall cell performance of greater or equal to 0.4 A/cm^2 at 0.8 V or 0.1 A/cm^2 at 0.85 V operating on hydrogen/air with precious metal loadings of 0.3mg/cm^2 that are amenable to mass manufacturing technology. Advances from this work as well as from development of a membrane which operates at 120ºC and 25% relative humidity will be integrated into pilot manufacturing, aimed at delivering stack scale components and testing these components at the stack scale.

Overall Project Score: 3.1 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- "Integrated manufacturing" implies a cost reduction but cost isn't discussed.
- Most of the project is on track to provide useful contributions. The high-temperature membrane's usefulness is less clear.
- As the technology matures, low-cost high volume manufacturability will become more and more important.
- Reduced catalyst loadings are critical to meeting fuel cell cost targets. High-temperature membranes support the R&D plan objectives.
- The project clearly addresses the key technical barriers outlined in the multi-year R&D plan.
- Low cost manufacturing processes will be key to making fuel cells commercial.

Question 2: Approach to performing the research and development

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

- The only "integrated manufacturing I can find in this work is the ion beam assisted deposition (IBAD) of catalyst onto the gas diffusion layer. The resulting performance of the MEAs is so-so, but there is no cost analysis. Is IBAD really a low-cost manufacturing process? Where is the data? Surface area is relatively low, similar to that in 3M "whiskers."
- Developed new catalysts and gas diffusion layers (GDLs), but little detail on approach. The GDL material is a cloth. Stack manufacturers may have difficulty accepting cloth.
- Three distinct tasks with separate description of the approach for each. High volume low-pt loading is important.
- The approach is well thought out and focused on the key issues. The activities are well integrated among the partners.
- Three important areas have been addressed (catalyst, membrane, MEA manufacturing).
- Approach for GDL improvement remains unclear.
- The IBAD approach seems creative and promising but cost and manufacturing issues must be included at an early stage.
- Down-selection criteria would have brought the membrane R&D work into better focus. Too many activities in parallel.

- Some fundamental hurdles should have been considered in an earlier stage (low loadings).
- Well integrated program.
- Fuel cell evaluation only used single cell and short stacks – full scale testing is desired. But, perhaps this was beyond the scope of the project.

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

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

- Some progress on making MEAs using the IBAD deposition but the results do not appear to reduce cost in any significant way.
- The results with ion beam deposition are surprisingly good for a non-impregnated structure. This may have significant advantages for some systems and for manufacturability, particularly if good cathode performances can be achieved. On the other hand, the results with the high temperature membrane are not promising.
- Have shown good performance with fine gradient GDLs. Have shown progress with IBAD electrodes. Also conducted stack tests (Nuvera).
- The project has made significant technical progress during the last year. The IBAD process has shown good results.
- A robust gas diffusion layer working at a different set of conditions (dry, wet, hot, cold, high load, low load) is crucial. The durability test at constant current density at modest temperature and full humidification is not very meaningful in this regard.
- The quality of the IBAD coating should have been analyzed more thoroughly. Does this process yield continuous layers even at very low Pt-loadings at the microscale? Discontinuities are likely to impair the durability.
- Fuel cell evaluation only used single stack and short stacks – full scale testing is desired.
- The performance of the catalysts and the membrane has been increased. The long-term tests have to be extended. That will provide information on the stability of the multi-component catalyst. Tests with potential cycling needs to be performed.

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

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

- Excellent involvement of collaborators.
- They have universities and commercial organizations as team members.
- Very good collaboration among the university and industry partners.
- A very competent and experienced team with a broad and diverse background.
- Balanced participation of academia and industry.
- Transfer of research results to industry ensured.
- Good collaboration with partners.

Question 5: Approach to and relevance of proposed future research

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

- Future work has to address the cost barrier in a more convincing and quantitative way. The membrane work should be discontinued.
- Future work is solid and builds on previous efforts but not particularly ambitious. The future direction of the high temperature work is not clear as the membranes are not providing the requisite low RH performance.
- Planning to lower Pt loadings but will need to address sealing and gasketing issues.
- Project is scheduled to end in August 2006.
- Planned future work builds on successes to date and validation of improvements in stack tests.
- Integrate as many of the individual components as possible into one MEA technology and see if results indicate an overall improvement.
- It appears adequate. It is difficult to say more without compositions of catalysts, membrane.

Strengths and weaknesses

Strengths
- Potential major suppliers of both components and stacks involved.
- IBAD and cloth GDL work are potentially useful contributions.
- Good collaborations among industrial and academic organizations. Broad experience and expertise base.
- Strong collaborative team, with good balance of university, small business, and industry participation.
- Overall project has been well coordinated and implemented.
- Good partnership with complementary skills.
- Good collaborations.
- Good technical progress.
- Appreciation for differences between hand-made and machine-made components. Reliability is inherently tied to manufacture and scale-up.

Weaknesses
- The value of the high temperature membrane effort is not apparent.
- Durability is still a concern, particularly under cycling conditions.
- The cost of the IBAD process was not discussed.
- Catalyst stability/durability needs to be better established.
- Big project, too many areas.
- Catalyst durability only superficially addressed.
- Component down-selection and integration into a single MEA technology comes if at all at a late point.
- Need full stack testing of proposed improvements. No mention of cost models, production capabilities, production metrics, large scale manufacturing.

Specific recommendations and additions or deletions to the work scope

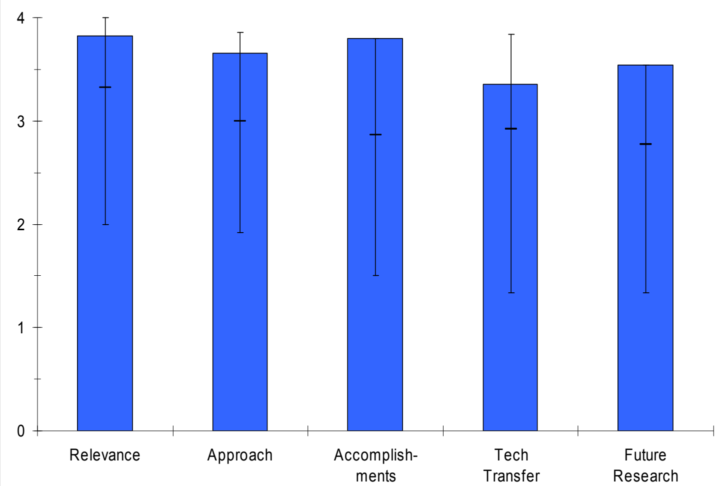
- The project should consider focusing on aspects other than the high-temperature membrane effort.
- Apparently, the project is ending in a few months. If this work continues, suggest durability testing under cycling conditions and investigations of degradations/failure mechanisms.
- The large-scale manufacturing process and the cost of the IBAD process needs to be better established.
- Catalyst stability/durability needs to be better validated.
- For a project that is coming to an end, the presentation should include a discussion of progress in terms of lowering cost and improving durability. PI should communicate how close they came to fulfilling the original goals proposed to the DOE. Tell us what did you discover in your program that we should consider for other programs?
- Focus on the IBAD route, which may also be applicable to ccms.
- Not only take into consideration the reduced noble metal content, but also the required power density (ultimately 1 W/cm^2).

Project # FC-19: Advanced MEAs for Enhanced Operating Conditions, Amenable to High Volume Manufacture

Mark Debe; 3M

Brief Summary of Project

Overall Project Score: 3.7 (7 Reviews Received)



3M is developing high performance, durable, lower cost membrane electrode assemblies (MEAs) qualified to meet demanding system operating conditions of higher temperature and little to no humidification, with less precious metal catalysts and higher durability catalysts and membranes than current state-of-the-art constructions. Objectives are to develop durable, lower cost MEAs for operation in the range of 85 < T < ~ 120ºC (develop ultra-thin layer catalyst electrodes [nanostructured thin film]; optimize perfluorinated sulfonic acid based ionomers modified for enhanced durability at low relative humidity; match MEA components for enhanced performance under demanding conditions; utilize roll-good fabrication processes for lower cost) and to investigate approaches for membrane operation in the range of 120 < T < 150ºC.

Question 1: Relevance to overall DOE objectives

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

- High volume manufacture flexibility is very important.
- Not only is the project relevant to the DOE objectives, this team has clearly focused and targeted on the critical issues and a deliberate path towards meeting those objectives.
- The choice of perfluorinated systems shows their understanding of the long history of such systems and they understand that such chemistries are not, de facto, expensive, as is assumed by others.
- If successful, this project will result in game-changing technology.
- Highly relevant topic.
- Project seeks to solve major problems with catalysts and membranes relevant to DOE transportation applications.
- The project addresses several critical issues: membrane performance and durability, catalyst performance and durability, support durability.
- This project addresses several key issues for the fuel cell technology and as such is very relevant to the program objectives.

Question 2: Approach to performing the research and development

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

- Most of the work is done with roll-goods manufactured materials.
- 3M's approach is methodical and step-wise; more important is their recognition that ultimately, any chemical system explored must be amenable to continuous manufacturing operations.
- Their holistic approach, from polymer choice, through MEA surface innovation (their "tunable" whiskers) for catalyst support, to catalyst exploration (downselecting to ternary systems) is impressive.
- The amount of experimental data generated (including those of long-term testing under simulated load profiles) is laudable.

- The NSTFC approach is very creative and innovative with tremendous potential – excellent.
- The approach outlined in task 2 (HT membrane R&D) is impressive in itself. Care must be taken to link the activities at an appropriate time. Otherwise task 2 may remain a stand-alone development effort.
- Approach has been thorough.
- Project takes integrated approach to the problem.
- The approach has been systematic and focused.
- A few uncertainties that the PI may want to address in the future have to do with NSTFC/3M PEM operation under the condition of high humidity and low air stoichiometric ratios.

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

This project was rated **3.8** based on accomplishments.

- Impressive progress has been cited. Ternary Pt-Co-Mn catalysts show high performance and ability.
- Impressive set of accomplishments (mass activities based on their NSTF catalysts and correlations with the whisker morphologies; downselection to their Pt ternary structures; wealth of the CV cycling data to assess durability; identification of loss mechanisms for catalytic activity; equivalent weight (EW) effects of the 3M ionomer on the polarization curves, etc.)
- The progress shown by the team is solid, including the reproduction of MEA properties on roll goods from systems produced by hand.
- Outstanding results on the catalyst development. All the relevant investigations have been performed. Very meaningful and hence convincing data.
- The advantage of 3M's 730 EW membrane as compared to Nafion® 1000 EW at 120°C, 20% RH is surprisingly small. Only upon reduction of the humidity down to 10% does the difference becomes more pronounced. This deserves closer investigation.
- Sensitivity tests at lower temperature (flooding issues) should be included.
- The technical accomplishments are very high, specifically with respect to high performance at low catalyst loadings, durability, high temperature performance and high temperature durability..
- Progress to date is excellent.
- Short stack testing is good, but full stack testing is really required.
- Performance and durability of the MEA have been significantly improved.
- Impressive cycling stability test results in the last year.
- The source(s) of (i) an increase in the performance during the first ~50 stop/start cycles and (ii) the reversible performance loss should be clarified.

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

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

- Showed a list of collaborators but did not point out these contributions.
- This phase of the project was NOT as collaborative as the earlier phases; hence the low scoring.
- The presentation (unlike those of previous years) appears to be primarily based on 3M internal programs.
- Good partnership.
- The project could benefit even more from the input of the OEMs.
- While this evaluation criteria is difficult to assess from a half hour presentation, it appears that learning from collaborations has been incorporated into research and such collaboration has proved fruitful.
- The only entities missing from the formal list of partners are a stack/system developer and an automotive OEM.
- Very good collaboration with multiple partners; stronger stack developer presence would have been a plus.

Question 5: Approach to and relevance of proposed future research

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

- The project is essentially complete, except for delivery of a short stack to DOE (Argonne) and final report.
- This project may be approaching a "pre-commercial" phase.
- This project has graduated from the R&D phase and the future R&D plans appear to be less "inspired". Given the short remaining duration the plans are absolutely adequate.
- This project is 97% complete so the remaining research is limited. The proposed work beyond the time limits of this project is appropriate.
- 3M is continuing work to commercialization.
- In addition to the research already proposed by the PI, future work should also include: (i) testing of the resistance of the low surface-area NSTF catalysts to surface-active the impurities likely to be present in the cathode feed; (ii) performance recovery after NSTFC flooding; (iii) suitability of the approach to other (non-3M) PEMs.

Strengths and weaknesses

Strengths

- This systematic, comprehensive, project has to be considered a successful demonstration of the novel surface structure-property relationships in the MEA design and manufacturing.
- The catalyst selection demonstrates solid achievement.
- This project has moved the product development into a "pre-commercial" stage.
- Highly innovative approach and outstanding technical accomplishments.
- Relevant testing conditions (performance and durability) have been chosen resulting in meaningful data.
- Well focused, excellent technical progress, well positioned to commercialize technology which has been developed in this project.
- Good approach and execution.

Weaknesses

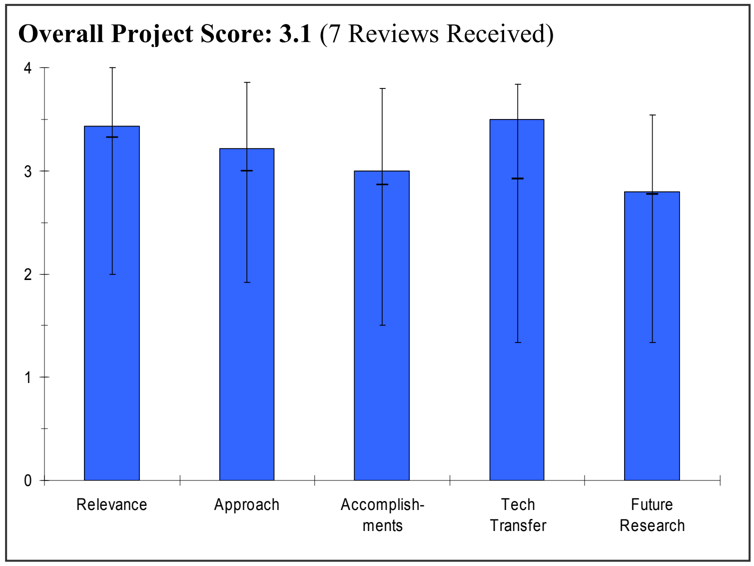
- Not many; but more stack versus cell data to confirm the progress made would have been instructive.
- More collaborative results with major stack developers would lend credence to the product development claims in this presentation.
- Make sure membrane and catalyst development efforts find their way into a combined MEA technology.
- More testing in full scale stacks.

Specific recommendations and additions or deletions to the work scope

- Develop more full-stack data.
- Work with key stack/system developers.
- No information on duration of break-in stages of the NSTFC has been provided.
- This is important especially for automotive OEMs. Concentrate on the scale up of the NSTFC production.

Project # FC-20: Development of High Temperature Membranes and Improved Cathode Catalysts for PEM Fuel Cells

Lesia Protsailo; UTC



Brief Summary of Project

In the area of high temperature operation, United Technologies Corp Fuel Cells is assessing and optimizing fuel cell materials that define and influence performance and durability of PEM fuel cells at operating conditions of 100-120°C, 25-50% relative humidity. Scope of work includes development, evaluation and optimization for high temperature operation of such materials as membranes, catalysts, gas diffusion layers, seals, etc. Effects of temperature, relative humidity and cyclic operating conditions on performance and durability are investigated using both *ex situ* and in-cell tests. Improved high temperature membranes development includes modification of Nafion-like materials with solid acids and fabrication of novel hydrocarbon ion-exchange membranes. For improved cathode catalysts, Pt alloy fabrication procedures are being developed; catalysts are fabricated, and electrodes in membrane electrode assemblies are optimized. Performance and durability of these catalysts are evaluated though the use of *ex situ* techniques (RDE, liquid half-cell tests) and *in situ* fuel cell tests.

Question 1: Relevance to overall DOE objectives

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

- It was difficult to fully evaluate this project. For example, one could not determine the power density or specific power of the stack sent to ANL. It would have been helpful if the presentation included a summary of membranes and cell performance vs. the DOE metrics
- The UTC team has focused on critical performance, endurance, and cost issues. The information generated is very useful to the fuel cell program.
- High relevance. Addresses key barriers of durability, cost, water management and operation at elevated temperature and low RH.
- Highly relevant to transportation fuel cell applications.

Question 2: Approach to performing the research and development

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

- The PIs made an effort to develop a downselect methodology for the polymers. Unfortunately, they built a stack too late in the project and did not realize the benefit of their new catalysts. This is a major flaw in the schedule, and UTC should have built the stack earlier to allow time for trouble shooting.
- The PI implemented a very complete and integrated technical plan which is closely coordinated with team members.
- Increased emphasis on durability studies greatly strengthened the project.
- Project focused on high-temperature operation of the MEA. Well thoughtout approach which looked at a high-temperature MEA, not just a high temperature membrane. Integrated experimental and modeling work.
- Broad approach to possible solutions with subsequent downselect to two membrane approaches. Binary and ternary catalysts pursued for stability.
- Combination of new materials and fundamental understanding of high temperature operations.

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

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

- The biggest technical improvement of the program was the extended durability of the PtIrCo catalysts
- Good effort and progress this year. Other developers should be look at the hydrocarbon membrane results.
- Excellent results in the catalyst area, made significant progress in durability and demonstrating catalyst durability at elevated temperatures.
- Determined the relationship between O_2 permeability and membrane durability; and use of the Fenton's test with hydrocarbon membranes.
- Both BPSH and composite membrane show promise, but project did not bring either approach to fully satisfactory state. Stability of PtIrCo catalyst is promising, but understanding is not complete, limiting contribution to catalyst "design." Demonstration of high mass fraction, thinner electrode useful.
- Highly innovative project. Well coordinated among the several elements.

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

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

- UTC worked well with polymer suppliers. The stack work was not as well coordinated/transferred.
- The PI has shown very good coordination with all team members.
- Good collaborative effort with significant exchange of information.
- Collaborations between Virginia Tech and UConn. on membrane development coordinated well and benefited the project. These subprojects were continued beyond the Go/No-Go when they showed some promise, rather than cut when they did not meet the target, which was a good choice since the continued work led to an increased understanding of high-temperature membranes.
- Broad collaborations. Some dropped (appropriately) based on technology downselect decisions.
- Should have collaborated with MEA company.

Question 5: Approach to and relevance of proposed future research

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

- Clearly UTC made some improvements and more work is needed.
- Project is complete and stack was shipped to ANL.
- Program ending. Good closing presentation.

Strengths and weaknesses

Strengths
- Brought some resolution to the wide number of claimed PEM membranes.
- Good team and long-term progress.
- Catalyst durability at elevated temperatures was demonstrated under this project, countering claims by some that high temperature operation would not be achievable due to limitations of catalyst dissolution.
- Broad collaborations led to useful screening of possibilities.
- Well coordinated effort integrating large number of elements.
- Good closing presentation for end of program. Clearly improved their own knowledge base.

Weaknesses

- Stack development of the new catalysts was rushed, and none of the advanced membranes could be used. The project ended up being 2 separate programs for new catalysts and new membranes, but a more integrated program toward a high temperature stack would have made more of a contribution.
- Need to bring TEM analysis into accelerated testing to look for changes to catalyst population and distribution.
- Membrane development did not meet target and a high temperature membrane was not included in the stack; therefore the stack cannot be tested under high temperature, low relative humidity conditions.
- Carrying two membrane approaches forward may have limited chances to push one or the other further.
- More discussion was needed on how this project made measurable contributions towards the DOE cost and durability targets. It is unclear how much knowledge transfer occurred externally.

Specific recommendations and additions or deletions to the work scope

- The PIs should have addressed the contributions they made to this project. It would have been good to see some lessons learned, feedback on test protocols, etc.

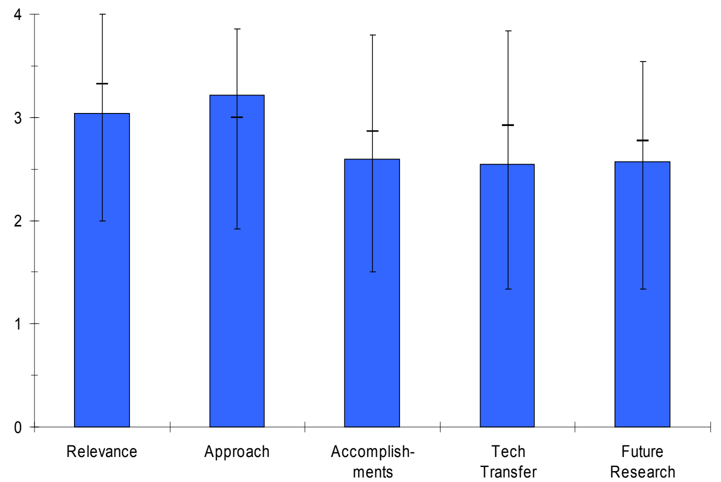
Project # FC-21: Electrocatalyst Supports and Electrode Structures

Mahlon Wilson; LANL

Brief Summary of Project

This project involves the development of alternative supports to carbon that improve the dispersion and stability of the platinum catalyst. Work is focusing on hexaboride supports which are shown to readily provide high Pt dispersions using a simple spontaneous deposition process unique to the hexaborides. Electrochemical characterization of the Pt/hexaboride catalysts demonstrates that activity is maintained even with highly-dispersed, noncrystalline Pt.

Overall Project Score: 2.8 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Need to identify metric for electrocatalyst stability and definition of testing conditions to mimic automotive usage, including start-up/shut down conditions.
- Need to identify the requirement of the supports (expected higher corrosion resistivity than carbon).
- The objectives are clearly consistent with the hydrogen vision, but either separately or together may not be sufficient or enabling. The catalysts have to be compatible and integrate with the other components in the MEA.
- Durability and performance are clearly addressed – provided the novel supports turn out stable.
- Contribution to cost reduction depends on the costs of the alternative support materials.
- Low-Pt durability is directly relevant to transportation goals.
- Minimizing use of platinum group metals (PGMs) (cost reasons) and utilizing them in a manner that will be stable, after long-term fuel-cell use (durability reasons) are valid concerns and worthy project objectives.

Question 2: Approach to performing the research and development

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

- Material selection for Pt stability is good, however, corrosion resistivity (dissolution) of support material should be considered.
- It is very early yet to know if the hexaboride approach will have any utility. It would be valuable to eliminate, as soon as possible, any mechanisms that might cause the release of Ca cations from the high surface area CaB_6 and poison the membrane. If there is, then this approach should be reconsidered.
- The use of interactive supports is an innovative and very promising approach to reducing surface mobility and at the same time increasing Pt crystallite activity. High surface area supports with sufficient stability are a prerequisite, however.
- Analytical model of Pt deposition provides fundamental insight. Exotic materials (europium) and high temperature processes covered.
- The project can be summarized as long term, high risk, and exploratory research in a novel strategy for utilization of PGMs. The researchers appear to be expert and have appropriate analysis tools.
- This is one of those high-risk, high-reward projects that may or may not be successful. But we need this kind of fundamental R&D to identify new materials, especially at the National Labs. Identifying alternatives to carbon supports to improve durability is a key enabler for fuel cells. LaB_6 has a long way to go to reach practicality, but that's OK at this point. Need the preliminary work on many materials to identify the most promising ones.

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

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

- Good to identify morphology of support carbon and electrocatalyst performance.
- Progress has been made in opening up a new area of research for potential supports to replace carbon, but no concrete results yet to indicate that performance, durability or cost barriers can be reduced by this approach.
- Good investigative work on the nature of borides and the deposition of Pt.
- Initial results from key experiments indicating increased Pt activity and reduced surface mobility are missing.
- Very limited fuel cell data. Assertion of Pt monolayer and hexaboride needs to be experimentally demonstrated.
- The PIs are asking good questions and are mindful of the ultimate performance requirements (e.g., Slide 3). It is too soon to tell whether the approach being tested will advance the state- of-the-art.
- The spontaneous deposition with hexaborides is a welcome bonus – provided the materials meet durability targets. Could pave the way to a cheaper manufacturing process.

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

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

- Need to involve catalyst/MEA supplier.
- Might benefit from some collaboration with someone having expertise utilizing such materials in other applications. Not sure where that would be, however.
- Good cooperation betweens the National Labs.
- Input from catalyst, MEA manufacturers, and end users is appreciated.
- Interaction with other National Labs, but limited or no work with industry or academia.
- BNL, ORNL. No commercial or university partners. Good job using National Lab resources.
- Work spans three LANL groups and three other DOE labs. It is premature to collaborate with industry for tech transfer.

Question 5: Approach to and relevance of proposed future research

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

- Need to narrow the objectives. Materials should be down-selected for the next step. Support material corrosion resistivity is still necessary.
- The work appears comprehensive but perhaps should be focused on answering the most critical question first, the durability aspects of the new support materials. Assume the same activity can be obtained with Pt and focus more on just determining if the corrosion resistance of these new supports is any different than carbon – the material being replaced. Also, if there is any difference in the resistance to Pt dissolution from CV cycling up to 1.2 volts at 80°C under H_2/N_2. If the materials cannot perform better than carbon, then there is no reason to try and improve activity.
- Key experiments must be designed and conducted to show whether the alternative boride supports actually do enhance Pt activity and simultaneously reduce surface mobility.
- More focus on stability of supports is needed. CV results were interesting, but as the presenter indicated, these should give one pause. Recommend more emphasis on increasing surface area and less work on fuel cell performance.
- Major work needs to be done on synthesis processes.
- The PIs are on the right track.
- The quantum mechanical modeling is important, so it is welcome that LANL plans to expand it. Need to carefully consider if the BNL catalyst material merits further pursuit. The fact that the hexaboride support may be sacrificial to meet durability may not be a detriment. The question is whether it is better or worse than carbon, which also undergoes the sacrificial corrosion over time.

Strengths and weaknesses

Strengths
- Material engineering based on molecular level modeling.
- Highly innovative approach with great potential.
- Novel approach to develop alternatives to carbon for support materials, interesting properties.
- Elegant modeling and analysis techniques.
- Interesting hypothesis and initial results (e.g., "self healing"); research on track.
- Good long-term, fundamental R&D to develop new classes for materials for catalysts.

Weaknesses
- Systematic development.
- Very narrow scope on boride supports.
- It must be shown in an early stage that the presented supports hold some of the conjectured advantages. To date this does not become apparent, not even from theoretical calculations.
- Subsequently, many other aspects must be considered for fuel cell operation, e.g. H_2O_2 formation, ionomer interaction.
- Greater interaction with other groups outside of LANL/ORNL/BNL.
- Uses exotic material and processes. This could take us away from low cost. No fuel cell data (1 polarization curve). Very basic science.
- Since cost saving is one of the main motivations of project, it would be helpful to also give a back-of-envelope estimate of what the ultimate cost might be. This is particularly important because competing, novel methods (e.g., 3M's in FC-19) have similar goals (that is, Pt on C is not the most relevant baseline).

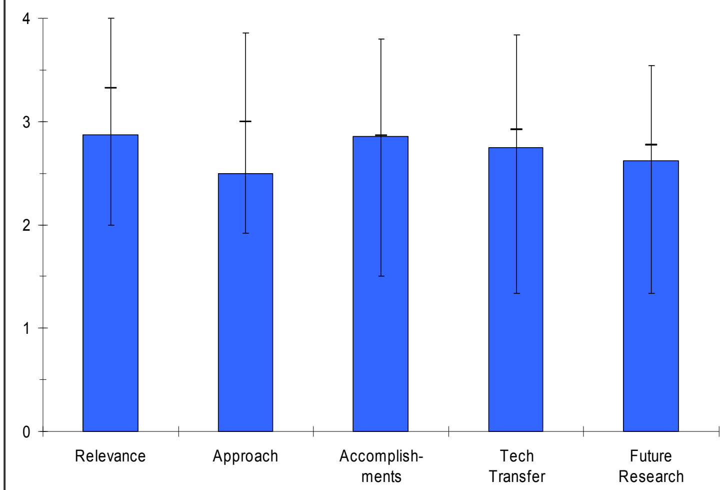
Specific recommendations and additions or deletions to the work scope

- Involve industry partners (catalyst/MEA suppliers).
- Focus first on durability of the new supports. Are they any different than carbon – better or worse?
- The interaction of Pt with the support deserves closer investigation. What exactly is the effect of the electron donating support?
- Key experiment to show reduced surface mobility and increased Pt activity (and corresponding Go/No-Go criteria) must be included. Based on the outcome of these experiments other supports than Borides should be considered.
- Important to keep the Fuel Cell Tech Team apprised of progress through yearly updates.

Project # FC-22: Fundamental Science for Performance, Cost and Durability

Bryan Pivovar; LANL

Overall Project Score: 2.7 (8 Reviews Received)



Brief Summary of Project

This Los Alamos National Laboratory project develops the fundamental understanding and technical underpinnings of technologies for improved cost, performance, and durability of fuel cell components. This effort focuses on phenomenological modeling of the membrane-electrode interface, development of hydrogen oxidation reaction models, and investigation of the platinum-ionomer interface to decrease cost, and improve performance and durability.

Question 1: Relevance to overall DOE objectives

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

- Weak relevance. CO poisoning is no longer relevant.
- This work seems to be in three related areas that might be better pursued separately.
- Addressing the cause and solution of MEA delamination would be of greater value if compared with models correlating delamination with performance. Improved understanding of poisoning is important for determining mitigation strategies.
- Title rates a "4", but will need to see that their approaches apply to performance, cost, and/or durability over the next year.
- Hydrogen oxidation is not one of the major fuel cell problems with pure hydrogen as fuel. On the other hand, anode poisoning e.g., by CO, H_2S, etc., and its mitigation should be pursued as a separate topic. In this consideration, HOR investigation should be limited under this project.
- Relevant but not the same impact as many of the other projects.
- All three areas covered by the PI are critical.
- Strong project team.
- Modeling work important.
- Membrane electrode delamination does need to be studied.

Question 2: Approach to performing the research and development

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

- Basket of 3 tasks which are not interrelated. No project synergy.
- Using old fuel cell I-V curves as sources of inquiry may introduce unknown variables that cannot be easily tracked. The old fuel cells may have had other design or assembly problems that could have resulted in the odd curves. It might be better to use more modern fuel cell materials and designs, and careful break-in and leakage testing, along with careful test equipment calibration rather than using old data with uncharacteristic curve shapes as the source of inquiry. If modern materials and careful testing cannot duplicate the old, odd curve shapes, perhaps they should be discarded.
- A multi-faceted approach with some possible payoffs.
- Not clear if the delamination model is accurate or relevant.
- HOR model can be useful if applicable to lower (5-20 mV?) overpotentials due to reduced loading and/or impurities. A better understanding of the electrodes is always welcome.

- Concentrate on the cathode. Will be interested in next year's results.
- Pursue more experimental delamination studies (accelerated) to validate model-based data.
- Design and pursue modeling/experimental studies mimicking real-world catalyst-ionomer interface.
- Identify leading contributors to performance degradation and determine their relative contribution.
- Logical approach.
- Approach is very academic.
- Question whether delamination results truly represent physical delamination.

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

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

- Delamination model is poorly correlated with experimental data. CO poisoning evaluation is not relevant to H_2 cells.
- It is difficult to draw correlations between the phenomena discussed and the contaminants of interest. The modeling approach seems to have merit, but it might be helpful to have more recent parametric studies to compare to predicted results.
- Appears to be good results on the anode modeling.
- Approach to remediation of degradation or durability problems should be developed based on understanding of these problems.
- The results of the microelectrode work are particularly important accomplishments.
- Good quality data generated.

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

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

- No commercial collaboration.
- Good collaboration.
- Collaboration with fuel cell and/or membrane companies would be beneficial.
- LANL has good interaction with other labs and programs.
- Perhaps project would benefit from increased interactions with OEMs to define future work?
- Collaboration with other laboratories and universities is quite extensive.
- Industry collaboration lacking; this was partially responsible for poor relevance of some portion of the work.
- The PI could benefit form closer collaborations with industrial partners.
- Good coordination and assignments of tasks.
- Good list of publications.

Question 5: Approach to and relevance of proposed future research

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

- Limited effort projected for data correlation – this is probably the most important aspect.
- Although delamination was shown by modeling to affect performance, no work was done to understand the causes of the delamination.
- It might be helpful to try to model the cause of the delamination and validate that.
- The stated fact that methanol accelerates delamination might be a clue to the causes of delamination. It might be important to know the causes.
- Will be interesting to see what comes of the Pt-ionomer interface work.
- The future proposed approach is quite logical and fairly coordinated.
- Contingency consideration is a weak point.
- Very important to continue to correlate delimination model with experimental data
- Question whether proposed future work may be accomplished in time remaining on project

Strengths and weaknesses

Strengths
- The presenter appears to be knowledgeable and up to date on fuel cell chemistry.
- The research team and their physical facilities are excellent.
- Technical approaches employed are very appropriate.
- The PI has identified critical areas where research is needed and designed excellent test methods to address these.
- Expanding microelectrode work to peroxide generation is very promising approach.
- One of the best presentations this week.
- Overall well thought-out and managed project.
- Strong team.
- Important technical issues addressed.

Weaknesses
- The simultaneous pursuit of disparate phenomena in one project might tend to weaken the work.
- This reviewer expected more work done for the level of funding afforded.
- Little or no interaction with industry or academia. Although "applied" is in the title, it seems that the projects are not sharply focused on getting information that is relevant to a broad spectrum of investigators. The model for delamination gives expected results and doesn't seem to advance technical understanding significantly.
- The PI could work more with industrial and university partners to use these techniques to address real problems such as studying delamination under different durability protocols or with new experimental membranes, or applying microelectrode work to the development of new electrode formulations with increased durability under hotter, drier operating conditions.
- More experimental vs. model data needed.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- Perhaps this project might be stopped, with two or three new well-defined projects to take its place. It might also be useful to coordinate it with other work examining how contaminants affect cell performance. This project seems to be using older data.
- Recommend no additional work on modeling MEA delamination. A better use of resources would be to identify the causes and solutions of delamination rather than correlating degree of delamination with performance.
- Degradation of catalytic activity, particularly at the cathode (sintering/surface area loss/flooding) should be included in the future experimental and modeling work.
- Project goals are in three areas. May need to focus more to meet deadlines.
- Include timetable, milestones, and Go/No-Go concepts into future planning thought process.

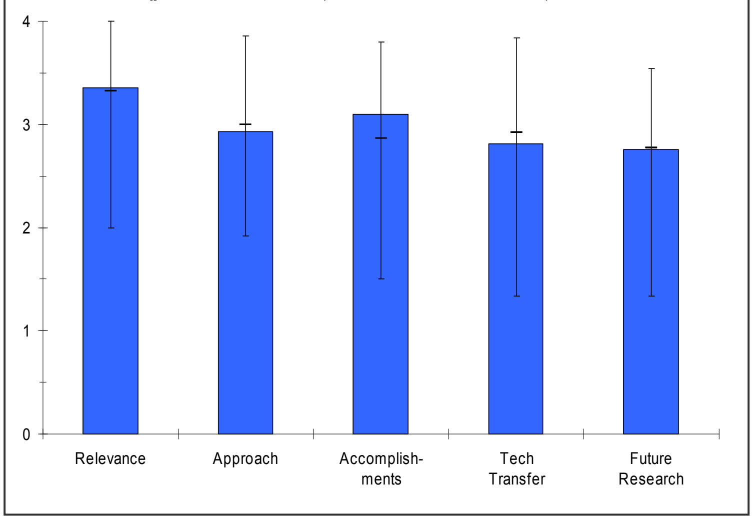
Project # FC-23: Fuel Cell Systems Analysis

Rajesh Ahluwalia; ANL

Brief Summary of Project

For this project, Argonne National Laboratory has developed a fuel cell system model and is using it to assess design-point, part-load and dynamic performance of automotive fuel cell systems. This effort is aimed at supporting DOE in setting R&D goals and research directions and establishing metrics for gauging progress of R&D activities. Objectives are to develop, document, and make available a versatile system design and analysis tool, and to apply the model to issues of current interest.

Overall Project Score: 3.0 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Do not understand how N_2 purge is a significant technical barrier in need of new models. The need to purge N_2 due to crossover seems to be well understood and is not a large contributor to overall system efficiency.
- The system analysis of a fuel cell is an important and essential part of realizing the objectives for the President's Hydrogen Fuel Initiative. The project focus is narrow and appears to consider only one type of fuel cell system. The system analysis evaluates components that have not been fully developed and demonstrated and, as a result, the project's contribution to real world solutions is suspect and could be misleading.
- This project applies modeling to a variety of topics in support of the program. It would appear to be a modeling center of excellence for the program. If this is, in fact, the modeling center of excellence for the DOE Fuel Cell Program, additional resources would be helpful for more model validation, field data comparisons, and component data acquisition. Modeling is difficult in the best circumstances, and a high level of resources is necessary to both accomplish good modeling and to update the modeling techniques used in response to technology movement.
- Very practical effort. Doing good work on sub freezing startup, impurities in feed streams. Well organized research effort. Why is GC Tool being licensed? Making the program free will improve its adoption and use.
- The investigator is clearly responsive to the analysis needs of the hydrogen R&D plan.
- Vital work. Meets the need to have a validated model for fuel cells.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. Applied correctly, this initiative could be extremely valuable to DOE and FCV integrators.

Question 2: Approach to performing the research and development

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

- System level freeze modeling to determine ability to start-up a fuel cell stack seems to be insufficient compared with experimental stack testing. As other presentations in this review have shown, stack level changes (e.g., GDL and MEA) have shown to markedly improve ability of a stack to start-up from freezing temperatures. These types of effects are not captured in a system level model.
- System level modeling to understand component effects on overall system efficiency could help DOE understand research trade-offs. It is not clear how the other modeling efforts (e.g., freeze, N_2 purge) will significantly facilitate DOE's understanding of research needs.

- The approach of the project is strongly tied to the use of a compressor/expander developed by Honeywell, an enthalpy wheel from Honeywell/Emprise, a membrane humidifier from Honeywell/permapure, and radiator results compared to Honeywell data. The approach appears to be to work with a Honeywell PEM fuel cell concept. On the other hand, Honeywell is a component manufacturer and not a fuel cell integrator, such as Ballard, Plug Power, GM, or UTC Power. It appears the model is a collection of Honeywell components and the project has not worked with fuel cell integrators to ascertain the quality of the system being analyzed.
- Although model validation was not discussed in the presentation, model validation is essential for good modeling. If continuous validation is in fact accomplished, this should be highlighted. If continuous validation is not done, it should be done. Feedback from the FreedomCAR Program should be used to help to validate the modeling results. Additional data from demonstration programs, including the FreedomCAR Program and others, would be helpful to validate the modeling techniques and computer programs used.
- Comparison of model and experimental data to be applauded. Outstanding body of work to understand and overcome the design limitations for fuel cell powered systems.
- Efforts should be taken to validate start-up approaches at the system level. Otherwise the approach is solid and is addressing important technical barriers. Plans for and progress in validation at the stack level appears to be appropriate at this time.
- This project is a rational approach to addressing the President's hydrogen fuel initiative. Information and modeling on boot strap starts, N_2 dilution of the fuel and system sizing are valid and applicable.

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

This project was rated **3.1** based on accomplishments.

- The effect of model results on the overall DOE program was not articulated.
- The project has contributed to the advancement of fuel cell technology. The project did not report the results of other researchers who have done start-up tests at freeze conditions. These researchers demonstrate unassisted rapid start. How does this project rationalize that data with their data? How does the compressor/expander operate during transient conditions? What is the effect on efficiency of cyclic load? These points should be addressed since they are an efficiency limitation of the pressurized system. The project did not identify that the enthalpy wheel is a single point device designed for 60% relative humidity at peak power. What happens at non-peak conditions?
- The results on hydrogen crossover and hydrogen effects would benefit from additional validation from actual stack performance data. Some validation of the predicted optimum purge would also be helpful to both validate the model for this aspect of the work and to see if other aspects of cell stack design should be evaluated.
- Making good progress. This effort will impact future system designs.
- The investigators have made solid incremental progress toward refining system and stack level models.
- Now that self-starting at ≤ -20°C and the role of water has been elucidated, can the analysis explicitly offer solutions to the startup issues? E.g., if the stack is heated to 0°C first, what are the implications and what is the energy impact--do we have a battery?
- The technical accomplishments and progress on this project are very good. The information on bootstrap starts from subfreezing temperatures is interesting. Consideration and recommendations should be given on how a fuel cell system would be applied these conclusions. The effect of diluents entering the fuel processing system is very important from a performance and safety perspective. The system and sizing activities are warranted to benchmark where the industry is heading versus the DOE goals.

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

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

- A greater degree of collaboration with fuel cell companies could illuminate potential model deficiencies (particularly with respect to the efficacy of a system freeze model).
- The project is putting its information into the open literature which is good. The project should work with fuel cell integrators to establish if the concept being proposed is valid. Would fuel cell manufacturers make a fuel cell like the concept proposed here? Are all the components and subsystems consistent with the real fuel cell

world? An affirmative answer would establish the credibility of this work, otherwise the project is not linked or helping the advancement of PEM fuel cells.

- Close coordination with field data is important to good modeling until the methods are validated for a long period of time with stable system design types. Additional resources would be helpful to allow additional coordination with field data provided by the program to allow fine-tuning the modeling approach. These modeling results should be coordinated and compared to LANL work on freezing cells to both validate the modeling and to complement the freezing cell work done by LANL.
- Good teaming with Honeywell, FreedomCAR and TIAX.
- The investigators have worked with a variety of other DOE programs as well as other developers to incorporate current practice into their system models.
- ANL should continue to support TIAX in their fuel cell cost assessment. Likewise the guidance from the fuel cell Tech Team must continue. A lot of effort spent on N_2 as an impurity, but it is not clear how the results have been fed back to the H_2 fuel quality specification work done by SAE and ISO. Is there a disconnect here? Do these ANL results support the maximum N_2 concentrations proposed by SAE and ISO?
- The collaborative efforts are good, but information from public and DOE sources do not appear to be leveraged well. For example, HNEI, JARI and LANL are doing impurities testing of electrodes. As part of the testing, they are periodically measuring impurities in the cell exhaust. If requested, this data could be supplied to supplement the model on nitrogen diffusion. Additional effort needs to be applied in modeling the effects of recycle and possibly cascading. The radiator studies done as part of the system work could leverage other manufacturers' public literature (e.g., Modine, Harrison).

Question 5: Approach to and relevance of proposed future research

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

- Not clear how system level modeling of impurity effects will be illuminating. It seems that experimental testing of stack sensitivity to impurities (e.g., CO, Sulfur, etc.) is required for this understanding.
- Not clear how further system freeze modeling will be beneficial, as stack level effects (e.g., GDL, MEA design) seem to be such a strong contributor to stack freeze start-up performance… (see FCP-21_Patterson and FC-30_Mukundan). Experimental testing and/or MEA/GDL analysis is required to quantify these effects.
- The project proposes to continue its strong dependence on Honeywell components. Honeywell is a component manufacturer and not a systems integrator or manufacturer of fuel cells. This approach greatly limits the scope and value of this program.
- The predicted cold startup schemes analyzed could be compared with actual stack and system design to see if the predicted cold startup methods are consistent with current system designs that can accomplish cold startup. This comparison would show if industry is using these techniques and if the techniques are consistent with the model prediction. The work at LANL on cell freezing should be considered as validation input.
- Most of the proposed work is the logical extension of the ongoing effort.
- The proposed future work included several potentially valuable initiatives; care needs to be taken to coordinate with the Fuel Cell Tech Team and industrial partners to ensure that focus is maintained on critical aspects.
- The proposed future work is relevant and appropriate. Leveraging other information sources and collaborators would enhance the effort.

Strengths and weaknesses

Strengths

- Keeping modeling expertise in one area can lead to increased speed of project development and improved results.
- Good that the modeling results are being tied to experimental data.
- The project is set up to be very responsive to the needs of the Fuel Cell Tech Team and industry. Good efforts are under way for validation of the results.
- Excellent team of investigators.
- The strengths appear to be the breadth and scope of the work. The work is applied science and meshes well with commercial efforts.

Weaknesses

- The purge recycle data has been known for several years and it is not clear that this was a contribution to the advancement of PEM fuel cells. The crossover of nitrogen is well known. This did not appear to advance the technology. Most fuel cell manufacturers are moving toward atmospheric operation which appears contrary to the direction of the model in this project.
- Modeling requires high levels of staffing and interaction with real systems to provide optimum results. Additional funding could improve this project if additional staff and resources are added both to the modeling effort and to the validation effort.
- Nitrogen contamination in feed streams leads to reduced cell performance. This is a simple dilution effect which is not surprising.
- Careful management must be maintained to ensure the same responsiveness in the future, the direction of future work can take many possible directions and the same attention to critical modeling efforts needs to be maintained. It does not appear that there are plans in place to validate start-up strategies at the system level.
- The weakness appears to be related to the leveraging of data sources and applying real world experience. Real world experience would question the use of 40 FPI heat exchangers. The concern would be fouling of the air flow paths by road dust, pollen, oxides, etc.

Specific recommendations and additions or deletions to the work scope

- Recommend deleting freeze system modeling and system impurity modeling, which seem to be more effectively addressed by experimental testing.
- The scope appears to be somewhat fluid, depending upon the needs of the program. This is expected.
- Romesh Kumar should update the H_2 Quality Task Force (and perhaps the Codes & Standards Tech Team) on the N_2 impurity work and determine whether it has any impact/implications for the SAE/ISO list of impurities. Future research plan should be reviewed with Fuel Cell Tech Team for input and suggestions. Review model on automotive fuel cell system with Systems Analysis Tech Team yearly.
- Expand the collaborative information sources and include more "real world" experience.

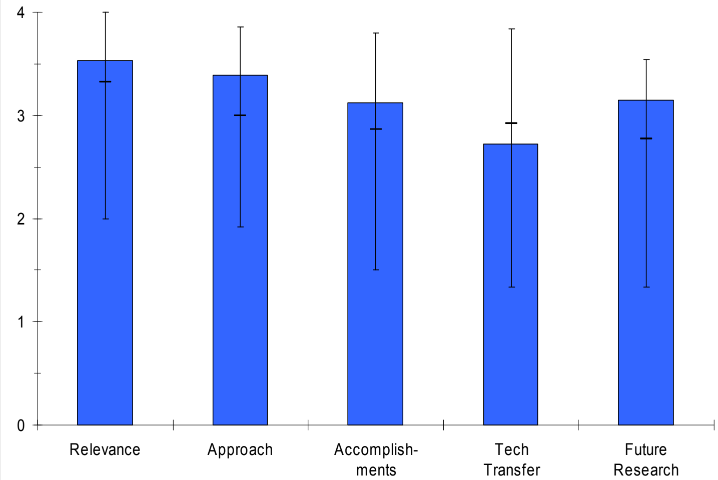
Project # FC-24: Effect of Fuel and Air Impurities on Fuel Cell Performance

Fernando Garzon; LANL

Brief Summary of Project

This project is focused on understanding the effects of impurities in the fuel and oxidant streams on fuel cell performance. The effects of fuel impurities such as hydrogen sulfide and air impurities such as sulfur dioxide and hydrogen sulfide are specifically targeted in this study. Methods to mitigate the negative effects of impurities are being developed along with models of fuel cell-impurity interactions. Collaborations and discussions with USFCC, Fuel Cell Tech Team, Industry and other National Laboratories foster a better understanding of impurity effects.

Overall Project Score: 3.2 (9 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- My view is that impurities are not a high priority compared to other challenges. I understand that I may be in the minority.
- Project is focused on one of the most critical areas. Stable and lower cost electrodes will be required for long term success. We need to understand the lifetime issues of the MEAs.
- Understanding of impurities effect will be important if used appropriately in the development of future fuel purity standards.
- Excellent research effort. The PI did a great job of presenting the results of this research project and did an excellent job of synopsizing how various issues impact system design.
- Very important work relevant to both understanding, and eventually reducing costs, of FC catalysts as well as assisting in determination of fuel specification.
- The project is very relevant to the Hydrogen Fuel Initiative and it is well planned.
- Understanding effect of impurities deals with DOE goals on improvement of durability and electrode performance.
- This project addresses critical issues for fuel cell commercialization and the stated goals of the DOE.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. Results from this research should be used as direct input into the national fuel quality requirements.

Question 2: Approach to performing the research and development

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

- Sound experimental approach.
- More fundamental than last year.
- Could recommend additional ex-situ testing with follow through electrochemical analyses.
- The research approach is excellent in that achieving the initial goals of developing analytical methods of tracing impurities and operating under a variety of conditions should allow sufficient measurements to achieve the breakthrough goal of developing models of fuel cell-impurity interactions.
- Well thought through effort. The appropriate analytical tools are being used to characterize poisoning of Pt catalysts. Nice ongoing and proposed work on competitive adsorption effects.

- Good balance of empirical testing, modeling, and team capabilities to develop fundamental understanding of impurity effects.
- The technical approach is quite focused to understand technical issues in the fundamental level.
- Only some basic clues to the technical barriers have been identified.
- Approach is suitable to the goals.
- The PI presents a good approach and excellent capabilities to perform the work.
- The research approach is outstanding. The methodology being used is very similar to the pure and applied science research of the major fuel cell developers. This approach should be considered the goal for other independent researchers.

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

This project was rated **3.1** based on accomplishments.

- Progress is good and quite valuable.
- Should other sulfur-based species be investigated?
- Salt (chloride) effects on the performance might need to be looked at over a longer period of time and conflicts with existing knowledge. Should try to resolve this.
- Other organic decomposition products should be looked at.
- The impact of the GDL should be considered in greater detail. Is it a dense GDL like ETEK or a more open structure? Does it make a difference (anode or cathode)?
- The effect of water (humidification process) and the impurities should be considered, e.g., H_2S and H_2O electrochemically.
- 0.1% CO is too low – what happens if more realistic concentrations are used?
- Should coolant contamination be considered?
- Supported Pt is a major research topic – i.e., new supports. Is there any planned work on some of the new concepts being proposed?
- Impact of voltage on H_2S adsorption should be further investigated.
- This project's primary objectives and accomplishments to date seem to be an understanding of the physical science causing the barriers, with overcoming them an effort that will be mostly reserved for the future or other efforts. In terms of understanding the barriers, the research team has determined a number of possible reactions and mechanisms that are taking place, in a very controlled environment. These hypotheses will need to be confirmed before the logical leap to real world scenarios can be made with confidence.
- Making very good progress. This effort will is expanding the baseline knowledge of how impurities impact system performance and durability.
- Technical progress is quite significant.
- Coordinated experimental work leads to some valuable data that will benefit fuel cell developers.
- Given the funding level, more progress could be expected.
- Good progress in identifying conditions that lead to cathode/anode contamination/recovery.
- The technical accomplishments to date are very good. Line items need to be completed and information incorporated into complementing commercial activities.

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

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

- Better effort to incorporate field data from demonstration programs would help. Little discussion of the field data and the magnitude of the problem. How are the specific impurities prioritized, air vs. fuel, vs. corrosion?
- Appears to be acceptable for this stage of the program.
- Effort should identify the companies they are working with.
- To date the only obvious collaboration has been to use the hydrogen impurity mixture proposed by the FreedomCAR Fuel Cell Tech Team. With the end goal of influencing the development of codes and standards, increased coordination with other laboratories and industry will be essential; a similar effort is taking place as part of an Argonne project, (FC-23) for example, that will need to be integrated.

- Good teaming with FreedomCAR and USFCC. However the team interactions were not well communicated.
- Continue dialogue with Fuel Cell Tech Team, US Fuel Cell Council, and OEMs.
- Establish synergies with ANL (Debbie Myers) similar impurity studies. Perhaps a divide & conquer approach is warranted?
- This is a weak aspect of the project. Collaboration with other labs/university/industry is ignored.
- Industry collaboration, in particular could be very helpful to address practical issues.
- PI needs to increase collaboration with other institutions.
- A stronger interaction with industrial partners would benefit this program.
- Communications between commercial activities need to be improved.
- Communications and collaboration with HNEI and JARI has been initiated on testing and test methods.
- Communication and collaboration on detection methodology needs to be addressed. Specifically collaboration with the ASTM D03 committee (Raul Dominguez of SCAQMD is the chair).

Question 5: Approach to and relevance of proposed future research

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

- Additional potential sulfur gas phase species should be looked at.
- Additional air contaminants need to be investigated.
- Air cross-over is not always an option – additional operating scenarios may be of interest to investigate (voltage cycling, etc.).
- Study other organic species from the reformed process.
- Work should include other GDLs and other Pt/C supported concepts.
- Still would like to see additional *ex situ* testing, fuel cell testing can be difficult to interpret and is time and resource consuming.
- Cycling to measure recovery impacts, determining fuel performance thresholds, and further investigation of competitive adsorption effects seem to be promising areas of study, only some of which are highlighted in the proposed near-term work.
- Most of the proposed work is the logical extension of the ongoing effort. The multivalent cation work that was proposed is also very important.
- Ambitious project. Agree with direction.
- Continue emphasis on anode fuel specification with more single impurity component testing.
- Emphasize testing and understanding towards lower anode and cathode loadings.
- Investigate impact of membrane thickness? (Ex: Will O_2 crossover impact anode results?)
- Proposed future research is quite relevant to the defined objective.
- The outlined work is consistent to the current progress.
- Future effort should focus on studying the influence of impurities on HOR and ORR in half cells in order to elucidate the mechanism of impurity effects.
- Very good overall.
- The PI should verify that there are not additional air impurities that should be studied as part of the program.
- The proposed near term work plan is extremely good. Work needs to be accelerated if the DOE goals for a national hydrogen fuel standard by 2010 are to be met (California wants it by 2008). Completion of the work on sulfur and ammonia will go a long way towards this industry goal.

Strengths and weaknesses

Strengths
- Good focus on fundamental mechanisms. I would encourage continuing the emphasis.
- LANL continues to build off a strong program.
- Substantial technical expertise on multiple related areas.
- The project is a very useful test of the hydrogen oxidation models, and the development of the low-cost H_2S detector seems to be a key accomplishment, especially if it can be scaled for use by other researchers.
- Very good experimental effort focusing on relevant problems.

- The research team is very strong. Available physical facilities are excellent. Experimental approach is quite focused to relevant problems.
- Electrochemical techniques were complemented by new analytical techniques for sulfur determination.
- The PI presents a good systematic approach with focus on fundamentals.
- The PI presents excellent capabilities to perform the work.
- The analytical work and test results.

Weaknesses
- The connection with validating or modifying the specification is not clear. An effect of poisoning is good, but what does that imply about the proposed limits? Testing is not yet representative of the application, automotive with lots of start/stops.
- The project still seems to lack an ultimate goal/deliverable, as achieving the 2010 fuel cell targets (cost, durability, etc.) Simply by mitigating the impurities is impractical and beyond the scope of this project. If the purpose is to influence codes and standards, then that should be more explicitly stated and have a greater impact on the future research direction.
- Good work showing impurity effects on system performance. Additional work needs to be done to understand what is happening at the molecular level.
- External collaboration is nonexistent. Without industry collaboration, benefits will be marginal.
- Mechanism of the influence of impurities is not clear.
- Experiments should be done to break down the effects from hydrogen impurity mixture better – the PI should confirm some verifiable theories such as that the performance loss is due to resistance change from NH_4^+.
- Flow down of results and application to industry needs. By this it is meant that results are incomplete and not necessarily scalable for the various stack designs. Further collaboration with ASTM, FreedomCAR CSTT, HNEI, JARI, and USFCC should address this issue.

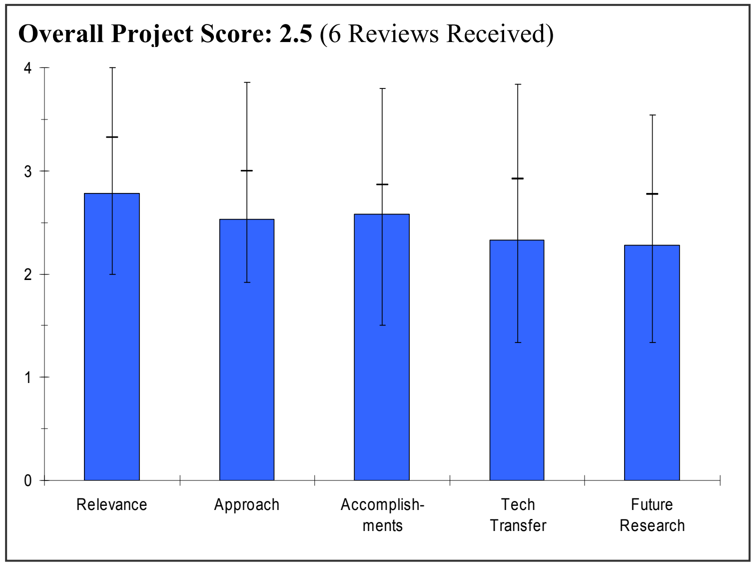
Specific recommendations and additions or deletions to the work scope

- The goal of developing methods to mitigate the negative effects of impurities (beyond cycling which is fairly easy and useful to perform in the controlled environment) could be reserved for future efforts that could be performed as part of a greater systems approach to improving fuel cell performance under real world conditions versus selected impurity mixtures.
- External collaboration is strongly urged.
- Extensive work on CO poisoning and its mitigation should be given special attention.
- Look at lower catalyst loadings in the range of the 2010 loading targets – this may give very different results.
- The PI's explanation of the effect of Na from NaCl (a membrane resistance effect) does not seem consistent with other work done in this area – look at Okata, T. in Handbook of Fuel Cells: Fundamentals, Technology and Applications, Vol. 3, Vielstich, W.; Gasteiger, H. A.; Lamm, A. Eds.; John Wiley & Sons: West Sussex, UK, 2003; p. 627.
- Complete the work on sulfur and ammonia. Interface with HNEI and JARI to demonstrate results are repeatable (at LANL) and reproducible (at HNEI and JARI). Test results should be combined into a single DOE report for each compound tested to be used as the definitive and referenceable DOE documents on the effects of the compounds on PEMFC (and PAFC, AFC – where applicable).
- Work with ASTM on impurities test methods. In the near term, the demonstration programs need to verify and quantify the impurities in the fuel grades being used. Laboratory test methods are lacking. ASTM D03 is generating recognized and published test methods for local commercial laboratories to follow to do the required testing. Input in the writing of these methods and validation of these methods needs to be addressed. LANL could assist in this area in several areas (e.g., the sulfur detection method discussed in this Program Review).

Project # FC-25: High Temperature/Low Humidity Polymer Electrolytes Derived from Ionic Liquids

Jim Boncella; LANL

Brief Summary of Project



The overall objective of this project is to develop high temperature polymer electrolytes for transportation applications of fuel cells. Specific goals include: 1) improving fundamental understanding of conduction in 'free' proton-containing ionic liquids; 2) investigating how phase separation behavior affects conductivity in well defined phase separated ionomers; 3) probing the dependence of properties on ion exchange capacity, water content and temperature; and 4) increasing conductivity at high temperature (~120˚C) and low relative humidity (<50% RH).

Question 1: Relevance to overall DOE objectives

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

- If Polynorbornenes are not good candidates for actual fuel cell electrolytes, this research is several steps removed from usable electrolyte systems.
- Program addresses technical improvement of PEM fuel cells. Program seeks to expand range of operating parameters for PEM fuel cells for automotive applications.
- Very good idea. Ionic liquids need to be evaluated for this application.

Question 2: Approach to performing the research and development

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

- Working with admittedly weak backbone allows better materials science but takes results out of real world. Complete processing steps could point to high cost ultimately.
- Since a high level of effort was made to successfully use polynorbornenes, this effort would need to be repeated using a more suitable polymer in the future. Use of a more suitable polymer at an early stage would be encouraged.
- Program identifies the use of dihydrogen phosphate as one of the anions. The dihydrogen phosphate would lead to anion adsorption on the catalyst and reduce the activity. The approach would probably be of little value. The use of bisulfate at elevated temperatures could be a concern with the dissociation to form sulfur at the anode. This is a temperature limitation. The program addresses the formation of polymers made from the ionic liquids, but should also consider the anion effects before going too far.
- Good possibility for alternative electrolytes. I like the amount of work compared to the somewhat modest $200K/year funding. Researchers need to keep focused on the target conductivity needed for these materials.
- No clear path to a low cost or high durability membrane, promising RH performance but may not lead to a useful membrane.

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

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

- Have not demonstrated ability to create a block polymer. Water solubility will continue to be a problem.
- Progress has been made, but using an unsuitable polymer as the basis of the work is not encouraging.
- Good progress; project in early stages. Movement towards obtaining membrane for fuel cell evaluations in next year is important.
- Slow progress forward. Need to investigate more realistic anions. Needs better synthesis capabilities to integrate the IL cation onto polymers which are capable of being used in a fuel cell.
- The PI needs to do a better job on characterizing these new materials to include: transport, physical properties, etc.
- Lot of progress for the money spent. I would like to see the annual talk have more on the ultimate targets (conductivity and life). I assume the targets are in the back of the minds of the investigators, but it needs to be clearly captured.

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

This project was rated **2.3** for technology transfer and collaboration.

- Good university collaborations, UMass., Virginia.
- No coordination was mentioned.
- One publication. Project does not appear to be coordinated with industry. Any reasons why?
- Moderate teaming. This project needs a collaborator who "knows" (electrochemistry, transport, and physical properties) ionic liquids such as Dr. Paul Trulove, United States Naval Academy or Dr. John Wilkes, United States Air Force Academy.
- Keep in mind an ultimate developer who would use the technology. List of collaborators is growing, which is good. More discussions with 3M, Dupont, etc. Would be valuable, but only on a precompetitive basis (at this point). The technology is new enough that it is not appropriate to go proprietary at this point.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- No clear plan for incorporating block copolymer. No clear plan for a stable backbone.
- Future work still continues to plan to use norbornene polymers. This is not encouraging.
- Future work identified does not show correspondence to fuel cell applications. The project needs to be directed to solve fuel cell issues. Critical issues appear to indicate that the project may not lead to advanced membrane for PEM fuel cells.
- Future research not discussed in the slides. Some future research areas where mentioned in the question and answer period.
- Still early in the development of these compounds and LANL has a good broad-based plan.

Strengths and weaknesses

Strengths

- The project team appears to understand the electrochemistry very well. They appear to be experienced and knowledgeable. They also understand why these polymers are not suitable for fuel cell electrolyte.
- Good idea to investigate ionic liquids for fuel cell applications. Covalent attachment of ionic liquids to polymers is innovative.
- LANL fully understands fuel cell requirements.

Weaknesses

- Have not achieved a stable, non-soluble membrane. No clear approach (other than "thinking with chemistry") to achieving a useful membrane. No identified backbone.
- The basis of the work is using a polymer that is admittedly not suitable for fuel cell electrolytes.
- Good work showing impurity effects on system performance. Additional work needs to be done to understand what is happening at the molecular level.
- Input from knowledgeable reviewers (3M, DuPont, etc) is valuable.

Specific recommendations and additions or deletions to the work scope

- Funding at this modest level of effort is not likely to lead to useful, practical results.
- The project should start to work on more promising polymer types.
- PI should investigate preparing and characterizing polymer/ionic liquid blends. This technique has been used previously to prepare gels.

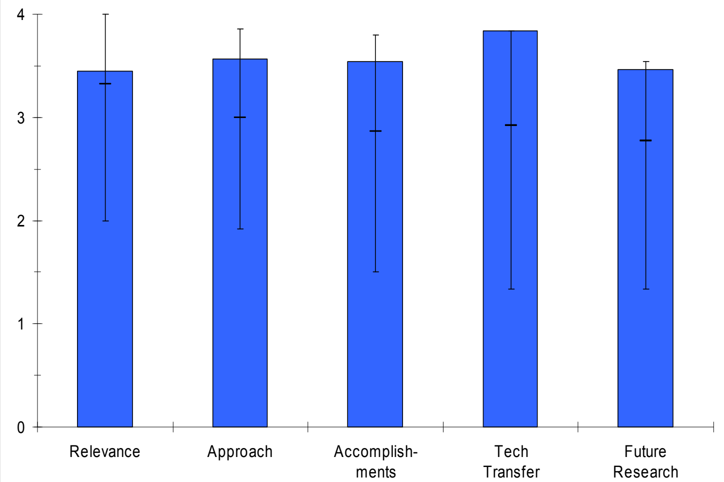
Project # FC-26: Neutron Imaging Study of the Water Transport in Operating Fuel Cells

Muhammad Arif; NIST

Brief Summary of Project

The goal of the National Institute of Standards and Technology project is to develop effective neutron imaging based, non-destructive diagnostics tools and an experimental facility to characterize water transport in operating PEM fuel cells. Objectives include providing research and testing infrastructure to enable the fuel cell industry to test commercial grade fuel cell flow field and MEA designs; training industry to enable it to use the imaging facility independently; and transferring data interpretation and analysis algorithms/ techniques to industry to enable it to use research results more effectively and independently.

Overall Project Score: 3.5 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Water management in fuel cells is required for efficient operation. This project provides a novel tool for visualizing water *in situ*. The data obtained are critical for flow field design and thermal issues. The work is important for realization of the President's Hydrogen Fuel Initiative.
- Water management is one of the key enablers to durable fuel cell design. This project plays a very significant role in the understanding of water management and facilitating design.
- Water management is a major issue.
- Understanding water behavior is critical to managing and stabilizing performance during transients, steady-state operation, and start-up/shutdown cycles. This technique facilitates this understanding.
- Neutron imaging at NIST is proving to be a valuable tool to visualize water flow in operating fuel cells. It is extremely relevant to the DOE Hydrogen Program as evidenced by the many requests for work under proprietary agreements.
- The project is highly relevant to DOE objectives.
- In-situ monitoring and analysis of water in an operating fuel cell represents a unique capability that could provide useful information.

Question 2: Approach to performing the research and development

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

- The project is well designed and the strong push to lower size scales makes the potential impact of this technique much stronger. The addition of tomography and low temperature control is likewise beneficial.
- Approach will result in geometry-specific results.
- Approach should include working towards models.
- The approach is good.
- The Advanced Fuel Cell Imaging Facility is state-of-the-art. The addition of a freeze chamber is an important addition to the existing apparatus.

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

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

- It is easily seen how the results to date can be useful for modelers and flow field designers. The project doesn't involve direct research, but the data presented in collaboration with others are insightful.
- State-of-the-art facility.
- Facility and capabilities are impressive but results to date are not.
- NIST has made crucial progress in the area of spatial resolution, gaining insight not only across the facial area of the fuel cell (x and y directions) but also in the z direction.
- NIST is completing the studies on schedule and they showed several examples of the results of their work in the presentation.
- The research team has made good progress in monitoring water generation and distribution in an operating fuel cell. It is not clear how data collected at the facility are being used by modelers who are studying water management in an operating fuel cell (i.e., where is the match/comparison of experimental data and model predictions?).
- This new facility should be very popular.

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

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

- Project inherently involves a number of other participants. The structure to be limited to 50% proprietary data, limits this aspect of the project in score, but is acceptable.
- Excellent balance of open and proprietary research.
- Are all FC publications resulting from the NIST imaging facility use listed on one site? If not, recommend listing on their website.
- Essentially all work is transferred either public information or to proprietary partners.
- NIST has a very broad spectrum of collaborations comprising component developers, stack manufacturers, fuel cell system integrators, and users. Facilities are available to everyone.
- The NIST neutron source is considered a national user facility. Therefore all of the work that NIST undertakes in this area is as a result of a collaboration with fuel cell developers.
- The imaging facility has a nice group of users and collaborators. I was surprised and somewhat disappointed that LANL was not on the user/collaborator list.
- Probably the highest technology transfer project in DOE. A very impressive leveraged project. It would be good if such technologies and lessons learned can be transferred to other facilities with neutron imaging capacity to help better manage the resource demands.
- Excellent collaboration with industry, much better than the average project. Good to see that industry is sharing in providing funds.

Question 5: Approach to and relevance of proposed future research

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

- Tomography and lower size scale resolution are certainly useful. The results shown to date suggest advancements can be made in this area. It would be useful to understand the ultimate limit of size resolution to better understand the ultimate limit of this technique.
- If dynamic gas flow capability (1-2 second response) is not yet available, consider for future development.
- Freeze/thaw planned for following year will be a strong added capability.
- Good, but should include collaboration with model developers.
- Enhancing spatial resolution will be extremely valuable. Addition of freeze/thaw capabilities is also important.
- NIST is installing an environmental chamber at the beam line that will have the capability of conditioning the fuel cell at various ambient conditions including temperatures as low as -40°C.

- More collaboration with researchers studying water management is warranted. Future research should focus on water generation and distribution in a fuel cell stack, as opposed to a single MEA.
- How do you plan to accommodate more users?
- Freeze chamber addition and tomography good.

Strengths and weaknesses

Strengths
- Novel, useful technique. Project leveraged through other funding sources to improve cost/value ratio for DOE.
- Well-deserving of their American Competitiveness Initiative recognition!
- Very powerful tool with potential for much useful input.
- NIST is able to complete the agreed upon work on schedule as presented. Environmental conditions down to -40°C ambient can be achieved.
- The facilities are excellent and the addition of a freeze chamber will enhance the Imaging facility's capabilities.
- Only 20% of the facility's annual budget is provided by the DOE. The project team has successfully attracted users/collaborators that can pay for use of the apparatus.
- There is a nice mix of users/collaborators from academia, industry, and government labs.
- A tool for all to use. A good balance between proprietary and non-proprietary work.
- Open tool available to all. More than other projects, this activity advances the fuel cell technology for everyone.

Weaknesses
- Size scales probed are still too large for a number of fundamental aspects that would be interesting.
- As currently operating, work is mostly a development tool for specific designs.
- It is not clear if this technique can distinguish the 3 states of water.
- The interpretation of data is weak. For example, experiments showed that there is a maximum in the water content of an MEA at a point between the anode and cathode (closer to the anode) but this finding is not intuitively obvious and it has not been verified through additional experimentation or by theoretical modeling.
- There is no collaboration with the fuel cell group at LANL.
- The 3D tool needs to be ready soon. It is still unclear how a 200+ cm^2 multicells stack can be evaluated with this technique on a cell to cell basis at this moment.

Specific recommendations and additions or deletions to the work scope

- Project is worthwhile, and researcher involved has provided solid results.
- Try to use the impressive capability to help produce (through appropriate partners) models and simulation capabilities.
- NIST should ensure that reactants can be conditioned to the same temperature as the fuel cell when cold start experiments are performed. This capability was not mentioned in the presentation.
- Work more closely with fuel cell developers/modelers, especially at LANL.
- Carry out experiments in a fuel cell stack.
- Please invest more resources into developing software that would help extrapolate information for large multicell stack development.

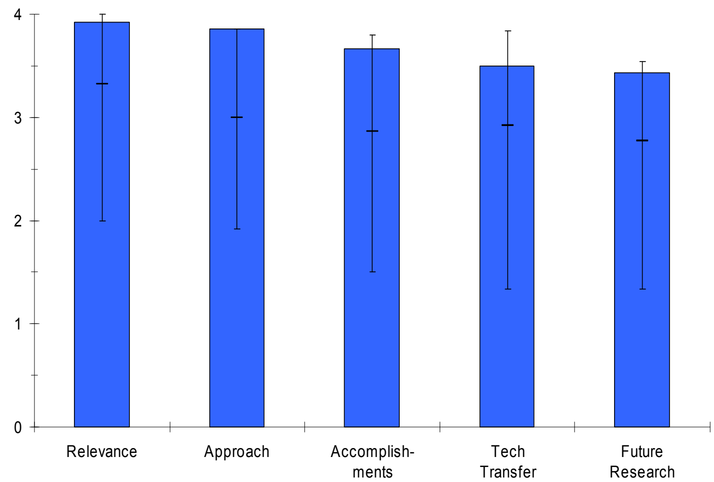
Project # FC-27: Microstructural Characterization Of PEM Fuel Cell MEAs

Karren More; ORNL

Brief Summary of Project

The project at Oak Ridge National Laboratory (ORNL) is focused on elucidating the mechanisms contributing to performance degradation of PEMFC membrane electrode assemblies (MEAs) by conducting extensive microstructural and compositional analyses using high-resolution electron microscopy and spectroscopic techniques. These analyses are performed on (1) the individual MEA constituent materials (i.e., electrocatalyst, catalyst support, membrane, ionomer) and (2) fully-fabricated MEAs, which have been subjected to a variety of processing and/or electrochemical aging conditions.

Overall Project Score: 3.7 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.9** for its relevance to DOE objectives.

- As MEA component materials are developed for the stringent automotive MEA requirements, the problems and issues require increasingly greater in-depth analyses. The ORNL TEM project is a great example of a successful program accomplishing just that. One might always argue that even without this technology analysis, the H_2 Initiative barriers will be overcome, but it may take longer and cost more.
- This project is developing key analytic tools needed by fuel cell technology developers to solve their specific degradation mechanisms.
- This highly valuable characterization technique can contribute to minimize the use of precious catalyst, and improve the durability of electrodes and membranes.
- Characterization is essential to improving the performance of the fuel cell MEA – a key component of fuel cell systems. Long-term durability is a critical issue in fuel cell development.
- The project is very relevant to the Hydrogen Fuel Initiative and of practical importance. It is consistent with the DOE's multi-year goals and objectives.
- Extremely important to have a better understanding of MEA degradation issues.

Question 2: Approach to performing the research and development

This project was rated **3.9** on its approach.

- The approach itself cannot of course develop or invent new materials that will directly remove a barrier. However this work is very critical for decision making on what material sets have the potential, or do not have the potential, to overcome critical barriers to the H_2 Initiative.
- The materials characterization capabilities of ORNL seem to be well applied to this area.
- The project clearly demonstrated the usefulness of this technique to meet the DOE target especially on MEA durability.
- Developed and used a "treasure chest" of characterization tools to study durability and performance of fuel cell catalysts.
- Technical approach is very sound and well coordinated.
- Work plan is well-designed; practically feasible concepts are being pursued.
- Provides a unique view of electrochemical aging which can lead to better designs for durability.

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

This project was rated **3.7** based on accomplishments.

- Program has made excellent progress towards meeting its objectives. The project by its nature identifies things that go wrong rather than what went right with aged MEAs. This in and of itself is very important, but cannot really chart a path to tell how a barrier will be overcome.
- The aging effect to the catalyst growth and migration within the electrode and into the membrane has been clearly validated.
- Significant progress has been made in using TEM characterization to follow catalyst degradation as a function of operational lifetime.
- The technical progress is very significant.
- Experimental data should be quite helpful to the understanding of some key technical issues.
- No remedial methodology has yet been developed; more extensive work needed for practical benefits.
- Very impressive and very important results.

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

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

- It would be interesting and potentially useful to validate results from other DOE sponsored research projects using these analytic tools.
- FreedomCAR & Fuels Partnership funds should not be going toward proprietary efforts with developers. If the developer is paying full cost, then it is OK. FreedomCAR funds directly to the National Labs should benefit everyone. ORNL seemed to be an outlier in the amount of proprietary work underway.
- Broad interaction with MEA industry was mentioned.
- The detailed procedure needs to be shared with other public and private institutes who can use this excellent technique for their MEA development.
- Extensive collaborations will both other National Labs and industrial organizations.
- Project relies on input (characterization requirements) from other organizations.
- Information gained from this project should help extend the performance lifetime of the catalyst.
- Level of industry/university/other laboratory collaboration is commendable.
- Practical issues encountered by industry partners should be given due priority.
- Good, but there could be better transfer of information.

Question 5: Approach to and relevance of proposed future research

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

- More of the same will be excellent.
- Verbal statements made in presentation clarified future work.
- More quantitative analysis is needed.
- Future work is planned to bring new characterization techniques on-line as well as continue the important collaborations that are already in place utilizing TEM characterization.
- Proposed future work is very relevant to practical issues and consistent with the outcome to date.
- Proposed collaborative work with membrane/MEA developers should be beneficial to the industry.
- Hard to fault an ordered continuation of this work.

Strengths and weaknesses

Strengths

- Expertise of the PI and ORNL TEM laboratory.
- This is highly relevant work. The industry needs to have the tools developed which can be used to understand and accelerate development of this technology. The results from this project are primarily in the catalyst degradation area which is of high relevance to performance and lifetime.
- Great technique for understanding Pt migration and clustering. Good national resource.
- An excellent microstructural analysis tool has been delivered.
- The application of existing characterization tools at the National Lab to address problems at other organizations including those in industry represents an important return on the "public" investment.
- The PI clearly demonstrates the knowledge required to interpret characterization data in terms of optimizing fuel cell performance.
- The research team strength is outstanding. Physical facilities available for the project are excellent. Collaborative partnership is most helpful and is a big plus.
- Impressive use of sophisticated techniques.

Weaknesses

- Assessment of membrane morphology has only started and it is unclear its value to the general PEM research field. Further to this, developing tools to understand PEM degradation mechanisms is an area worthy of future research. I suspect that work has been done in this area but is proprietary and not publishable.
- The only non-proprietary work seems to be what is being done with LANL. Non-proprietary work should increase.
- No collaboration activity to transfer the technique.
- The research team should have given more attention to develop remedial approach (es) to address some of the issues.

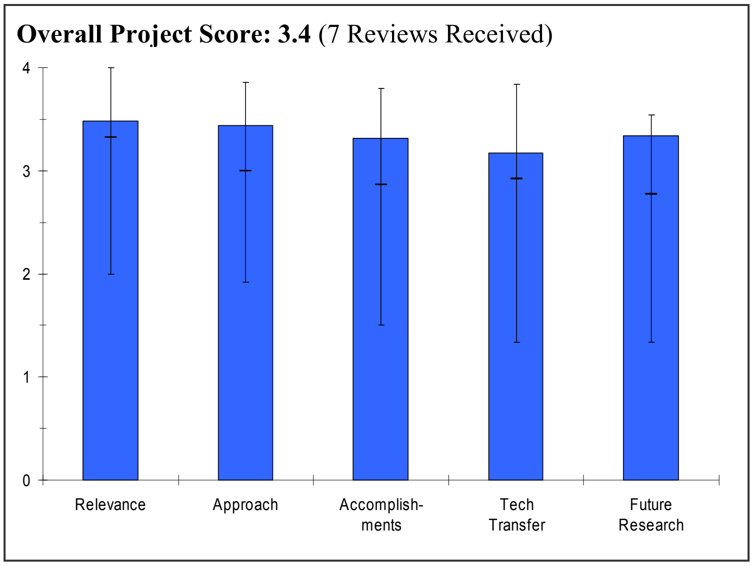
Specific recommendations and additions or deletions to the work scope

- ORNL should not apply FreedomCAR funds to the proprietary part of the work.
- The PI needs to transfer the technique to be public.
- Keep up the good work – if demand for this characterization support exceeds current capability- funding (which is modest) should be expended to help meet requests for support, particularly from industrial organizations.
- Work on new membrane/catalysts/MEAs should be extensively pursued.
- Perhaps a little more emphasis on technology transfer.

Project # FC-28: PEM Fuel Cell Durability
Rodney Borup; LANL

Brief Summary of Project

In this project, Los Alamos National Laboratory is identifying and quantifying factors that limit PEM fuel cell durability by measuring property changes in fuel cell components during long-term testing (membrane-electrode durability, electrocatalyst activity and stability, gas diffusion media hydrophobicity, bipolar plate materials, and corrosion products) and developing and applying methods for accelerated and off-line testing. The overall objective is to meet the 2010 DOE target of 5000 hours with cycling.



Question 1: Relevance to overall DOE objectives

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

- Presentation stressed performance, not causes of degradation.
- Project is focused on one of the most critical areas. Stable and lower cost electrodes will be required for long term success. We need to understand the lifetime issues of the MEAs.
- This effort continues to pull together the critical understanding of the elements of the electrode.
- This research project examines a range of MEA durability issues. Durability is an important issue.
- Nice project. Doing a good job of trying to nail down several "real life" issues that will dramatically impact fuel cell durability.
- Fuel cell durability is one of the two or three primary enablers to FC commercialization.
- This project is directly focused on identifying the mechanisms affecting degradation durability which is a prime barrier to fuel cell commercialization. As such, it directly supports the Hydrogen Fuel Initiative and the MYPP.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. The results from this effort are directly supportive of industry efforts.

Question 2: Approach to performing the research and development

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

- This project seems to be working at the early stages of durability analysis. Additional work is necessary to draw cause-and-effect relationships
- Between changes in the membrane, the GDL, and the catalyst and changes in cell and stack performance. Additional work beyond this
- Effort is necessary to understand the cause of the changes being seen in the various materials.
- Sound experimental approach.
- Fundamentals continue to remain the main focus.
- The collaboration with ORNL is excellent, but even further integration of the two programs should be considered.
- A variety of different durability tests was performed. Accelerated testing of fuel cell MEAs is important, but such experiments must be carefully scrutinized to insure that the results mimic the actual degradation that will occur during long-time fuel cell operation. Although one of the objectives of this project is to "design new materials with improved durability," no such materials were proposed.

- Good solid approach characterizing the effects of fuel cell cycling, platinum particle growth. It would be very beneficial to understand the mechanism by which this occurs (physical migration or dissolution followed by deposition). Also what is driving this phenomenon? Making good progress figuring this out.
- GDL characterization effort good. Needs to be supplemented with some surface characterization (XPS, Auger, TOF-SIMS).
- Strong combination of testing capability, diagnostics and analysis.
- Project continues to provide good insights into fuel cell durability issues.
- The project is sharply focused on fuel cell durability, looking at many of the key degradation mechanisms.
- Very clear, lucid approach to the examination of PEMFC degradation mechanisms.
- Project is developing modeling activities in an attempt to further understand and correlate empirical degradation testing results. This is a good idea; however, a note of caution is that modeling development should not come at the expense of the excellent progress being achieved via durability testing. Project is looking at so many areas that there is a risk of small degradation nuances being missed.
- The approach to this project is well ground in sound engineering practice and managerial judgment.

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

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

- Membrane modeling of platinum particle size growth was compared to TEM scans, showing good correlation.
- Technical accomplishments are significant and relate well to the operating modes of the fuel cell.
- More delineation of the MEA components needs to be included (GDLs and electrode layers are being investigated and modeled, but are these representative of what the industry is developing?) Not all GDLs are the same but it is good to see they were identified.
- Need to take the technical knowledge of this excellent program and work with the industry suppliers and fuel cell developers even more closely.
- Would like to see actual mechanisms identified. To date, the group is still in a "measurement" phase.
- Can this effort develop a standardized post mortem testing protocol?
- Good to see transient behavior studied.
- Catalyst agglomeration models exist in other industries (should utilize the existing knowledge).
- Loss of hydrophobic character: General Electric showed very long GDL life 30 years ago – why is it different today?
- Lots of data were collected by this research team, but the interpretation of the results was rather weak and there was no suggestion(s) as to how one might improve the properties of various fuel cell components to minimize/eliminate degradation.
- Good progress forward. Many new important results.
- Many effects observed and measured (changes in hydrophobicity/hydrophilicity, GDL porosity, etc.).
- Causes for these effects are postulated, but need further validation/verification. Looking for the team to close the loop on the observations and insights they've gained these last couple of years to prove mechanisms.
- Excellent technical progress achieved in all areas in identifying, examining, and elucidating degradation mechanisms.
- Prodigious technical output quantifying factors limiting PEMFC durability.
- The accomplishments to date are impressive. Relevant tools are being generated and properly applied.
- Data mining may be in order. There might have been some research in the hydrophobicity of carbon substrates as part of the PAFC research in the 1980's. Review of the quarterly reports to NASA and to DOE METC during this time frame may be of value.

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

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

- Good coordination with other labs and groups.
- Should increase activities even further.

- It is not clear how industry, universities, and other National Laboratories are using the data collected by this research team. There needs to be closer collaboration with other fuel cell groups (e.g., catalyst degradation should be examined in collaboration with a fuel cell catalyst group).
- Good teaming. The PI has assembled a good team to obtain and characterize PEM fuel cell degradation. Need additional surface characterization team members.
- Strong visibility through publications, presentations and reputation in community.
- Good collaboration within various National Labs.
- Increase collaboration with industry material/component developers.
- Good interaction and coordination with other institutions and projects.
- PI may want to consider downloading a greater degree of the project tasks to other institutions if feasible given the extent of work activities.
- Collaboration to date has been excellent. Additional potential contacts could be found among the USFCC material & components working group, durability task force.

Question 5: Approach to and relevance of proposed future research

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

- It was difficult to draw conclusions regarding causes of degradation from the presentation. Although much data was presented, there was not much regarding the causes of the changes and not much data regarding the effect of the changes on cell performance. Additional work should add efforts to identify causes of degradation.
- Work as much as possible with industry standard materials.
- Better coordination is needed between this research team and other DOE-sponsored research groups. There seems to be considerable duplication of effort on many durability topics, such as catalyst particle growth and the degradation of GDL and membrane materials.
- Good future research proposed. Research ideas relevant.
- Aggressive future activity schedule.
- PI should consider narrowing down somewhat future scope to look more deeply at a few key areas and run more tests within each area.
- The list of future activities listed is impressive. Catalyst durability information may be available from PAFC research in the 1980's. Documenting and publishing suitable test protocols may be warranted.

Strengths and weaknesses

Strengths
- Excellent results regarding degradation.
- LANL continues to build off a strong program.
- Substantial technical expertise on multiple related areas.
- This research team has performed numerous experiments and has collected a large quantity of data over the past year.
- Qualitative conclusions have been drawn on some results.
- Very relevant project. Good results.
- Very strong diagnostics and analysis capabilities.
- Excellent test capability, including transient control.
- Very strong laboratory approach with prodigious and excellent *in situ* and *ex situ* testing results.
- Project is casting excellent insight to the broad picture of fuel cell degradation mechanisms.
- The focus and diversity of the team.

Weaknesses
- Limited data on causes of degradation.
- There was no identification of new materials with improved durability.
- There was little interpretation of degradation data.
- The materials cost issue was not addressed (one of the stated barriers).
- PI needs to perform additional surface characterization of GDLs.

- PI should be careful not to lose the excellent focus and progress achieved so far by attempting to cover too many areas simultaneously. Possibly consider greater use of collaborators.
- The apparent lack of reviewing previously sponsored work for the US Government.

Specific recommendations and additions or deletions to the work scope

- Spend more time on causes of degradation and mitigation in the future.
- There needs to be more collaboration and less duplication of effort. Better coordination of MEA durability studies, from all DOE-sponsored groups, is warranted.
- Evaluate hydrophobicity of the GDL in both oxidizing and reducing environments. Publish durability test methods through a recognized authority. Two options might be the USFCC and SAE.

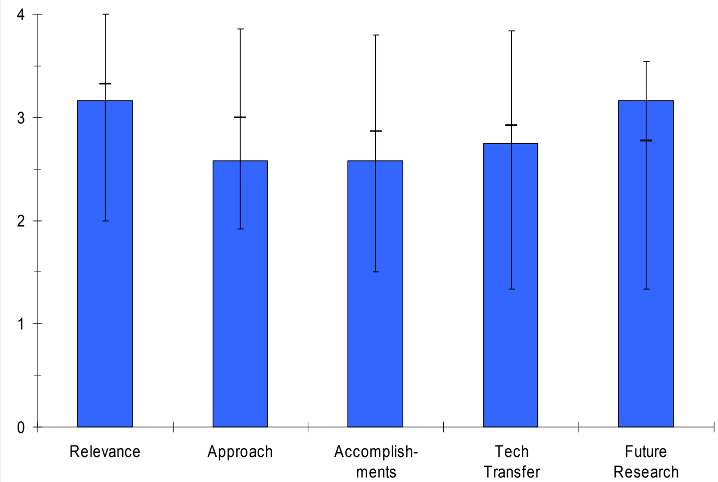
Project # FC-29: Investigating Failure in Polymer-Electrolyte Fuel Cells

John Newman; LBNL

Brief Summary of Project

This project focuses on examining various types of fuel-cell failure, including both mathematical modeling and supporting experimental studies. The issues investigated include fuel-cell water and thermal management, subzero operation, membrane degradation, and mechanically-related failure. By understanding and describing these phenomena through both modeling and experimentation, failure points, such as water depletion due to membrane stress effects, and conditions that lead to failure can be identified and minimized through subsequent theoretical optimization of material properties, operating conditions, and possibly start-up and shut-down scenarios.

Overall Project Score: 2.8 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Presentation disjointed. Very difficult to follow presentation and understand subprojects being worked.
- Theoretical analysis is essential to address the durability issues.

Question 2: Approach to performing the research and development

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

- The approach looks good in the slides. It did not come across during the presentation. Membrane constraint result interesting.
- Model development is good, but make sure validation is a continuous process.
- Experimental validation of the model needs to be added.
- Most of the work is on modeling normal fuel cell performance instead of simulating failure modes.

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

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

- Project appears disjointed. Project has questionable relevance.
- Hard to distinguish what was done in the past year vs. literature and older modeling work.
- There is too much emphasis on modeling fuel cell performance in model operation mode. Many researchers already have modeled and reported similar results.
- No experimental validation of the modeling results is shown.

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

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

- Minimal teaming. However, funding of effort is small.

- In transferring results by presentation, it would be useful to have one chart with text (maybe small print necessary) informing/reminding the audience in words what phenomena are included in or excluded from the model.
- For this small project, it is critical that it be integrated with partners such as LANL and Georgia Tech, provided they have parallel programs on failure also.
- Industrial partners, who can commercialize the catalyst, will be a beneficial collaboration.
- Are there any opportunities to model some of the public NIST water results as a way of model validation?
- No results from the collaborators are shown. More collaboration is needed to validate the modeling results.

Question 5: Approach to and relevance of proposed future research

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

- Future research proposed is reasonable.
- Further challenge could be to model the thermal/chemical/mechanical stress coupled phenomena occurring around a pin-hole or membrane breach often found near edge of MEA.
- Do we know peroxide production vs. Temp, H_2 flux, potential?
- Can a model be developed explaining why OCV and low RH conditions are so accelerated?
- The effort as proposed is very logical.
- The PI should focus more effort on membrane degradation work, particularly mechanistic understanding.
- Investigating the major failure mechanisms is planned.

Strengths and weaknesses

Strengths
- Membrane constraint results interesting.
- Approach taken of modeling at fundamental level is sound and not easily carried out by many other institutions.
- Use of nano-size bimetallic catalysts is quite promising. Analytical tools used for screening are effective. Alternate method for synthesis.
- Water and thermal management work addresses critical issues.
- The PI offered an interesting explanation of Schroeder's Paradox.
- Planned stress model development good plan.
- The PI has very strong skills and experience in theoretical analysis of electrochemical systems.

Weaknesses
- The membrane degradation studies with Georgia Tech proposed look like work which has already been published by DuPont and GM. It is not clear how this will be different.
- More collaboration is needed for comprehensive analysis of the failure mechanisms.

Specific recommendations and additions or deletions to the work scope

- If not already done, consider a review of similarities and differences between LBNL/Newman model(s) and ANL/Kumar system model.
- Could LBNL be used as input/"back-office" of ANL?
- Low temperature studies of membranes are probably not relevant to real cold start and freeze/thaw issues.

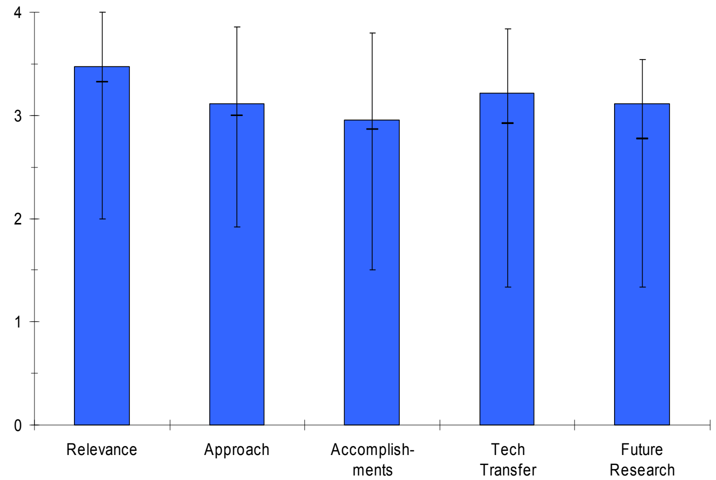
Project # FC-30: Sub-Freezing Fuel Cell Effects

Rangachary Mukundan; LANL

Brief Summary of Project

The overall objective of this project is to assist the DOE-HFCIT Program in understanding the role sub-freezing temperatures play on fuel cell performance and durability to meet DOE milestones for sub-freezing startup (-20°C, 30 sec, 5 MJ) and survivability (-40°C). Characterization of water in ionomer, catalyst, and gas diffusion layers is being done and degradation mechanisms are being identified including freeze/thaw cycling (ice formation), startup and shutdown, and thermal cycling.

Overall Project Score: 3.1 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Freeze start and degradation are key barriers to automotive fuel cell commercialization.
- Important work that needs to be done. Understanding cold start-up and temperature cycling induced failures is critical.
- Sub-freezing studies of PEM fuel cells are important.
- This project supports the goal of ensuring that fuel cell technology works in all weather and climates – a vital factor for wide acceptance of the technology.
- Understanding of fuel cell freezing effects are critical to fuel cell applications.
- This work is essential to having workable vehicle systems in the world market.

Question 2: Approach to performing the research and development

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

- Nice combination of material properties, necessary for understanding, and proposed mechanism development.
- Experimental characterization of water in the ionomer, catalyst and gas diffusion layer is well underway. Identification of the degradation mechanisms during ice formation, startup and shutdown and thermal cycling is also underway.
- Good approach. Doing a good job testing the worse case scenario.
- The state of water in Nafion is well understood. The research team's interest in examining non-Nafion membrane materials is worthwhile, but I do not understand why they are focusing so much of their time and effort on the BPSH material. Both U.S. and Japanese automakers have no interest in this type of membrane, for a variety of reasons.
- The series of studies on fuel cell components seems to be reasonable. Studies should be broadened to complete systems, including designs in balance-of-plant to alleviate the freezing problem.
- Extending work to non-Nafion membranes is key to future developments.
- Good research, focused on hardware effects.

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

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

- Reasonable progress so far. State of water and conductivity measurements.
- Need to test greater # of freeze/thaw cycles. Target around 1000+.
- Need more post-mortem diagnostics, ECSA, etc.
- An understanding of the state of water in Nafion at different RH and the RH effect on bound freezing water and non-freezing water was presented. Conductivity measurements have been made. Water of hydration measured. Freeze/thaw cycling of MEAs is performed. Mechanical degradation of GDLs observed. Start-up performance measured under fully humidified conditions with different GDLs. Flow problems investigated.
- Overall good technical accomplishments. The temperature cycling results are very important. DSC results are suspect, at a minimum, results skewed by over 15 degree Celsius. A little more careful experimental technique will go a long way.
- A great deal of data has been collected, but the interpretation of the results is weak and there was no suggestion as to the use of new materials. It appears that freeze/thaw cycling has little effect on the voltage-current density behavior of an MEA (for 45 or 100 cycles). Such data indicate that there is no real problem with regards to freeze/thaw events. Since there does not appear to be a degradation mechanism associated with freezing and cold start-up, perhaps this work should be de-emphasized in the coming years.
- This project is focused on identifying causes of failure in subfreezing conditions, but only indirectly with remedying them.
- Some of the experiments were not able to demonstrate performance degradation with freeze cycling, and this needs to be sorted out to better interpret failure modes.
- Interesting work. The results showing no degradation after freeze/thaw cycles are counter-intuitive, but well documented for a low number of cycles. GDL fiber breakage is another interesting result.

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

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

- In contact with both National labs and OEMs – good.
- Partners with other National Labs like Sandia and LBNL and also with GM and GE through sharing of data.
- Very good teaming. UTC Fuel Cells/UTRC, General Motors, Plug Power, and Hunter College.
- Collaborations with industry/universities/other labs are weak. Other groups are examining degradation mechanisms.
- Project spans three National Labs and two large industrial partners. There seems to be a lack of coordination with a similar project at ANL (FCP-23). Project should include coordination with suppliers of components being tested, so as to work towards subfreezing-compatible components and system designs.
- Nice work in support of industry. Other experimental and modeling efforts are going on at LBNL and ANL. The efforts could be coordinated to avoid duplication and yield a better understanding of effects. Improved measurement technique from initial data presented last year.
- It appears that this work should be better coordinated with the Argonne modeling effort.

Question 5: Approach to and relevance of proposed future research

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

- Extend start testing to colder temperatures, to -40°C. While the DOE roadmap indicates fast start from -20°C, unassisted starts will be required at lower temperature.
- Consider testing with different catalyst layers (thickness, loadings) to assess impact.
- Conductivity effects on non-Nafion® membranes during freeze will be interesting. Transient responses and mechanical property and degradation mechanism studies are indeed needed.
- Good future research plans. Understanding membrane thinning and failure is important.

- Future work, as indicated on the Project Summary slide is of limited scope and lacks details. What electrical components will be characterized? From recent experiments, it can be concluded that there does not appear to be any degradation due to freeze/thaw cycling. There is essentially no effect of cold start on fuel cell performance at high voltage (about 0.6V). So why spend time and money to look at such effects? Examination of cold starts and freeze/thaw events in a fuel cell stack (as opposed to a single MEA) would be worthwhile.
- PI indicates plans to procure environmental test chambers for component research – a worthwhile extension. Future research should also include tests on complete systems.
- Need to add analysis of freeze/thaw and start/stop dynamics. More data points to get meaningful statistics for degradation and failure rates.
- It would be good to add higher cycle testing, to simulate a full stack life cycle for an automotive system.

Strengths and weaknesses

Strengths
- Teaming and thorough experimentation looking at the fundamental steps in freeze/thaw should provide information for mechanism development.
- Good temperature cycling results.
- The research team is well qualified to study durability issues. The team has collected a great deal of data over the past year and has made some important findings, in particular that freeze/thaw cycling has little effect on fuel cell performance.
- Good start for a project that covers a fundamental program need – analysis of failure modes of FC components at subfreezing temperatures.
- Highly relevant work in support of fuel cell industry. Strong leadership by LANL and good collaboration with GM.
- Good work focused on real hardware effects.

Weaknesses
- Need better characterization of water environments in the membranes.
- There is little coordination between the work of this team and the research efforts of others who are looking at degradation issues. The team's focus on BPSH-type polymers as alternatives to Nafion is misdirected and will eventually hurt the credibility of the team and the DOE. Automotive companies are not interested in the BPSH materials. The free water content of BPSH-30 and BPSH-40 were contrasted with Nafion in one slide, but these materials have a proton conductivity considerably lower than that of Nafion, so any comparison of water content is unwarranted.
- Project coordination between this work and that at ANL not clear – potential duplication of effort. In addition to components, PI should articulate goal to develop testing program for complete systems, with and without design features for mitigating subfreezing effects.
- Lacks coordination with ANL/LBNL efforts in this area.
- Higher cycle testing is needed.

Specific recommendations and additions or deletions to the work scope

- Would be nice to see how the MEA performance is altered during freeze/thaw. ECA measurements, change in gas crossover in addition to mechanical property measurement would be nice.
- The research team should carry out fuel cell stack tests. Experiments should be coordinated with fuel cell system people, e.g., should water be purged from the system prior to shut down and freezing or should water in the system be allowed to collect and freeze, with the eventual thawing of any frozen water during start-up.? Wind down the freeze/thaw work, since experiments have shown that such events do not affect fuel cell performance.
- Addition: testing of complete systems.
- Need more data for better statistics of failure modes. Environmental chamber added at NIST should allow neutron imaging to be added for next year.
- Full system testing would be a positive addition. This might be integrated with Argonne testing of the Plug Power system.

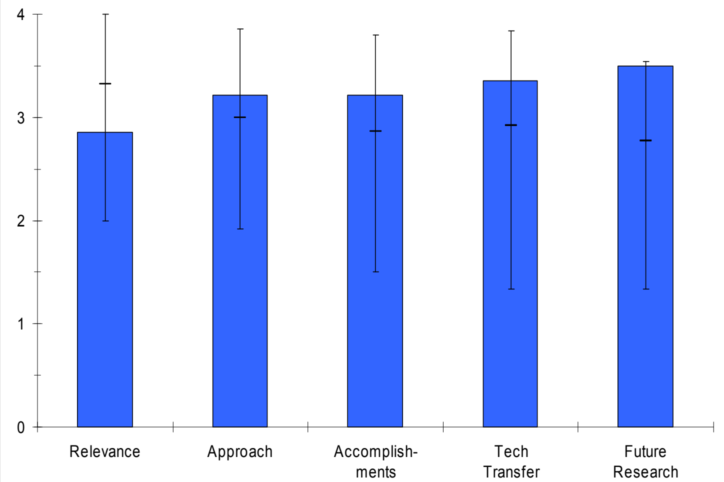
Project # FC-31: Back-up/Peak-Shaving Fuel Cells

Daniel Rodriquez; Plug Power

Brief Summary of Project

The objective of the Plug Power project is to advance the state-of-the-art of fuel cell technology with the development of a new generation of commercially viable, stationary, back-up/peak-shaving fuel cell systems. Plug Power is developing, building and testing three identical fuel cell back-up systems and is field testing them at three sites, including an industry host site to identify technical barriers. Other objectives include developing a cost-reduced polymer electrolyte membrane fuel cell stack tailored for hydrogen fuel; developing a modular, scalable power conditioning system tailored to market requirements; designing scaled-down, cost-reduced balance of plant; and certifying the design to Network Equipment Building Standards and Underwriters Laboratories.

Overall Project Score: 3.2 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The product is not directly related to vehicle programs but could lead to near-term high value products.
- High value products allow fuel cells to be deployed earlier than vehicle systems will accept the high cost of current fuel cell systems.
- While backup power/peak shaving applications could generate demand for fuel cell components (e.g., membranes), they would not have an appreciable affect on energy demand. Thus, the primary benefit of the program would seem to be stimulating the demand for fuel cell components, which might reduce the cost of envisioned energy intensive applications. To the extent that the market is very large for these applications, this may be of benefit to DOE. However, if the market is small, the benefit to DOE might be negligible.
- The project is not a R&D program. The project is a system integration program. Based on the presentation, the progress of the project is very good. It is a good stationary power project.
- The project aligns with the Hydrogen Fuel Initiative and MYPP.
- Focused on specific technical barriers facing stationary fuel cells: cost, durability, power electronics, start-up time and thermal and water management.
- The project is aligned with stationary power goals.
- Project looking for early market niche with better chance of hitting viable price point. Success of this project is useful, but it is not enabling for President's vision when technology breakthroughs are required in other parts of the program.

Question 2: Approach to performing the research and development

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

- This project is not fundamental research, but rather it is a product development task.
- The system is well-designed and the technical approach is excellent.
- The approach is conventional following a traditional business model of technology and system assessment, product design, validation, and field demonstration.

- Approach could have benefited from involvement of additional industry customers at the front end of the project to help scope system requirements and provide a broader customer commitment base.
- Interesting application incorporating ultracapacitors.
- Plug Power addressed technical barriers for the development of a backup power system, including design for passing testing requirements.
- The product development has necessary steps of requirements development, technology assessment, product design and fabrication, and product certification and validation. Appropriate collaborators and potential customers are involved.

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

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

- The actual certification testing of the system is interesting, but it would have little or no application to other designs since certification testing is unique to the particular design.
- It was not clear from the presentation where the final design is projected to perform against customer requirements. Thus, it is difficult to evaluate the success of this design. Future reports should clearly identify key requirements and projected (or actual) status against those requirements.
- It appears that a great deal of testing has been or will be accomplished to ensure the design will meet requirements, which is good. Unfortunately, the results of those tests were not communicated.
- The progress for the project is excellent; the system design and integration is also excellent.
- Plug Power indicated they met fuel cell system requirements, but Plug Power did not provide comprehensive information to support this claim.
- PI did indicate impressive accomplishments with regards to design validation, reliability and safety under very hazardous conditions such as fire, wind driven rain, firearms resistance, etc. to be in compliance with NEBS and UC.
- Cost is a major unknown.
- True test will be if product meets Bellsouth field demonstration and ANL baseline requirements.
- The project has made excellent progress in developing a commercial backup power PEMFC. Certification to NEBS standards is a big accomplishment.
- The product developed will be available ($14,995 each). Comprehensive commercial certification has been completed. Initial testing is positive. Products have been delivered to independent evaluators.
- Plug Power has made great accomplishments in terms of reduction in footprint and improvements to system components.

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

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

- It might be helpful to bring data from this system stack design into comparison with modeling work done at Argonne.
- This design could provide much real data for validation of modeling done at Argonne and elsewhere.
- The project is not an R&D program; the project is a system assembly and integration program. The collaboration with BellSouth, Argonne National Lab and FAA is excellent.
- Overall, collaboration with other organizations seems appropriate in scale, conduct, and variety.
- Collaborations with FAA and Bell are excellent. Collaboration for testing is good.
- Nice mix of customers, test sites, suppliers and test/certification organizations.

Question 5: Approach to and relevance of proposed future research

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

- Although this is not research, results of deployment of this high-value product might help to guide the work being done by Battelle to assess near term transition markets.

- Future product validation plans seem sufficient.
- Find a safe and reliable hydrogen source for the 5kW PEMFC system.
- Proposed future activities are self-evident via laboratory and field testing at BellSouth and ANL.
- No contingencies nor off ramps were presented should technology fail in final testing phases.
- Future research is for outside testing/validation of results. Project is almost complete.
- Project is nearly complete. Remaining activities are appropriate to project closeout. Customer validation that the product developed meets customer requirements remains to be completed.

Strengths and weaknesses

Strengths
- This high value product could help to pave the way for future lower cost fuel cell systems. The data could be used to validate modeling done elsewhere.
- Excellent system integration work.
- Logical, business engineering approach to product development.
- Technical accomplishments appear reasonable but are somewhat hard to gauge due to lack of presented data.
- Successful development project.
- Good systems level understanding.

Weaknesses
- This is not a vehicle system and the work supports only one manufacturer.
- Where is the 99.99% hydrogen source?
- Broader collaboration with industry customers upfront would be beneficial.
- It is unclear whether unit with bottle storage will have a compelling advantage over peak-shaving/backup power alternatives. Business case was not developed in presentation.
- Lifetime of 1500hrs.

Specific recommendations and additions or deletions to the work scope

- Federal purchases of such equipment would allow better customer feedback if they were included in the program. Other federal purchases should be added, with other manufacturers and other products. The Battelle program may allow this to occur.
- Find a reliable and safe hydrogen source for the 5kW PEMFC system.
- Develop off ramps and contingencies for future work should testing results with BellSouth and ANL be unfavorable.
- Project is nearly complete; no modifications or extensions are recommended.

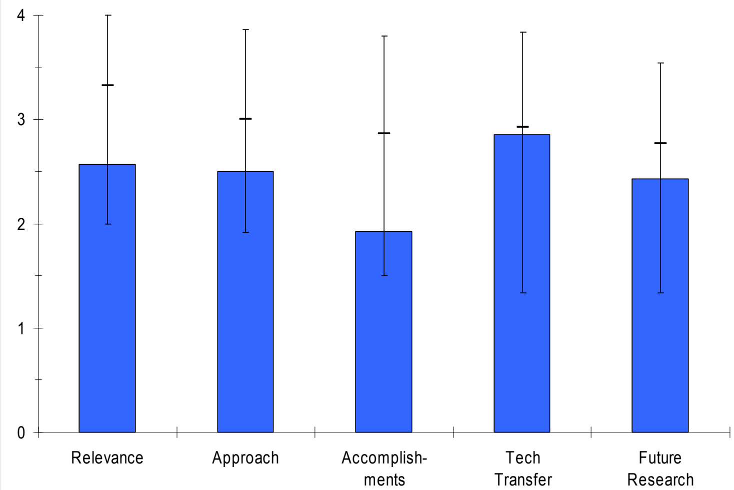
Project # FC-32: Market Opportunity Assessment for Direct Hydrogen PEM Fuel Cells in Transition Markets

Harry J. Stone; Battelle

Brief Summary of Project

Battelle Memorial Institute and its team are developing an understanding of the economic, technological, and market forces that are necessary through 2015 for commercialization of stationary polymer electrolyte membrane fuel cell (PEMFC) systems. The objectives are to evaluate potential transitional markets for direct hydrogen PEMFC applications; identify critical success factors required for commercialization; develop a technical targets table for each application (cost, reliability, size, response, emissions, electric load versus time, etc.); evaluate potential impacts of technological breakthroughs on cost and quality; and educate stakeholders and raise awareness of national programs.

Overall Project Score: 2.3 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- The identification of transition markets is best conducted by fuel cell developers and their investors. The developers are currently actively engaged in working with fork lift companies, in developing UPS systems for telecom and other applications, supplying residential systems for demonstrations in Japan, and providing stacks and systems for bus demonstrations.
- The project assumes the transitional markets will assist in the development of a transportation fuel cell application. The project has not strongly made that case. It has not shown how the transition market will develop a supplier network or the infrastructure that will promote the transportation applications.
- It is hard to understand the relevance of the transitional markets, identified to the automotive markets – so specific examples were offered.
- Success in transitional markets will be critical to success in automotive applications. Thus, identification and support of these markets is of interest to DOE. However, the markets need to be large enough to facilitate bringing down fuel cell component cost. Thus, potential market size is an important factor to consider in this analysis.
- Near term market opportunities can provide income for stack, module, and system developers to sustain a business model prior to achieving the cost reductions necessary for widespread vehicle deployments.
- Getting a handle on the possibilities that might exist in the "early adopter" community will be of key importance in enabling the beginnings of a hydrogen infrastructure via stationary projects while work continues on meeting the road-going targets.
- The information is very relevant but the timing is a bit off. By focusing on markets in 2008 the information will be published too late for it to have much value for industry.

Question 2: Approach to performing the research and development

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

- Development of products usually entails discussions of proprietary information involving the application requirements and performance benefits that will differentiate the new system from the market place.

- Focus groups are not an effective format for these discussions.
- The approach tends to repeat the approaches that were used for other non-transportation fuel cells such as the phosphoric acid fuel cell, the alkaline fuel cell and the molten carbonate fuel cell. The project has identified a discriminator to distinguish its results from previous studies.
- The approach assumes that potential users of fuel cells know enough to provide credible information – this is not borne out by the H_2IQ project (Christy Cooper).
- The approach is reasonable.
- Should consider larger scale transitional markets (e.g., Transit), which could more quickly facilitate transition to light duty applications.
- Battelle should identify large PEM markets and ignore small PEM markets.
- This project is not a research product, but is rather a market assessment.
- Diversity of market research respondents is excellent, should provide key market driver data for DOE in evaluating near-term markets.
- The approach incorporates a number of analysis types in an effort to ensure commercialization opportunities are prioritized correctly.

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

This project was rated **1.9** based on accomplishments.

- The PI did not present any information on which to judge the status of the project. A change of direction has been implemented, but the criteria for power markets can be discussed in engineering terms that then translated into metrics for identifying the cost/performance that a new technology must meet or exceed. In assessing potential markets this understanding provides metric to establish the potential for value pricing. If the PI is thinking along these lines, his presentation did not reflect this.
- Project reported that it had completed an interim report that identified backup power applications, engineering cost models, cost drivers, life cycle cost analysis of direct hydrogen system and comparison with batteries / diesel generators. These were not reported in the review. Where are the results? The review identified future work not the results of past activities. The reviewers cannot assess the contribution unless reviewers willing to discuss results.
- Not a single one of the technical accomplishments listed was specifically discussed in the subsequent presentation. No results were presented or discussed at any length.
- It is not clear how the analysis will narrow down the primary transition market to 3. It seems from the schedule communicated that this should already be done, but the 3 primary markets were not presented, so presumably they have not been selected.
- Battelle has made a good start on an important evaluation.
- More specific highlights of interim report (available soon through DOE) would have helped in evaluation of "technical" progress.
- While there may be funding issues connected to part of the problem the results and work is moving too slow for the industry to realize maximum value. One suggestion would be to focus on 2012 instead of 2008 and buses need to be considered as part of the transition and not main stream transportation.

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

This project was rated **2.9** for technology transfer and collaboration.

- By definition the project has a high degree of interaction with fuel cell developers and potential end-users.
- The project does not engage universities and laboratories.
- Battelle has produced limited publications for such a large effort. Little information was presented at the Program Review.
- There was no discussion of technology transfer or collaborations, but this measure is not really applicable to this project.
- The project is seeking input from both Federal and Private Sector customer classes. Battelle will also share its market assessment and its methodology.

- Partnerships are not formal and don't need to be; many companies have been involved in focus groups or interviews.
- There is good collaboration with a wide cross section of stakeholders.

Question 5: Approach to and relevance of proposed future research

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

- The identification of transition markets is best handled by fuel cell developers
- Project does not appear to have work plans but is responding to DOE directives.
- Future work described is reasonable but in the absence of any interim results, it is hard to say how valuable the results will be.
- Applications such as buses and scooters and fork-lift systems (and other non-car transportation systems) should be included.
- Scope was revised in February 2006 to prioritize transitional markets; fortunately the analytical market research framework makes the change relatively simple to incorporate.
- Efforts for FY07 are dependent on 2006 findings; there should be more detail and prioritization of how the potential $1.3 million left on the contract may be spent.
- Future work is mostly reactive and is guided by results and where EERE thinks things are headed. Battelle should be more directive in where the money should be spent to best accelerate or facilitate the industry penetration.

Strengths and weaknesses

Strengths
- Good effort to explore high-value near term markets.
- Very thorough analysis of potential markets via breadth of respondents involved in the project.
- Good approach to working with a broad array of stakeholders.

Weaknesses
- No results were presented even if they are internal results.
- Left out near-term high-value vehicle markets such as buses, scooters, fork-lifts, mine and construction vehicles, and similar applications.
- Future efforts that involve significant funding potential may not be specified in detail before budgets are approved.
- Not considering bus fleets as part of the transition is a mistake. They are controlled fleets and infrastructure is easier to handle.
- Study results for 2008 are too short-sighted and should be redirected to 2012. Industry has already done the 2008 analysis.

Specific recommendations and additions or deletions to the work scope

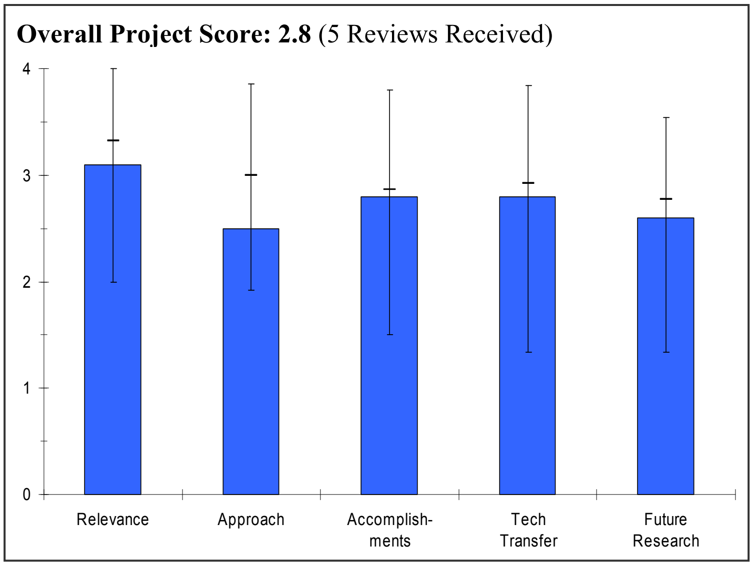
- I believe that DOE should question the allocation of valuable resources to market development activities when this is best handled by industry. The investment community and private industry has an internal reward systems that provides incentives for individuals in these companies to succeed.
- The responsible DOE / EE / HFCIT Technology Development managers should find ways to ensure that future presentations on this project are informative and meaningful.
- Add near-term vehicle markets such as buses, scooters, mine and construction vehicles, and similar applications.
- Market data on near-term transition markets should be made available as possible to industry to best leverage the government's substantial investment in this project.
- Consider adding some element of international market differences on a macro scale and only in a qualitative fashion. As an example, scooter markets are likely to take off in Asia and be a priority over there sooner than in America. Trying to get too detailed would be overly complicated but we should not ignore the international market drivers.

Project # FC-33: Scale-Up of Carbon/Carbon Bipolar Plates

David Haack; Porvair Corp.

Brief Summary of Project

Porvair Fuel Cell Technology intends to develop material and manufacturing methods leading to a low-cost carbon/carbon bipolar plate. Objectives are to evaluate and demonstrate performance within a fuel cell stack; evaluate potential cost of manufacture; develop low volume production capabilities; develop incremental, near- term cost reduction technologies; manufacture a 10 kW fuel cell sealed plate demonstration stack; develop and implement a comprehensive quality assurance plan; and develop a comprehensive cost model for high volume production.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Inexpensive and effective bipolar plates are critical for realizing RD&D objectives; it is just not evident that this project can attain the targets.
- The main factor in plate costs is increasing volume. This project is aimed at developing a high-volume process. This was a manufacturing project (prior to the manufacturing initiative).
- Bipolar plate cost reduction is quite important.
- Development of durable, lightweight, easily manufacturable, low cost bipolar plates is important enabling technology for fuel cell stacks. This project addresses these goals.
- The project is very relevant to and consistent with the President's Hydrogen Fuel Initiative goals and objectives.
- Hardware reliability/cost issues are of significant concerns.

Question 2: Approach to performing the research and development

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

- This approach primarily produces porous plates and has only a single primary customer. The technology is not yet ready for those companies that need sealed plates. The plates will undoubtedly be much too expensive to reach technical targets.
- Project focused on manufacturing of bipolar plates at high volume.
- Cost reduction: "Achieved or not" is not targeted.
- The PI is using a solid engineering approach to testing and statistical evaluation of results.
- The project is based on sound technical approach to address some technical vulnerability (H_2 permeation/sealing, hydrophobility/hydrophilicity) and cost reduction (mass-manufacturability) issues.

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

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

- Sealing progress is not sufficient as it is still a problem and there are some uncertainties with the sealing test procedure (i.e., if liquid is present as it appears, the applied pressure of 30 psig may not be high enough to

exceed the bubble point of small pinholes). In the plate forming process, there are still difficulties in achieving a full fill if the flow-field features are not within a certain size range.
- Good process control. Small variability in plate weight and dimensions demonstrated. Project has met most of the DOE targets for bipolar plates.
- Presenter did not show how the project targets have been met.
- Current status falls short of the 2010 and 2015 goals in all technical areas except one. Project is 90% complete so it is difficult to determine if further progress can be made in the time remaining or if a future project is planned. Having said this, it appears the project has made progress with the development of a material that has promise as a low cost bipolar plate, including materials development work to resolve problems with mold filling.
- Technical progress is quite significant thus far; technical milestones have been achieved in some respects.
- Manufacturing methodology, once fully developed, will greatly contribute to hardware cost-cutting benefits.

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

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

- There are interactions with UTC, but possibly only as an OEM materials supplier.
- Some coordination with UTC, including durability testing.
- Need to include additional partners in bipolar plate manufacturing, fuel cell stack development, and cost analysis.
- Collaboration is limited to one partner, but that partner is a fuel cell supplier who should be able to provide valuable feedback regarding progress and viability of the technology being developed.
- OEM partnership (e.g., UTC) is very pragmatic and technologically prudent.

Question 5: Approach to and relevance of proposed future research

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

- At this relatively late stage (90%) in a multi-year project, they are now proposing using thermoplastics instead of thermosets. Thermoplastics will introduce a whole new set of rheology (fill), shrinkage, and cycle-time issues (may be necessary to cool in the mold). Switching at this point is not likely to solve the underlying problems of cost and permeability.
- Future plans for higher throughput processing include using thermoplastic resins.
- Future plans address current shortcomings in the material development including critical needs such as finding a sealant that will withstand durability testing and improving process time to find a manufacturing process that could support high volume fuel cell production needs.
- The proposed future work is very consistent and well-coordinated with current progress. QA and FMEA initiatives are very timely and commendable.

Strengths and weaknesses

Strengths
- Have met most of the targets for bipolar plates.
- UTC as a partner will bring their long experience in bipolar plate making.
- Good engineering approach to solving the challenges of a bipolar plate done by a company that is familiar with manufacturing and working directly with a fuel cell stack manufacturer.
- OEM partnership with UTC and other fuel cell developers (not named) should be rewarding.
- Company commitment of its own resource (cost share) is BIG plus for the potential success.

Weaknesses
- Between cost and sealing issues, the sealed plates will not be capable of meeting technical targets. The porous plates may have a chance, but since they are of interest to only UTC, they would be of little benefit to the community as a whole.
- Did not show enough data on manufacturing process parameters to indicate the true potential for cost reduction.

- PI should be able to estimate and report the cost of the C/C bipolar plates at a volume level that could be agreed between DOE and the PI. While it is reasonable for the PI to state that the cost is volume dependent, this equivocation provides no information for comparison of this technology to other alternatives.
- Lack of collaboration with National Labs/university may cause some handicap in pursuing sophisticated technical measurements and understanding the fundamentals of the structural problem.

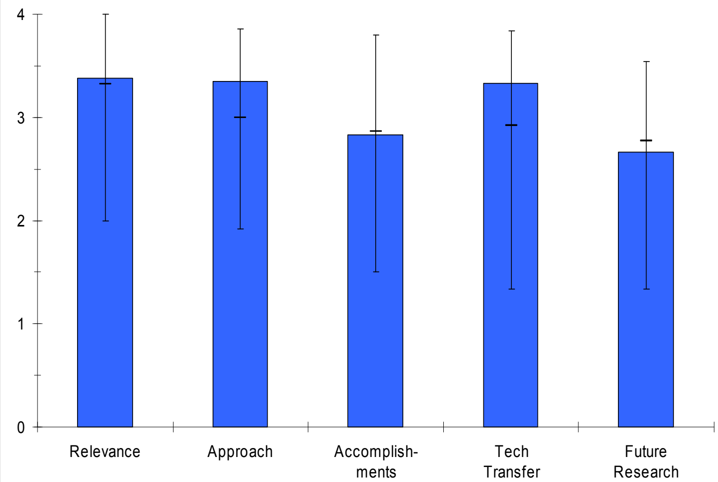
Specific recommendations and additions or deletions to the work scope

- The project is near completion, but it is strongly recommended that it is not extended in any capacity.
- Additional collaborative partners may help develop the concept to its full potential.
- Materials level durability, compatibility, degradation and poisoning possibility – these issues should be investigated early on (either in-house or with some collaborative partner).

Project # FC-34: Cost-Effective Surface Modification For Metallic Bipolar Plates

Peter Tortorelli; ORNL

Overall Project Score: 3.1 (6 Reviews Received)



Brief Summary of Project

Oak Ridge National Laboratory (ORNL) is developing nitridation surface treatments to protect metallic bipolar plates. The work is focused on formation of dense Cr-nitride surfaces on stainless steel alloys. The overall objective of this project is to demonstrate the potential for metallic bipolar plates to meet the 5000 h durability goals at a cost of <$6/kW.

Question 1: Relevance to overall DOE objectives

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

- The project is relevant to the goals and objectives of DOE.
- Metallic bipolar plates are an alternative to carbon composite bipolar plates and many automobile companies prefer metallic bipolar plates over carbon composite plates.
- Project is important to the success of the meeting the cost objectives of the program.
- Has good potential. Cost effectiveness is questionable. Could produce good durability if successful.
- Cost effective and durable/reliable bipolar plates are critical components for a fuel cell stack that is to be viable for transportation applications. Metallic plates offer the ability to be mass produced for low cost.
- Clearly, developing low-cost and stable bipolar plates is a critical component to meet the automotive goals and is a significant technical challenge. And, this addresses the key issue with metallic plates, namely, developing corrosion-resistant and low-contact-resistant materials with the potential for low-cost production. Nevertheless, it should be ensured that DOE also continue to support low-cost carbon-based bipolar plates, since it is not obvious (yet) which approach will ultimately be preferable.
- Lower cost bipolar plates are highly desirable.

Question 2: Approach to performing the research and development

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

- This approach is quite unique.
- This non-coating method could be the best solution to the surface defects caused by coatings.
- Approach is solid; significant emphasis on fundamental knowledge.
- Advancements have been made but on materials which may not be useful (Ni).
- Coating concepts and metal plates are important; more time should have been dedicated to the Fe system.
- Good attempts. Internal thermal stresses are a concern. Ni detected in MEA raises the question on the full blanketing effect of the technique.
- Project is well designed concentrating on low cost metallic plates with various coatings being evaluated for their physical properties and durability. Solid scientific and engineering approaches appear to be used to evaluate the results of testing and to develop alternatives when results are less than anticipated.
- Surface-conversion approach is good vs. coating.
- The proposed surface modification has an excellent potential to yield a low cost bipolar plate.

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

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

- Oxygen reduction problem could be very important for this technology.
- Internal nitridation is a key problem that needed to be resolved.
- There is no comparative data on coated metal plate conductivity with the conductivities of available commercial fuel cell plates.
- Progress has been good but the Fe system needs further development and evaluation.
- It is still not clear that the cost goals will be met based on the Fe system.
- Internal nitridation in the alloy can cause embrittlement. However, Vanadium addition improves the Cr- Nitride protective surface.
- When technical setbacks occurred, the PIs developed different coatings that can meet the necessary physical properties indicating improved durability. While progress has been a bit slower than could be wished, it appears that the PI has identified a combination of plate materials and alloys that are moving in the right direction of meeting the program's goals.
- Success with Fe-based materials definitely needs to be demonstrated to meet cost requirements. Good progress here, but PI still needs to show some in-stack durability of Fe-based materials.
- Use of Vanadium in low cost bipolar plates appears to be quite promising.

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

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

- Good collaboration team is in place.
- Not much collaborative work has been reported in this presentation.
- Appropriate level of exchange is in place.
- Project is collaborating with other National Labs, but more importantly with industry experts in the design and end use application of fuel cells.
- Excellent. Working with both end users (e.g., GM) and bipolar-plate manufacturer (e.g., GenCell).
- The stakeholders included have good fuel cell experience and commercialization capability.

Question 5: Approach to and relevance of proposed future research

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

- Significant amount of work has to be done in next few months as the project ends in Sept., 2006.
- The proposed work is important, but it may not be achievable due to the time constraint.
- Would continue to support additional coating research (Fe-based systems).
- Scale-up may be too premature.
- Good planning with good Go/No-Go decision.
- Future plans appear reasonable assuming that the project successfully clears the Go/No-Go checkpoint.
- Good plan, but can it all be completed by Sept. 2006? The Go/No-Go gate is a good idea. DOE should ensure the latest results obtained in this project be included in the determination if additional funding is granted in the latest solicitation.
- Long term cell and stack testing in needed to validate the technology benefits.

Strengths and weaknesses

Strengths

- Good approach. The surface defects, which are typically caused during the coating process, don't happen in this case.
- ORNL's very strong materials emphasis.
- Good concept. Could lead to good results.

- Good, solid engineering/scientific approach to the challenges at hand.
- Technical approach and industry collaborators.
- The protective nitride larger concept is very appealing. Analytical approach is very effective. Involving a manufacturer partner such as Dana helps to focus on manufacturing issues.

Weaknesses

- The technique is very new and there is not much literature precedence of this method.
- Very focused into few types of alloys. May need to do broader exploration of alloys that are compatible with the present coating procedure.
- Lack of cost effectiveness. Internal thermal stresses and deformation during the nitridation process.
- But a bit slow reaching the point where they are now. The costs reported do not include the cost of nitriding which does not appear to be fully understood. Until the total cost of the plates with nitriding can be determined and included in the total cost, it may be premature to declare that the FreedomCAR cost goals can be met.
- Potential environmental or recycling issues associated with Cr and/or nitrides? Will these Cr-based metals become a "material of concern"? Lack of long-term stability (e.g., 5,000 hrs) testing in stacks or in appropriate environments (e.g., H_2 and air in the presence of water/acid).
- Pre-test and post-test characterization of the coating after a long term cell test (>2000-5000 h) was not shown.

Specific recommendations and additions or deletions to the work scope

- Due to the time limitations, work should be focused onto the validation of the proof of concept of this coating technique to low-cost alloy material.
- Develop techniques and minimize the effect of thermal stresses and mismatch in thermal expansion coefficients between the Ni-Cr layer and the bulk of the base alloy. Consider more accurate cost evaluation methods and cost effectiveness.
- Recommend that the PI find a way to understand and estimate the total costs.
- Focus on testing the bipolar plates in long term cell and stack to further validate the potential benefits.

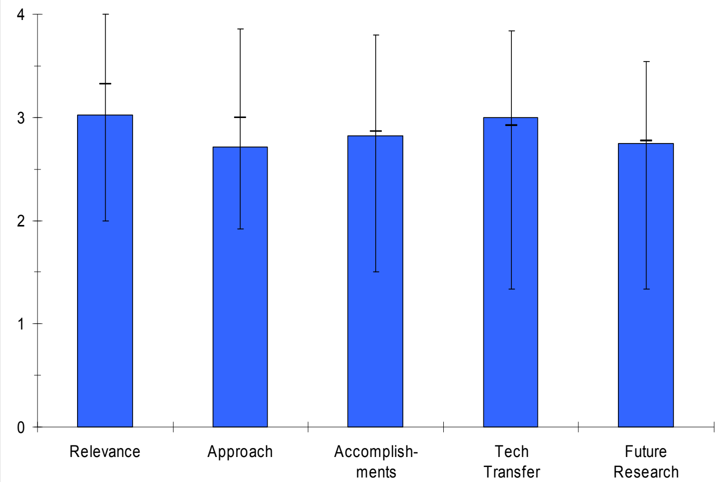
Project # FC-35: Platinum Recycling Technology Development

Stephen Grot; Ion Power, Inc.

Brief Summary of Project

This Ion Power, Inc. project will assist the DOE in demonstrating a cost effective and environmentally friendly platinum group metal (PGM) recovery and re-use technology for PGM-containing materials used in fuel cell systems. The objectives are aimed at recovering not only the PGM materials but also the membrane material in a usable form. Specific developments include processes for solubilizing catalyst-coated membranes, processes for the separation of catalyst and ionomer materials, test methods to determine vitality of the recovered materials, and partnering with the key stakeholders in this technology area.

Overall Project Score: 2.8 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Long term, recycling of Pt from stacks will be critical to Pt supply.
- Platinum recycling will be very important in maintaining a viable supply of fuel cell catalyst precious metals required for full scale fuel cell commercialization.
- Recycling of platinum is critical to achieving the hydrogen vision, and directly addresses the RD&D plan objectives.
- Pt recycling is important to life cycle cost of fuel cell development.
- Ion Power has a realistic approach to recycling platinum and Nafion, which will be critical to the cost and environmental friendliness of fuel cells.
- Membrane recycling is also important however the relevance of specific research tasks to overall objectives is difficult to determine.
- The project is highly relevant to DOE's Hydrogen Program for recycling Pt from MEAs.

Question 2: Approach to performing the research and development

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

- Approach builds on Principal Investigator's knowledge of materials.
- Inclusion of impurity identification is important to materials processing.
- DMFC membranes will not represent large volume of material in the future, and uncertain why they are included.
- Rather than investing in fuel cell materials performance characterization, additional investment might be made in better understanding the recycling processes.
- More effort needs to be applied to the core of the program: recovering precious metals, ionomer and then demonstrating their vitality.
- *In situ* membrane purification is interesting, but it is unclear if it is proposed for commercialization – very sound approach for Pt recycling.
- A convincing case for the value of recycling the membrane polymer.

- Unclear number of hours of service for stacks. Aging mechanisms and recycling challenges may differ depending on the hours of service. No mechanical characterization of the recovered Nafion, which is important to ensure that it is reusable. The real recycling challenge will be stacks from vehicle usage, based on future volumes. Ultimate durability targets of 5500 hrs means that current stacks used in this project may not be realistic.
- Good understanding of fundamental issues causing chemical degradation of Nafion membranes, as applied to recycling concepts. Very good analytical chemistry. Innovative way to restore Nafion. Good economic analysis.

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

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

- Results focused on analytical methods and fuel cell characterization of materials rather than recycling processes.
- Not clear on progress related to recycling process.
- Energy analysis shown but focus on cost. Cost analysis of the project appears incomplete.
- Interesting diagnostics and analyses of end of life membrane materials that might prove useful to other DOE fuel cell durability programs.
- The project has achieved success in clearly demonstrating the viability of platinum recovery.
- Good progress in recovery of membrane ionomers.
- The "cost" model of process energy analysis seems inadequate. Environmental costs of solvents and other waste streams may not be captured. Although a postive business case is presented, the current analysis should be regarded as an estimate.
- Performance test did not seem compatible with main project objectives.
- Unclear as to how much of the platinum as a percentage was recovered by the extra techniques used on the membrane.
- Based on the presentation, some solvents were used to separate catalysts and membrane electrolyte. Unclear if the recycling membranes still have the same prosperity as fresh membranes.

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

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

- Industrially focused project. Some involvement of university to characterize materials and fuel cell performance might free up PI to focus on recycling process.
- Partner with a catalyst company to evaluate and demonstrate the viability of the recovered platinum for remanufactured electrodes.
- Very good mix of collaborators, including major MEA suppliers and stack developers.
- Good interactions with suppliers but need to get hours of use on the stacks.
- Not much evidence of effective transfer yet.
- Working with a lot of OEMs.
- Good collaboration.
- The collaboration with partners is good.

Question 5: Approach to and relevance of proposed future research

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

- Mechanical processing of membranes not discussed thoroughly; many options exist for shredding MEAs that can take fair amount of resources.
- PI did not discuss how they would minimize materials that do not contain Pt.
- Economic analysis should use future target component costs as large scale fuel cell commercialization would not occur at today's prices.

- Future plans to focus on scale up processing and to further refine the cost models are on target.
- Careful economic analysis is vital to ensure that the recycling methods developed are the low-cost choices.
- Seems appropriate especially including other membrane materials and test of a re-manufactured stack.
- Future plans are unclear.
- Branching out to other membrane manufacturers beyond Nafion is a good next step.
- Based on the presentation, the future research approaches are fair. Recycling process steps should be better described.

Strengths and weaknesses

Strengths
- Interactions with stack suppliers.
- Good combination of synthetic, analytical, and fuel cell chemistry combined with working on practical problems.
- Detailed analysis work accomplished on the different membranes.
- Showed the value of the ionomer as being potentially as valuable as the platinum.
- Investigators have great deal of expertise in understanding Nafion's chemical and physical properties.

Weaknesses
- Value proposition seems to be tied to the assumption that Nafion would remain at current cost even if mass produced.
- A small weakness is that they are focusing on Pt and Nafion, and not fully addressing alloys (PtCo) and hydrocarbon membranes (note that future plans include seeking out manufacturers of non-Nafion membranes, so this may be fixed).
- Presentation could be better organized. The procedure for recycling Pt from MEA should be the focus.

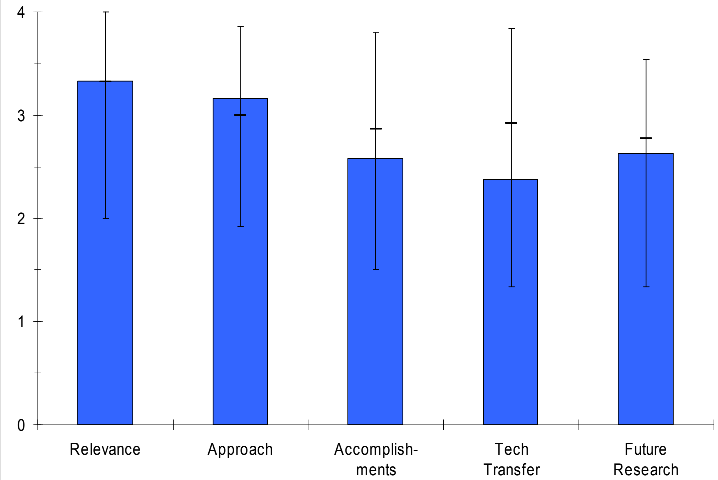
Specific recommendations and additions or deletions to the work scope

- Provide more explanation on how the recovered Nafion (or other material) would be recycled and at what cost.
- Quantifying the specific life of each membrane in hours to note the differences in chemical properties and contaminants over a time scale and for different applications would help to understand changes over time.

Project # FC-36: Platinum Group Metal Recycling Technology Development

Larry Shore; Engelhard

Overall Project Score: 2.8 (6 Reviews Received)



Brief Summary of Project

This Engelhard Corp. project is examining methods to recycle all precious metal-containing catalysts in a fuel cell "system." A primary objective is to develop a commercially-acceptable, environmentally friendly process for recovering and recycling Pt and Ru from membrane electrode assemblies (MEAs) by developing a process that does not emit pollutants (especially HF) and evaluating Ru recovery from MEAs. A process for PM recovery from metal monoliths, which are used in stationary reformers and potentially in hydrogen production, is also being evaluated.

Question 1: Relevance to overall DOE objectives

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

- Working on an approach to recycle Nafion and Platinum with concern for environmental impact.
- The project is highly relevant to the DOE's Hydrogen Program.
- One of the critical elements in long-term cost of Pt with large volume production of PEMFCs is the cost of recycling the Pt. Critical to understand the chemistry and byproducts (and environmental impacts) not just the yields.
- Platinum recycling is critical to future markets.

Question 2: Approach to performing the research and development

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

- How the individual tasks fit into an overall manufacturing process was not clearly defined.
- Initial economic analysis not apparent to determine if the selected approaches had the potential to be cost effective.
- Unclear who process economics were on 100 kg of MEA. Unclear how many stacks the MEA units represent.
- Innovation not apparent.
- Based on the presentation information, the technical approaches are excellent.
- Approach is not very well defined at least in the presentation. A lot detail is given about some aspects of alternative paths, but a clear picture of the pluses and minuses of alternatives was not presented.
- Solid path to environmentally friendly process for recovery of PGM.

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

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

- Unclear which approach was dropped and whether the microwave assisted process and the mechanical preparation of the samples would handle existing and contemplated MEA configurations.
- An overview of the future process and how these developments would fit in would have helped to provide a sense of progress and what challenges remain.

- Some progress but appears to be behind schedule based on money and time spent.
- The technical accomplishments are excellent.
- Status is unclear.

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

This project was rated **2.4** for technology transfer and collaboration.

- Industrially focused project. Some involvement of university to develop adsorbent.
- Working with OEMs, but not providing public information.
- Should increase collaboration with MEA production companies (such as DuPont) and PEMFC manufacturing companies.
- Unclear as to exact roll of collaborators outside of MEA donors and one testing company.

Question 5: Approach to and relevance of proposed future research

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

- Mechanical processing of membranes not discussed thoroughly, many options exist for shredding MEAs and can take fair amount of resources.
- Lack discussion of how to minimize materials that do not contain Pt to increase concentrations.
- Some future work seems to be work that should have been done by now, or is not likely to be completed by dates indicated.
- It seems premature to be certifying recovery processes before MEAs are fully commercialized; the certification would have to be redone if the MEAs change. It is not clear if there are enough fuel cells made now to make certification important.
- The future research plans are excellent.
- Very well planned future work with Go/No-Go criteria established.

Strengths and weaknesses

Strengths
- Leveraging commercial processes.
- The technical approaches are well-organized and stated in the presentation file.
- Domestic producer and refiner of Pt group metals is the prime.
- Good progress on a process to recover a high percentage of PGM.

Weaknesses
- Progress seems slow.
- Innovation unclear.
- Increase collaboration activities to possibly use some reclaimed materials in new MEAs and do some preliminary testing and comparisons to new MEAs.

Specific recommendations and additions or deletions to the work scope

- The economic value of the MEA will provide the industry the incentives to optimize recovery technologies
- Cost analysis needs to be presented (one presumes it is in the Project Scope).

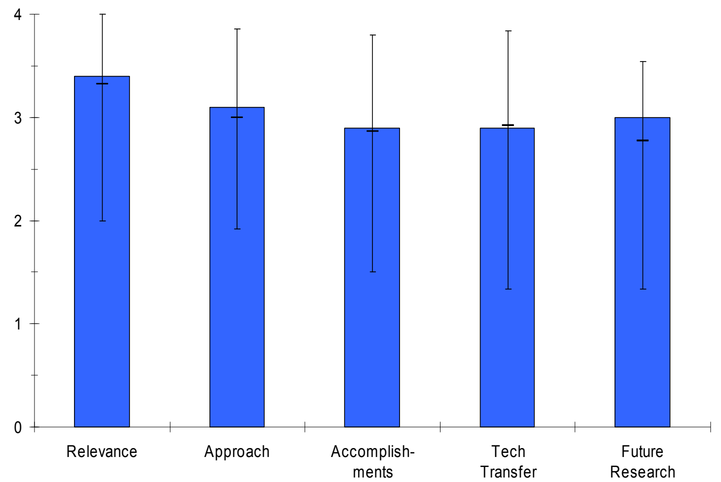
Project # FCP-08: High Temperature Membrane

John Kerr; LBNL

Brief Summary of Project

Lawrence Berkeley National Laboratory (LBNL) is investigating the feasibility of use of solid polyelectrolytes for high temperature operation that do not require the presence of water for proton conduction. Nafion and polyether polyelectrolytes doped with imidazoles are being prepared and their conductivities and mechanical/thermal properties measured. Imidazoles are covalently attached to side chains of ionomers with appropriate polymer backbones. Simplification of the overall fuel cell system is particularly important for transportation and mobile applications.

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- High temperature, non-water dependent membranes correlate well with the DOE objectives.
- Materials work to reduce fuel cell manufacturing costs is important.
- Project well-aligned with commercial issues, i.e. membrane operation with low/reduced water content (high temperature and low temperature membrane operation).
- Membranes for operation under dry conditions are critical for the commercialization of fuel cells in vehicles.

Question 2: Approach to performing the research and development

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

- PI is aware of past work using imidazole-type materials for fuel cells and understands their shortcomings. Membrane fabrication was well thought out using information from the literature.
- The approach seems to focus primarily on durability.
- Good concept and prototype design for evaluating the proposed polymer modification.
- Need to expand the scope to include activities that address/resolve likely chemical durability issues with the prototype. For example, additional review/experiments with fluorinated analogs as proposed by the PI would be appropriate for the next stage of this project.
- This is a totally novel approach, one of the few that are truly out of the box and although it is high risk, a clear path exists to solving the problem. This is outstanding research by a very competent researcher.

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

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

- Membrane characterization is very high showing high proton conduction and good mechanical properties. However, the PI is aware of the major shortcoming of this material (imidazole poisons Pt surface) but has not addressed this issue.
- While a good deal of work has been done, it is difficult to clearly see progress towards DOE goals.
- Project interruption for 1.5 years has slowed originally planned progress.
- The PI validated the concept design and documented the prototype demonstration very well.

- Need to solicit external resources (polymer manufacturers, fabricators, etc.) to identify and prioritize potential barriers to commercialization, ways to reformulate the concept with commercial materials, etc., and address those issues in the next stage of the project.
- The ionomer architecture has been worked out with few resources. A truly impressive accomplishment.

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

This project was rated **2.9** for technology transfer and collaboration.

- Collaboration is with NASA and "High Temperature Membrane Working Group." Level of interaction is very low.
- Collaboration appears limited.
- Project has excellent interaction with the other National Labs, utilizing needed expertise and analysis capabilities.
- PI indicated the need to interact with polymer manufacturers as a next step to address material selection and support demonstration activities. This will be important for continued development of this concept.
- Good, more collaborators than listed on slides.

Question 5: Approach to and relevance of proposed future research

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

- Preparation and testing of MEAs is critical to assess the real value of this research – it should be a milestone with a metric, both for performance over the relevant temperature range, and for durability against some of the known failure modes of PEM.
- Membrane work has shown promising results; however, until mitigation strategies to prevent Pt poisoning are clearly stated and tried, this project is simply an intellectual exercise.
- Future research should more clearly address barriers.
- Project well-aligned with other National Lab activities.
- Excellent. A clear path towards success exists.

Strengths and weaknesses

Strengths
- Good fundamental research into non-aqueous ion transport mechanisms.
- Imidazole-type membrane materials have been clearly shown by the PI and in previous literature that proton conduction at high temperatures without water is at an acceptable level. This type of material has the potential to be the "Holy Grail" for PEMFCs.
- This project has "synergy" with other projects (e.g., FC-25). The "concept" is an "engineering solution" which has merit and potential.
- Novel out of the box approach, concept totally validated.

Weaknesses
- The *ex situ* approach to developing these materials should be augmented by *in situ* evaluation of promising technologies. To delay *in situ* work until the latter part of the project incurs unreasonable risk.
- Without validation that these type of materials can be designed in a way to prevent Pt poisoning in a fuel cell, it is unclear what new concept the PI is bringing to the table. Conjecture and hand waving is not acceptable.
- Need to address potential "de-railers" as soon as possible.
- Resolve possible chemical stability of imidazoles and backbone polymer as currently defined by the initial concept.
- Identify partners (other resources) to evaluate the prototype concept versus commercial materials and manufacturing routes.
- Not enough resources for project.

Specific recommendations and additions or deletions to the work scope

- Add a milestone for *in situ* evaluation of candidate membranes, with metrics for conductivity over a temperature range and sensitivity to known failure modes of PEMs.
- Funding for this project as stated by the PI is low and MEA work is not currently being investigated. For this to be a worthwhile program, the DOE has to either increase funding to allow the PI to pursue MEA work (validate concepts to mitigate Pt poisoning) or kill the project. Doing it halfway under the current conditions will result in useless findings (imidazole is known to work already).
- Keep project focused on addressing barriers and goals.
- Consider collaborating with an industry partner.
- This project should be combined with other National Lab programs – to better expedite the development and provide more focused activities with industry partners for commercialization of the concept. Need to create a project with "critical mass", including better utilization of resources (people and funding).

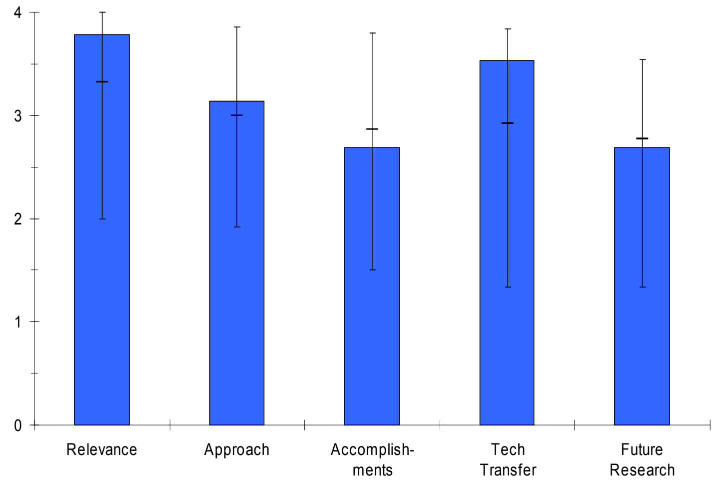
Project # FCP-09: Component Benchmarking

Tommy Rockward; LANL

Brief Summary of Project

Los Alamos National Laboratory (LANL), in close collaboration with members of the USFCC from industry, universities and other government entities, has helped advance a collective effort to provide the polymer electrolyte membrane fuel cell industry with a standard test protocol defining a consistent, repeatable method for conducting a single cell test and generating a polarization curve. These efforts are intended to provide a comparison benchmark.

Overall Project Score: 3.1 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Establishing standard test protocols is key to the adoption of technology. In this case, it is a useful exercise, but it is not clear if this particular test protocol is one that will be widely adopted.
- This project appears to be a "coordinating"/oversight program for all the other component analysis and characterization efforts.
- As such, this effort is clearly important and relevant to DOE's plan objectives.
- This is work that needs to be done; it is not flashy but enabling if fuel cells are to be widespread.
- The project addresses the important need for developing and qualifying test protocols that will allow valid comparison of fuel cell data.
- The project does not directly address the key barriers (cost, durability, electrode performance), but will indirectly foster development work in these areas by providing testing guidelines.
- The project will focus and streamline the development efforts of component manufacturers as they will not get sidetracked by issues related to MEA assembly (i.e. sealing, GDL bonding).
- This work will be useful in achieving the DOE goals by allowing more uniform comparison of fuel cell sub-components.

Question 2: Approach to performing the research and development

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

- It would be helpful to establish ways to modify the protocol as needed for different materials.
- The approach is vital to sharing and coordinating all the related DOE programs on component analyses. However, the simplification of certain procedures (e.g., single cell tests, gross crossover leaks, simplified conditioning, etc.) is disturbing in that more complicated problems may be missed. For example, LANL's MEA fabrication approach may work for Nafion-like membrane systems; but it would be inappropriate for non-perfluorinated systems and hence, an "apples-to-apples" comparison between different membrane families would not be possible.
- Straightforward approach to benchmarking. This is almost a National Bureau of Standards approach for fuel cells.
- The barriers (and resolutions) regarding the single cell test protocol need to be clearly identified and presented with more specific details – at least the "critical few".

- The rational behind specific test procedures should be explained more precisely (e.g., break-in period) and also the way in which a specific test protocol does affect or stress the individual MEA component.
- Guidelines on the data processing, e.g. extracting internal resistances from IV-curves, calculating Tafel slopes) and on the assessment of the MEA components should be given, too.
- For components (membrane, catalyst, or GDL) evaluation, consider developing MEA fabrication capability and prepare baseline materials.
- Develop electrochemical analysis, e.g. EIS, CV etc.
- The PI proposes a reasonable way to address a difficult question of identifying testing protocols for benchmarking fuel cell sub-components which will be relevant to numerous users.

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

This project was rated **2.7** based on accomplishments.

- The team has made good progress towards the second round of testing. Still waiting for publication of the results.
- Technical accomplishments and progress in this project are hard to gauge given the "coordinating" nature of this program; but the poster and researcher did not make clear the compromises and adjustments needed to deliver on the program.
- The testing protocols for break-in, etc. are overly simplified.
- A reasonable level of activity shown – the PIs could be more aggressive and have more impact.
- The technical accomplishments were difficult to evaluate, since many of the project's collaboration activities remain "confidential."
- While the program with the USFCC (single cell test protocol) was not confidential and LANL's valuable contributions are generally known, the presentation did not provide adequate specifics regarding the accomplishments.
- It was clear that the "timing" (and completion) of activities is not totally under the project's control – and the most likely the reason for slow progress.
- The achievements are highlighted in a very general manner. A summarized feedback from the partners/collaborators on the test protocols should have been included.
- The PI has made progress in developing good testing protocols.

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

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

- Good interactions with USFCC and academic/industrial partners.
- This effort is *de facto* collaborative; so despite the difficulties, the efforts are well-done.
- Good TT/Collaborations but more could be done.
- Extensive collaboration and support for a variety of groups and companies.
- This project involves a variety of fuel cell partners with obviously close interaction.
- More industrial partners would certainly increase the value and the acceptance of the project. It is, however, recognized that industrial support for standard tests is often difficult to obtain.
- Repeatability and reproducibility of testing stations should be evaluated.

Question 5: Approach to and relevance of proposed future research

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

- Stronger leadership for the establishment of standardized testing is needed. This is only one part of fuel cell performance/durability, materials characterization, and component testing.
- More details explaining specific coordinating programs are definitely needed, e.g., procedures which clarify the performance of membrane materials versus electrode formulations, etc.
- This was a bit weak. The project appears to be passive. More aggressive participation recommended.

- Project is generally providing a "service" to others, and future activity was not well defined – since it seemed to be very dependent on the "immediate" needs of the listed collaborators/partners.
- Future plans only briefly described, appear sensible.
- Emphasize accelerated/selective durability test methods to save testing time and help identifying the "weakest link" in the MEA.
- Developing a durability protocol that will be relevant and accepted broadly by fuel cell researchers in many application areas will be difficult.

Strengths and weaknesses

Strengths
- LANL has leveraged its position as a leader in fuel cell testing to bring about this collaborative effort.
- A good first start at data-gathering and establishing "uniform" procedures for basic characterizations and collaborations.
- Important activity, high capabilities, impartial lab.
- The PI is able to utilize many LANL resources, ranging from fuel cell hardware and instrumentation, to sample preparation, evaluation protocols, and personnel needed to define protocols and evaluate the results. These resources were presented in good detail – and the listing (and discussion during the presentation) reinforced their value to the fuel cell industry and others (universities, organizations, etc.).
- The project clearly addresses a key issue!
- Many partners involved, crucial for widespread acceptance.
- Provides guidelines to inexperienced fuel cell developers and allows quick positioning against state-of-the-art technology.
- MEA testing capability.
- Many collaborations with universities, National Labs, and industry.

Weaknesses
- This establishes a testing protocol for one particular set of conditions. More work is needed to establish standardized measurement techniques across the full spectrum of fuel cell technologies.
- More details re: future standardizations would help the presentation.
- Needs to more aggressively look for business.
- The PI needs to provide more specific comments on: sources of "site-to-site" error when using the test protocol; protocol remedies to reduce errors, etc.; and protocol and/or hardware limitations.
- These accomplishments were "inferred", but there were no specifics shown in the presentation.
- Guidelines on the processing and interpretation of the obtained data should be given.
- The project lacks some additional analytical standard testing methods (CV, EIS).
- There should be a plan for releasing a proposed testing protocol for public evaluation and input.

Specific recommendations and additions or deletions to the work scope

- It is not clear that DOE funds should be used for a larger effort, but stronger leadership for standardized testing protocols is needed.
- This project needs more funding and a further in-depth series of more uniform characterization programs.
- Increase overall energy within project and keep going!
- It could turn out useful to split the test protocol up into mandatory tests and those which can be done in addition.
- Sensitivity test (temperature, relative humidity, pressure, etc.) covering a wide operating condition range could be included (possibly through a design of experiment approach). System developers could largely profit from this.

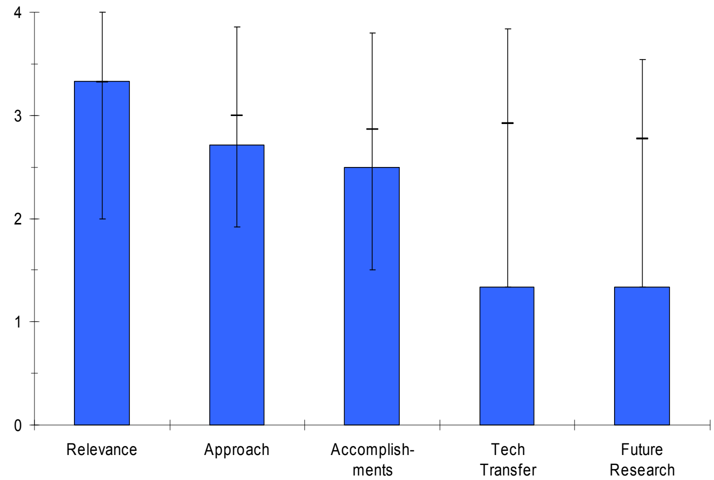
Project # FCP-13: Montana PEM Membrane Degradation Study

Lee Spangler; Montana State

Brief Summary of Project

Montana State University's overall objective is to determine membrane degradation mechanisms and how to prevent or mitigate them. Specific goals are to determine changes in membrane material properties as degradation occurs, determine if any electrical properties can act as a signature of developing degradation, and investigate the potential of advanced control systems to prevent degradation problems.

Overall Project Score: 2.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The relevance to DOE objectives is clearly stated and appropriate.
- However, the focus appears to be one emphasizing development of analytical tools for membrane analysis over truly understanding fundamental degradation mechanisms.
- The project sought to address membrane durability, which is one of the subject areas most in need of detailed attention.
- Parts of the work were probably seeing changes in the electrode layers which may have been misinterpreted as changes in the membrane (see below).
- The ostensible reason for the research is sound: the performance of fuel cells degrade over time and analysis of why can lead to longer lasting components. However, relevance is limited by experimental design flaws detailed in (2).
- Understanding of membrane degradation mechanisms is critical. Focus seems to be on DMFCs.
- The approach of database generation in real time describing fuel cell performance is very good. Correlation with durability is essential. Interesting tools proposed for characterization.
- *In situ* membrane degradation studies are an important topic for the Presidents H_2 Initiative.

Question 2: Approach to performing the research and development

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

- The technical methods and analyses are well presented and discussed.
- The emphasis, however, was more on analytical methods and tools, systems, and equipment; so the relevance of such an approach to fundamental understanding of membrane degradation mechanisms are a stretch, e.g., the linear resistive model is impressive in the equipment development to get at it; but what do voltage decrease and resistance increase tell you about where in the MEA the problems are?
- Local densification and variations have been uncovered by other researchers, without recourse to the X-ray analyses and the NMR studies.
- The project was intended to address the mechanisms of membrane degradation, but the in the electrical analysis the whole MEA was treated like a black box, not taking advantage of preexisting knowledge of how an MEA works.
- The linear resistance model used was overly simplistic. It is well known that the properties of an MEA are best modeled by a combination of kinetics, ohmic losses, and mass transport.

- The X-ray work was interpreted in terms of changes in the membrane before and after it was used, no? If so, wouldn't the high-Z Pt dominate the X-ray scattering, so that the observed changes would most likely be in the electrode layer rather than the membrane?
- The experimental design did not fully address the objective: "The overall objective is to determine membrane degradation mechanisms and how to prevent or mitigate them." The experiment reported a physical degradation mechanism (localized membrane densification), but no chemical degradation mechanisms were addressed (it is not clear how X-ray imaging was supposed to detect changes to the chemical structure of the membrane). Preventing degradation (an objective) involves not just load regulation but chemical engineering of the membrane. Objective "Investigate the potential of advanced control systems to prevent degradation problems" not addressed by experiment. Parallel/Serial interconnection method was acknowledged as poor experimental design: "The degradation of the cartridge modifies the extent to which the cartridge participates in the system and changes the excitation." "The amount and complexity of the data has been a challenge. Tens of thousands of data files per membrane exist." This was a problem of the PI's own making ... for what reason was all this data taken? What was the experimental plan and what were the results? Most significantly, what were the independent/dependent variables or the correlations between the measurables that this project was intended to study? After quantifying electrical behavior of cell with two parameters (Slide 12 – $\mu 1$ and $\mu 2$, being Thevenin voltage and Thevenin resistance), changes to these measureables were not reported as functions of anything, or even correlated with density variation data (Slide 28) or other measureables, such as changes in cell response time (e.g., Slides 11 and 23). Slide 25 hypothesizes a cause/effect (change in a time constant as function of "hydration/drying cycles"), but what excluded other possible causes such as: total operating hours, load, load oscillations/duty cycle, etc. (the presentation implies that the load is a cause, but provides no data)? Slide 38 shows "T2 maps of Nafion 117 Heterogeneity," but such data not provided as a function of other measureables; how does T2 change after membrane has been used in a cell with a specific history?
- *In situ* diagnostics are being used to elucidate degradation mechanisms. Attempting to correlate failure modes to fuel cell materials/physical aspects.
- A massive amount of data was obtained; PIs overwhelmed by these data and did not fully make use of all of it; characterization techniques are fine.
- The *in situ* SAXS experiments outstanding. However it is not clear how the NMR imaging fits in and why the use of methanol would be relevant to an hydrogen fuel cell.

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

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

- The technical approaches are an overkill for the conclusions arrived at.
- However, the methods development and the analytical techniques were well-discussed and the conclusions reached are on the mark; but the more detailed challenges re: membranes versus electrode layer problems were not resolved.
- There is clearly progress in the analytical developments.
- Use of a more sophisticated model would have increased the value derived from all of the detailed electrical measurements.
- If the X-ray measurements actually measure properties only of the membrane rather than of the entire MEA, then the way this was done should have been better clarified on the poster.
- It would be good to clarify why the T2 maps of the cutouts show variability in the Nafion 117 rather than possible problems with reproducibility of the results. Were similar images taken from a material known to be uniform and showed no such variability?
- Can the authors provide any physical interpretation of the two time constants?
- Work resulted in hypothesis: "Improved density uniformity may extend PEM fuel cell performance." However, this hypothesis was not tested.
- Some correlations have been established, e.g.., membrane density uniformity and cell performance.
- Modest progress; densification during use is really the only conclusion after a great deal of work.
- *In situ* SAXS measurements are a significant accomplishment.

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

This project was rated **1.3** for technology transfer and collaboration.

- There is NO clear evidence of external collaborations; nor did I see cross-references to the degradation studies conducted by other DOE program participants, such as 3M or DuPont.
- The project would have benefited from detailed discussions with people more experienced with fuel cells (e.g., in AC impedance of fuel cells) at its early stages.
- There was little or no evidence of collaborative work between the university and fuel-cell companies or DOE labs.
- No collaborations were reported although a collaboration was said to be imminent.
- Given the problems in data handling, they could have brought in experts. Additional help in characterization could have been imported.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.3** for proposed future work.

- Development of the electrophoretic magnetic resonance probe could prove to be useful.
- No future work was presented.
- Disappointing. Proposed is data mining that should have been included in the current project.

Strengths and weaknesses

Strengths
- The equipment and methods developments are well-executed and explained.
- Instrumentation.
- This is a university-based project that exposed students to fuel-cell engineering issues. The project provided student experience in the mechanics of laboratory instrumentation (e.g., labview).
- The approach – online, massive databank, correlation of membrane properties with cell performance is solid. Good test configuration established.
- *In situ* SAXS.

Weaknesses
- The sophistication of the equipment and analytical techniques employed has led to fairly pedestrian conclusions re: densification and membrane inhomegeneity.
- There is no evidence of further in-depth analysis of membrane degradation beyond phenomenological explanations. Also, future plans are not articulated in the poster presentation.
- Insufficient incorporation of knowledge from the established literature as an aid to data analysis and interpretation.
- The experimental design was not responsive to the stated objectives of the project. This four-year project is concluding with little or no useful information gained that would aid in commercialization of fuel cells.
- Should establish collaborations with others doing similar diagnostic work.
- Relatively little delivered in this project. All one has is that cells densify with declining performance. This probably would have been determined with a single test. Not much further upside suggested.
- Need NMR imaging and PFGSE of water swelled and dry membrane before and after durability testing. No collaboration or technology transfer other than by publications.

Specific recommendations and additions or deletions to the work scope

- If there is a follow-up program, use the methods developed to probe into more fundamental degradation mechanisms.
- Collaborate with the other research groups that are working on degradation studies.
- Need close collaboration with (a) fuel cell developer(s) so that all the work done here on instrumentation could lead to more solid physical interpretations of degradation mechanisms.
- Project concludes 9/2006. No additions or deletions are recommended.
- Not recommended for future support. Significantly different characterization is needed in the future as the current approach is not answering the questions posed.

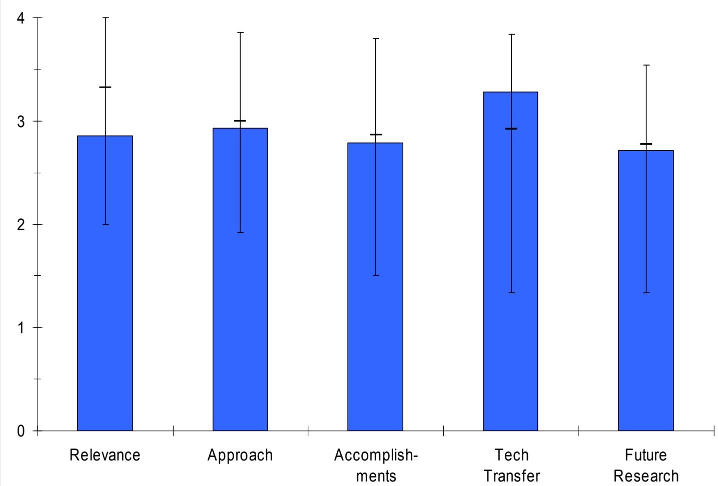
Project # FCP-20: Residential Fuel Cell Demonstration by the Delaware County Electric Cooperative, Inc.

Mark Schneider; Del. Co. Electric Co-op

Brief Summary of Project

Delaware County Electric Cooperative, Inc. (DCEC) is validating the objectives of propane-fueled hydrogen fuel cells for edge-of-grid residences via a field trial demonstration to understand the technical and economic viability of fuel cell alternatives to new line construction. Specifically, DCEC is measuring and reporting technical performance, providing raw cost data and economic viability analysis, documenting maintenance and operations concept enhancements specific to residential fuel cells, sharing safety related vulnerability analysis and lessons learned, and promoting education of state and local consumers.

Overall Project Score: 2.9 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The project is not an R&D project. It is more likely demonstration program.
- Demonstration projects are beneficial for the development of alternative power sources. The program demonstrated the importance of establishing the infrastructure to supply controlled fuel products to a PEM fuel cell. The program partially supports the President's hydrogen vision in that it identified problems in the delivery network and limitations of the "commercially" available PEM fuel cells. The project did not identify a process for advancing the state -of-the-art of PEM fuel cells. The project tests to determine if PEM fuel cells are ready for commercialization and mass distribution.
- This project provides demonstration experience in a real environment. The project provided the utility co-op and the fuel cell developer with a new understanding of failure modes and product design requirements.
- The co-op also assessed the economics of this particular site, thus providing information on potential early adopter markets.
- This project provides an excellent opportunity to educate public on fuel cell technologies.
- The project fits well within the goals of the hydrogen vision and RD&D plan, maybe better classified as a technology validation or education project.
- Field testing and demonstrations of prototype and near commercial fuel cell systems is essential to moving this technology forward.
- The project to demonstrate a fuel cell home that can function independent from the local power grid is relevant to the Hydrogen Fuel Initiative in that it enables experience to be gained with fuel cells in a stationary application. Even though this project is not related to transportation, it provides valuable learning and helps to establish the fuel cell manufacturing base.
- Real life demonstrations are very helpful.

Question 2: Approach to performing the research and development

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

- The PI should discuss and cooperative the project and the cooperative with PEMFC manufacturers and production companies.

- The project did identify barriers to installation and operation of the PEM fuel cells and the limitations of near commercial PEM fuel cell systems. The project did not address properly the codes and standards issues for fuels to be used in a PEM fuel cell. The approach should have carefully regulated the quality of the fuel for the PEM fuel cell. The project was fortunate the impurity was only methanol that affected the fuel processor cleanup process. Had the impurity been carbonyl sulfoxide or other species that can be formed in a reformer, dangerous emissions could have occurred. The project did not appear to fully appreciate the safety aspects of operating a fuel cell.
- The project developed a well thought out plan and pulled together appropriate team members to implement the plan.
- The project used available technologies, but was designed to provide information to assist in development of next generation projects.
- The approach is solid and the project has performed to plan. There is good integration with other similar projects but this could be expanded.
- Approach is systematic and clearly explained in 5 categories.
- The approach is reasonable for demonstrating an early generation fuel cell. Closer coordination with the vendor may have been able to foresee and prevent the early failure caused by the methanol in the new propane tank (which is used to remove any water in the as received tank). The presentation indicates that the barrier addressed in this project is co-production of hydrogen and electricity. In fact, the project does not produce any hydrogen for export.
- Demonstration at electric co-op sites provides very useful experience and data.

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

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

- The technical accomplishments for project are fair.
- The project has identified no real barriers that were not known by the manufacturer of the fuel cell. The flicker issue indicates the manufacturer is not providing a device that delivers grid quality power. The manufacturer should know the power is not grid quality and have disclosed this to the DCEC. The DCEC should have a contract that assures the delivery of grid quality power. These are not issues requiring technology accomplishments by the PI. The thermal recovery issues are well known problems for PEM fuel cells and should have been identified by the fuel cell manufacturer. Systems analysis clearly identifies these problems which are well known by the fuel cell industry. No new technical information was obtained through this project.
- This project provided information of value to both the fuel cell developer and the utility co-op. The fuel cell developer learned about the cause of early system failures and the co-op learned about power electronics requirements and product performance. These insights will save the fuel cell developer money in the future and lead to a better product.
- Additional operating experience will provide data for product optimization leading to better performance at probably lower overall cost.
- The project has had very good results for the low amount of DOE funds invested in the effort.
- This project identified "infant stack" failure as a significant concern.
- Large cyclic loads cause short-term flicker (not due to fuel cell!).
- Thermal management became an issue.
- The project was designed, built, installed, and is undergoing testing, which is almost complete. The project ends in January 2007. Several problem areas with the system were identified that highlight the need for further development of the Balance of Plant components, in this case, the power inverters. The lack of applicable codes for fuel cell systems in the NY area was also identified; although the project was successfully sited and operated over a ten month period.
- Additional data on energy saving benefits are not presented.

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

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

- The PI doesn't have any technology transferable. The PI should work the project with PEMFC system companies.

- Interfaces for technology transfer were established. The project should identify on its website that the PEM fuel cell does not provide grid quality electricity. Is that done? The internet site should identify that codes and standards for fueling the PEM system were not followed by this project or are not available. If the codes and standards are not available, some risk must be assumed by the operator of the fuel cell.
- It is not discussed in detail but clearly the co-op and the demonstration served to bring together industry and universities.
- What I thought was most important was the transfer of technology to the market place.
- The investigators have clearly demonstrated a strong effort and good success for sharing the results of the project; emphasis needs to be maintained through the end of the project.
- This poster itself is an excellent example of technology transfer – the fuel cell system is out in the field for a year.
- Public and academic outreach.
- There was evidence of collaboration between the partners. Final reports that are available to the public will be available at the partners' web site.
- This project has utilized excellent involvement of stakeholder's media and educators public internet access provision is a plus.

Question 5: Approach to and relevance of proposed future research

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

- DCEC is working closely with PEMFC companies.
- The project should complete its analysis of the performance of the fuel cell and power storage. The project should address how to eliminate the failures in fuel delivery and identify the limitations of the fuel cell power delivery.
- The project is approaching its end and so there are limited future activities.
- I think preparation of a good final report and distribution of this information to the general technical community is important to convey the benefits of real world demonstrations and also convey the specific findings.
- The proposed future work is appropriate for successful close out of the project.
- The project will be ending in June 2006 and the fuel cell will be decommissioned.
- Follow-on projects at other organizations are being planned.
- Technical analysis will be completed by January 2007.
- Data collection ends next month and the system will be decommissioned. Operating experience and data will be analyzed and a final report will be available on the project web site.
- Additional operation on fuel cell will be valuable if resources permit.

Strengths and weaknesses

Strengths
- The company is an Electric Cooperative company.
- There exists a need for real world demonstration of new technology. This project fulfills that need to a limited extent by moving emerging technology to the consumer.
- This project has a well thought out approach and good implementation.
- A number of important lessons have been learned that will benefit future products and make them more cost effective.
- The project has identified several operational concerns for utilizing stationary fuel cells, especially in a rural application. The investigators have taken appropriate steps to ensure that the results are shared and have been successful in these efforts. The project was a very effective use of limited amounts of DOE funds toward demonstrating and validating current technology stationary fuel cells.
- DCEC performed a real-world field trial for a year, so it could identify seasonal effects such as the mismatch between space heating requirements and thermal integration of the fuel cell system.
- DCEC identified several issues and proposed options to resolving them.
- DCEC had good support from the fuel cell vendor.

- After initial problems with drying the propane tank, the fuel cell operated well. Reviewer comments from last year were addressed. A large body of data from similar projects from around the country will be assembled from which conclusions will be drawn.
- Excellent concept for residential co-generation strong team particularly for outreach. Propane is a good selection for end of grid applications.

Weaknesses

- They need to work with PEMFC companies for installation and demonstration of residential fuel cell power. What is the hydrogen source? On page 14, propane and methanol are not good for PEMFC system. Propane and methanol need to be reformed for PEMFC system.
- The project did not fully address the requirements and risks of installing a new technology in commercial/residential environment. There appeared to be little adherence to codes and standards or to the safety aspects of reforming propane in a residential/commercial environment. Safety, codes and standards need to be fully addressed.
- Multiple installations would have provided more meaningful results.
- Technical data have not yet been analyzed and reported – so understanding significance of the project will have to wait until 2007.
- Understanding the variability of propane quality and its impact on system design needs to be improved. Overall energy savings estimates are not provided.

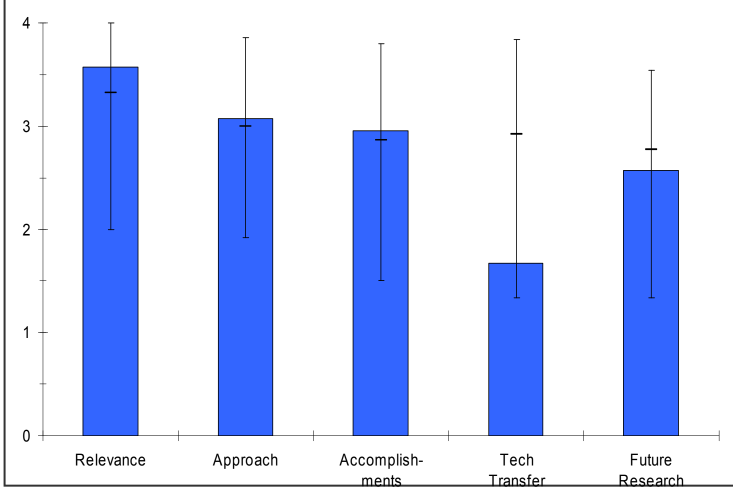
Specific recommendations and additions or deletions to the work scope

- The project is not worth continuing.
- One reviewer commented that the use of the term "infant stack failure" might be offensive to families that have experienced SIDS. Perhaps it should be changed to "early/new stack failure."
- Project approaching completion.
- Similar projects should be strongly encouraged to build a database of operating experience.
- This project would present better if data and experiences from its sister project in NY were included.
- Characterize all the components in propane that influence the design of the desulfurizer and then provide a strategy to match the load transients.

Project # FCP-23: Sub-Freezing Start-up of a Fuel Cell

Dennis Papadias; ANL

Overall Project Score: 2.9 (7 Reviews Received)



Brief Summary of Project

The goal of this ANL project is to understand fundamental aspects of the start-up process at sub-freezing conditions and to identify the key mechanisms that limit rapid start-up and lead to failure. ANL will study the effect of different start-up and shutdown protocols on fuel cell durability and performance.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Sub-freezing start-up of a PEM fuel cell is critical for success. The goal to understand the fundamental aspects is key.
- This is a new project and it shows good early progress.
- A modeling effort is needed to understand freezing effects in fuel cells better.
- Key transportation issue.
- Freeze start-up and survivability are critical to vehicular fuel cell development.
- Modeling work important.
- Experimental verification proposed.

Question 2: Approach to performing the research and development

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

- Model development and single cell experiments are proposed. The models and the experiments appear to be too general; focus should be given at first to local areas (i.e., one dimension) and then spread out. Experiments should be more carefully designed.
- The approach is a good combination of theoretic modeling and future filtering of experimental data.
- A more elaborate model is needed. Localized effects are modeled at steady state and will not give any real insight to the system performance.
- Modeling coupled with single cell validation is a good approach.
- It will be important to couple this effort with partners that can link stack issues (dimensions, response times, etc.) with single cell models.
- The researchers are not clear on what they might find or how they will use these results. There is no clear path to apply these findings.
- Good combination of experiments and modeling.
- Logical approach. Focused. Project addresses difficult issue.

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

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

- Modeling results on air flow rates and the effect on the removal of water are presented. No results on actual shut down or start-up protocols are presented. Models show water transport in GDL and flow channels Experimental apparatus appears to have been built and tested at 75°C and 25°C but not at below freezing yet?

- Based on information provided and guidance of the ANL fuel cell team, the project has made good progress.
- This is still relatively early in the program, and technical accomplishments are reasonable for this period of time.
- The project's initial results appear "on track" with regard to relating observed issues with predictions from the developed models.
- So far only baseline start-up has been accomplished.
- Project in very early stages.

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

This project was rated **1.7** for technology transfer and collaboration.

- No collaborations appear?
- It is not possible to assess this at this early date.
- Project is just starting; however, I would expect that the nature of this project will require partners to assist in the validation of the proposed models, and to generate data for improving the existing models.
- Proposed collaborative efforts not evident.
- No collaborators are mentioned, how does this work coordinate with similar work at LANL?

Question 5: Approach to and relevance of proposed future research

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

- No specifics given on future work.
- The PI and his team have made a good description of future work.
- Work is largely exploratory, and the plans for studying perturbations in the system are particularly weak. A more systematic approach is needed.
- This project has a lot of potential to identify key issues and generate/inspire "engineering solutions" for fuel cell stacks.
- Work proposed important.
- More details regarding future work would be useful.
- Timeline not detailed.

Strengths and weaknesses

Strengths
- Modeling effort is underway. Experimental apparatus built.
- Project is very relevant and addresses freezing effects that need to be addressed for fuel cell commercialization.
- Project has good underlying fundamentals, both with proposed models and the test apparatus available for generating data and validating the models. The test apparatus is quite "flexible" and should allow "engineering solutions" to be evaluated and qualified.
- Good combination of experiments and modeling.
- Addresses an important topic, quantitative models that elucidate phenomena.
- Overall well thought-out and managed project.
- Strong team.
- Important technical issues addressed.

Weaknesses
- Lack of focus and clear goals. No protocols for shut down and start-up are given. Which will be tested? What are the possible prior shutdown scenarios and power draw as a function of time and temperature?
- Correlation between experimental and simulation work needs to be better developed. Experimental work seems to duplicate experiments at LANL.
- This project's viability will benefit by having partners in both the theoretical and stack fabrication areas. These resources will be needed to accelerate and deliver any understandings to resolving cold-start issues.

- Lack of industrial collaborators.
- No collaboration. Since the focus is at the stack level, and because manufacturers are reluctant to share details of their design, this work will not achieve its potential.
- More experimental vs. model data needed.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- More details on the experiments and their priorities must be given.
- Project needs a clear vision of how these findings will be applied. Should coordinate efforts with modeling and experimental work going on at other institutions (UTC, LANL, LBNL, etc.).
- This project should include partners in both the theoretical and stack fabrication areas. These resources will be needed to accelerate and deliver any resolutions to resolving cold-start issues. This project should be closely aligned with other projects, if not combined, such that better utilization of resources and acceleration of results can be achieved.
- Need to test a large number commercial and non-commercial MEAs. Must cycle fuel cells through cold and hot conditions to understand materials durability issues.
- Add more fundamentals of transport of water during freeze/thaw rather than high level model of system.
- Prioritize to ensure continued focus.
- Disseminate results at least through publications.

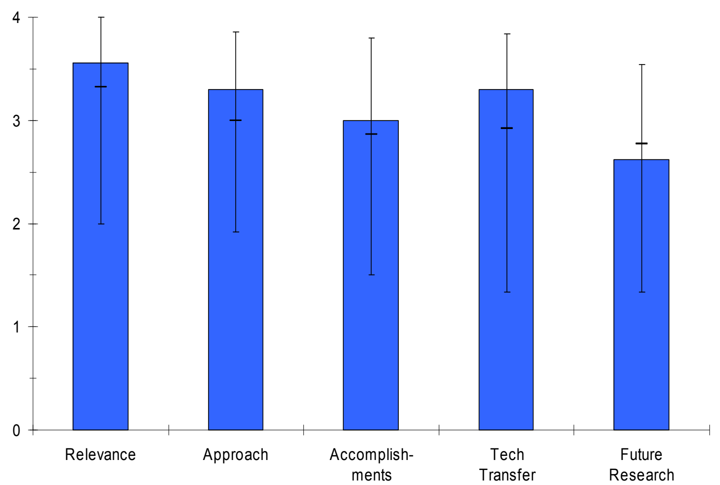
Project # FCP-25: Corrosion Protection of Metallic Bipolar Plates for Fuel Cells

John Turner; NREL

Brief Summary of Project

This project at the National Renewable Energy Laboratory (NREL) is concerned with the identification and characterization of metal alloys and coatings for application to PEMFC bipolar plates. This work includes determining corrosion rates in simulated anode and cathode environments, measurement of interfacial contact resistance, and analytical determination of the passive film composition. NREL will correlate these results with the composition of the base metal alloy and the coating (if any), looking to identify more stable alloys and coatings to provide low interfacial contact resistance. A major portion of this work is in collaboration with Oak Ridge National Laboratory

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Metal bipolar plates provide a possible alternative to graphite plates. At this stage of development, technology options reduces risks.
- The objectives of this project are aligned with targets for bipolar plate cost, resistivity, and corrosion rate.
- The subject matter (protecting bimetal plates from reaction with fuel cell fluids) is a very important, if less glamorous technology area that needs to be resolved.
- Corrosion work important.
- Good systematic approach to complex problem.

Question 2: Approach to performing the research and development

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

- The approach appears to encompass several parallel paths, rather than focusing the effort on one. Nitriding of steels, conductive coatings, assessing impact of cations on membrane conductivity, and then measurements in phosphoric acid (PBI-like conditions).
- Focussing resources on the most promising approach going forward may prove more effective.
- It may be more cost effective to validate the resistance and corrosion measurements in a single cell test rather than building a stack. The test setup is simpler and one could make more measurements to validate the resistance and corrosion measurements.
- Investigating plates for high temperature phosphoric acid applications is tangential. The remainder of the approach is relevant.
- The PI is trying a good collection of surface treatments.
- Should keep in mind/address the cost issue when coating metals.

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

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

- The work has differentiated different steels. It would be beneficial to validate this ranking of materials and the test methodology with single cell measurements.
- Need to improve the cost projection to include the cost of the coating and fabrication process. Are other properties important such as thermal conductivity? How will the coating work on plates with flow channels? Can one obtain a uniform coating on 3D profiles? How effective is the coating on edges or corners? Will thermal/mechanical stresses result in cracking of the coating?
- Is the tin oxide work in parallel with nitriding of the steel? How will this coating stand up to bending of the interconnect? Will cracks form? Given its use in the PV area, what is the projected cost? How will it work on 3D surfaces?
- For the budget, the work appears fragmented between too many subject areas: nitrided steels, tin oxide coatings, membrane measurements, and phosphoric acid tests.
- The investigators have identified a good range of candidate materials; beginning of life (BOL) resistance measurements are presented. Corrosion measurements must include cycling.
- The experiments, generally, show DOE goals being reached.

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

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

- Interaction with ORNL.
- Oak Ridge has been used for material synthesis, and Jadoo will be used for fuel cell evaluation; no further collaboration is suggested.
- There is a good degree of industry/government collaboration (e.g., Plug Power and NREL/ORNL). The PI realizes that project metrics also include manufacturability of plates, and partnerships in this area could be further developed (e.g., Manufacturing Roadmap).
- Should try to establish a collaboration with an industry member who can mass produce high volume metallic bipolar plates.

Question 5: Approach to and relevance of proposed future research

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

- Approach to determining adequate formability is unknown. The extent to which SnO_2:F coating will be incorporated in the study is unknown.
- Proposed research along lines of "more of the same." Future activity should also include ties with manufacturing research.
- Work proposed important.
- More details regarding future work would be useful.
- Timeline not detailed.

Strengths and weaknesses

Strengths

- The investigators have fabricated and confirmed their fabrication of materials that are worthwhile candidates for meeting the identified targets.
- Good results in important, if less prominent fuel-cell development issue.
- Covering a wide range of steel and stainless steel in testing.
- Overall well thought-out and managed project.
- Strong team.
- Important technical issues addressed.

Weaknesses
- Effort possibly spread across too many tasks for the budget.
- The failure to test with cycling prevents an evaluation of corrosion resistance. The approach requires organization.
- Needs greater ties to manufacturing research in order to minimize total, rather than only materials cost.
- Cost presentation does not account for manufacturing costs. A collaboration with industry would be useful here. When discussing cost targets, all costs need to be accounted for if possible. Cost of material is not an indicator alone when comparing to a final cost target.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- Use more analytical tools to characterize coating. Consider additional tests of coating that might represent stresses from handling and manufacturing. Consider coating or treatments on 3D profiles.
- Interested in XPS profiles before and after stack operation. What is the impact of inclusions in the steels on the nitride or coating protection? Can these lead to point defects?
- A test plan for corrosion evaluation and formability evaluation needs to be added. Such a test plan should also define which material/coating combinations will be studied.
- Recommend adding combination of research with manufacturability (e.g., low-cost plate stamping).
- Prioritize to ensure continued focus.

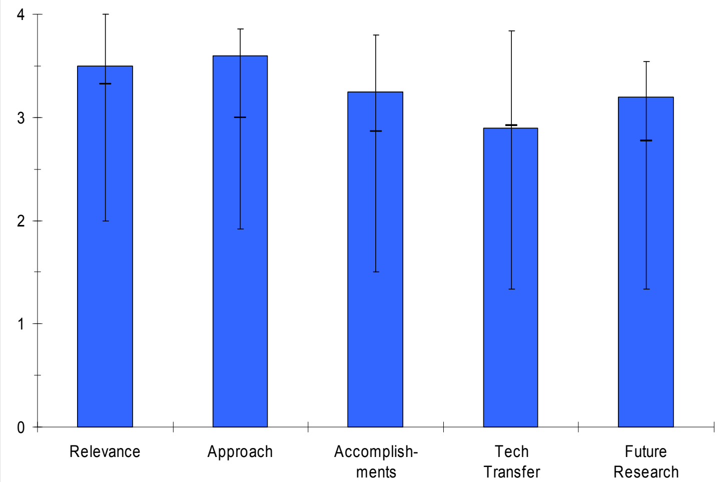
Project # FCP-26: Development of Low-Cost, Clad Metal Bipolar Plates for PEM Fuel Cells

Scott Weil; PNNL

Brief Summary of Project

To assist the DOE in lowering the cost and improving the durability of automotive PEM fuel cell stacks, Pacific Northwest National Laboratory (PNNL) is reducing the material and manufacturing costs of bipolar plates, while substantially increasing their resistance to corrosion and mitigating the release of poisonous metallic ions into the MEA. This project consists of a feasibility study to determine the potential efficacy of a clad metal approach in developing a low-cost/low-mass/low-volume PEM bipolar plate; formability testing of Nb and Ni clad metals, followed by *ex situ* validation testing of formed pieces; and fabrication of small-scale plates for short-stack testing.

Overall Project Score: 3.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Metal bipolar plates provide a possible alternative to graphite plates. At this stage of development,technology options reduce risks.
- Objectives are aligned with durability and cost targets.
- Development of durable, lightweight, easily manufacturable, low cost bipolar plates is an important enabling technology for fuel cell stacks. This project addresses these goals.
- Specific DOE targets highlighted.
- Good systematic approach to complex problem.

Question 2: Approach to performing the research and development

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

- The approach u provides preliminary data on a number of new materials.
- May want to consider if sufficient Nb would be available if fuel cells are commercialized by doing order of magnitude estimates and comparing with current or projected production volumes. Discuss with ATI Wah Chang.
- Include high level cost estimate of the boronizing treatment in the cost analysis.
- The use of clad metal material is a very particular approach. The investigators are giving this approach its best chance with Nb and boronized Ni.
- The technical concept of designing a low cost bipolar plate using a metal composite and roll cladding is attractive for its potential for easy manufacturing and low cost. The PI seems to be taking a logical approach to experimentally determining which cladding materials can best meet the needs of the program.
- Logical approach. Focused. Project addresses difficult issue.

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

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

- Both classes of clad materials still need to be fabricated at lower thickness (of cladding) in a very limited timeframe.

- Accomplishments to date seem promising with both the B-Ni and Nb cladding materials; however, the ability to form channels in the cladded materials without detrimental effects to the cladding thickness or material characteristics will be critical to assessing the viability of the technology.
- Good progress.
- Significant number of milestones met or achieved.

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

This project was rated **2.9** for technology transfer and collaboration.

- At this stage of feasibility demonstration and with the limited budget, would not expect very much collaboration.
- Given the limited timeframe, the direct National Laboratory/small company collaboration is sufficient.
- Collaboration at this time is limited to the roll forming/cladding facility.
- Publication record is good. Collaborations could be deeper. Not apparent the contributions from partner.

Question 5: Approach to and relevance of proposed future research

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

- Reasonable proposed next steps to assess performance in stack environment and to consider potential issues associated with exposed edges.
- As noted above, should consider high level estimation of processing costs for the boron treatment.
- The sequence of future work is well-defined; time may not allow all of it to be done. In-situ experiments are not listed.
- Future plans are consistent with the scope and funding level of the project. Work to investigate the ability to stamp channels in the material without damaging the cladding will be important.
- Reasonably focused plan.

Strengths and weaknesses

Strengths
- Sequence of tests is well-defined.
- The investigators are capable of fabricating the needed materials.
- Opportunities for low cost, easy-to-manufacture bipolar plates.
- Cladding materials seem to meet goals of project and are low cost and plentiful.
- Overall well thought-out and managed project.
- Strong team
- Important technical issues addressed.

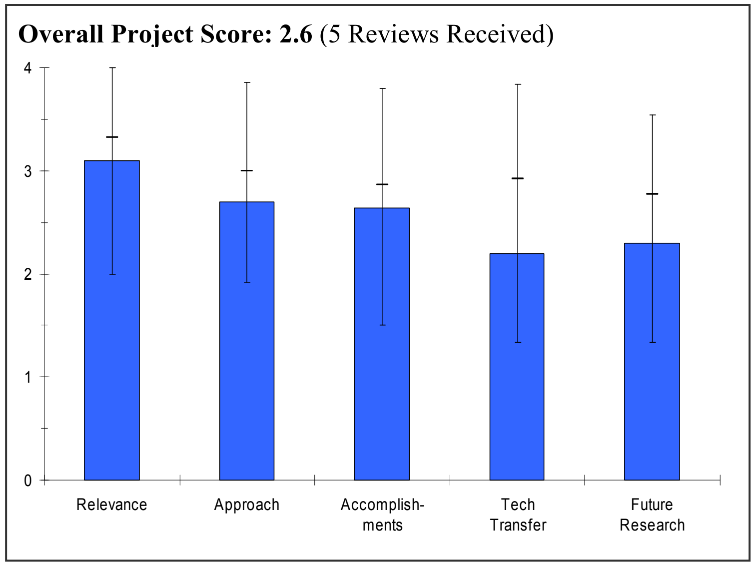
Weaknesses
- The project is too ambitious for the time given.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- Breakout cost of the substrate, clad material, and any post processing. Then add the cost of finishing operations to form flow fields to provide value that can be compared with molded graphite or expanded graphite plates.
- Consider discussion of ultimate plate thickness and impact on bipolar plate thickness. This will provide data to estimate benefits relative to kW/kg or kW/L of the stack compared to projected values for carbon plates.
- The project should be extended past 9/30/06. *In situ* experimentation on cladded plates will be able to confirm whether DOE targets are met.
- Prioritize to ensure continued focus.

Project # FCP-27: Advanced Catalysts for Fuel Cells

S. Narayanan; JPL



Brief Summary of Project

The overall objectives of this Jet Propulsion Laboratory project are to reduce the cost of fuel cell stack components and to reduce the amount of precious metal used. Work in 2005 and 2006 is focused on developing methods for combinatorial screening of oxygen reduction reaction catalysts, identifying catalysts capable of performing at 2500 mW/(mg of precious metal), and increasing the cathode potential by 0.1 V at 500 mA/cm^2.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project is irrelevant for the DOE 2010 target goals for the design of novel multi-metallic cathode catalysts.
- Reduction of fuel cell catalysts cost by reduction of Pt content is aligned well with the FC plan objectives.
- Reduction of Pt loading is one of the identified RD&D plan objectives.
- By addressing the cathode catalyst cost and precious metal loading issues, this project is very relevant to the objectives of the HFCIT program.

Question 2: Approach to performing the research and development

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

- The objective and approach is neither original nor well justified.
- Addition of low weight metal such as Ni or small at % of the tried element (such as Zr) will not substantially reduce the cost of cathode catalysts. The correct approach would be to improve catalytic activity of Pt or the existing Pt bimetallic systems. With the later approach the catalyst loading and thus the cost can be significantly reduced.
- Combinatorial methods are already well developed and no need for further improvement is required.
- Systematic approach based on sputtered alloys followed by structural characterization.
- Quite late in PEMFC development to be pursuing combinatorial approach with sputtered films.
- The approach is promising and quite novel for platinum-based cathode catalysis.
- Multi-electrode array approach has helped to achieve high throughput in testing the ORR activity of various compositions.
- Transition from thin-film morphology to dispersed catalysts may prove difficult.
- Initial durability data should have been presented by now to justify viability of the approach.
- Should keep in mind/address the cost issue when coating metals.

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

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

- Catalytic activity of Pt-Ni catalysts is rather low and are not consistent with literature data.
- The effect of Zr is neither well justified/understood nor provides any guideline how to create new multi-metallic systems.

- To make any progress in the quest of developing new cathode catalysts, the PI needs to get insight into the relationships between the surface characteristics and catalytic activity. The progress should be made if the PI would combine experimental results with the existing quantum mechanical calculations.
- This is a limited scope project, the library studied was well selected, might be beneficial to compare few binary or ternary libraries.
- As usual, combinatorial approach finds a "sweet spot" in composition. Problem is, of course, that the combinatorial method of synthesis does not produce a "usable" catalyst, i.e. one that can be used in a fuel cell. Further work, actually more difficult and extensive than discovery, is needed to produce a real catalyst with this composition.
- Improvement in the relative performance of the ternary catalysts versus that of the Pt reference is promising as is the potential for lowering the Pt loading.
- At ~0.77 V, the ORR onset potential at the Pt reference, defined as the potential measured at 1.5% of the limiting current, is much lower than expected. This casts doubt on the quality of the presented data for all tested materials. Could the purity of the sputtered catalysts be a problem? Should ORR kinetic studies be performed on large stationary electrodes as those used in the presented work?

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

This project was rated **2.2** for technology transfer and collaboration.

- The project needs strong collaboration with other partners because the PI does not have adequate knowledge in the field of the oxygen reduction reaction.
- No collaborative efforts were reported.
- None.
- Good partners in materials characterization. More collaboration in the materials development might be helpful.
- Should try to establish a collaboration with an industry member if they are planning on continuing this work. Is Pt/Ni new development?

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated **2.3** for proposed future work.

- Future work plan has no relevance toward eliminating the barriers and advancing the program.
- Important addition proposed in the work plan is to study various alloying elements. This will allow for comparison of effects due to nature of specific alloying element.
- Where is the plan to produce a real catalyst?
- Further catalyst performance screening should be preceded by durability testing of selected existing materials.
- Acceptable purity of sputtered catalysts needs to be demonstrated using electrochemical and, perhaps, other techniques.

<u>Strengths and weaknesses</u>

<u>Strengths</u>

- Relatively well developed sputter-deposition thin film method.
- Possibly in thin film synthesis.
- Original approach to making and testing cathode catalysts with low Pt loading.

<u>Weaknesses</u>

- The project is lacking fresh ideas which are required for the development of new generation of active materials for the ORR.
- Late in the combinatorial game and probably duplicative over what 3M and Jeff Dahn have already done (and many others, e.g. Honda/Symex).
- Most of the research effort has gone into technique validation, which was also the focus of this project in FY05.
- Lack of durability data.

Specific recommendations and additions or deletions to the work scope

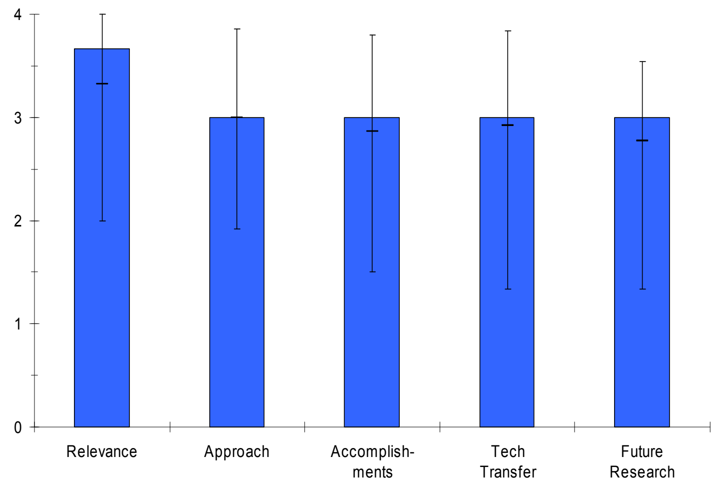
- Variations of synthesis method, e.g., evaluation of layered structures (top Pt layer, for example) combined with study of activity and durability.
- Why continue?
- Investigate the origin of surprisingly low values of the ORR onset potentials. Viability of the Pt-M-Zr catalysts for fuel-cell type cathode should be demonstrated soon.
- It is unclear in the poster if durability is considered.

Project # FCP-28: Contaminant Effects
Debbie Myers; ANL

Brief Summary of Project

In this project, ANL will: determine the mechanisms for the degradation of PEFC performance by impurities in hydrogen and in air; predict the long-term effects of impurities on PEFC stack performance; predict the effect of operating conditions and impurity concentration on PEFC stack performance; develop strategies to mitigate performance degradation and enhance stack durability; and develop strategies to recover stack performance after the impurity is removed from the fuel or air streams.

Overall Project Score: 3.1 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Understanding the effects of anode and cathode impurities is vital to both fuel cell durability and performance as well as in determining a proper H_2 fuel specification.
- Catalyst performance and lifetime are key issues for meeting transportation goals.
- Critical study to enable fuel cell commercialization.

Question 2: Approach to performing the research and development

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

- There would be value to extending the range of temperatures studied to sub-freezing when possible.
- PI has identified a useful evaluation technique for the initial contaminant studied.
- The PI's inclusion of "how permanent the contaminant effect might be on the Pt and methods to mitigate the effect" will be a useful protocol for future contaminants studied by the project.
- Good, but it is not clear how different this is from the 10+ years of work performed at LANL.

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

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

- Project is at a very early stage. Too early to gauge.
- I would expect further refinement/additions in the evaluation technique as other contaminants are studied.
- Good, but Cl^- poisoning has been well studied by Uribe et al. Organo-chlorine compounds have not been studied in detail yet so a nice extension.

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

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

- Would benefit from more collaboration with automotive (and stationary?) OEMs to focus cathode contaminant candidate list.
- Link and develop synergies with similar LANL project (FC-24).

- The PI needs to include the FreedomCAR Fuel Cell Technical Team's feedback as the various contaminants are studied.
- Work with tech teams is good, but really needs to be coordinated with LANL so that a lot of time is not wasted repeating experiments.

Question 5: Approach to and relevance of proposed future research

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

- Too much listed for a single year. Needs multi-year funding.
- Project just starting, and has the potential to provide much-needed guidance on the contaminants issue – both for prevention methodology and selection of catalyst formulation.
- Good, but are organo-chlorine compounds really the biggest priority, especially in relation to pollutants that the early fuel cell cars will have to deal with from conventional combustion.

Strengths and weaknesses

Strengths
- Good technique and protocol for evaluating contaminant effect/duration on catalyst systems.
- Inclusion of potential contaminant's "persistence and mitigation" will be an important feature of the project.
- Component level analysis tied to stack model.

Weaknesses
- The project should include a listing of the "top 10" contaminants as defined by the FreedomCAR Fuel Cell Technical Team. The listing should include the source (air, components, etc.), expected concentration, priority for review (based on expected performance decline), etc. This "scorecard" can then be updated by the Project's results on the effect of the contaminant, including how fast the contaminant affects the catalyst system, is the effect permanent or reversible, etc. This "scorecard" then becomes a guide for industry in selecting catalyst systems as well as specifying fuel cell stack/system design, etc.
- Needs to be coordinated with past and present work at LANL.

Specific recommendations and additions or deletions to the work scope

- Need to carefully consider real pollutant and impurities that will be experienced by the early fuel cell cars in real air sheds.

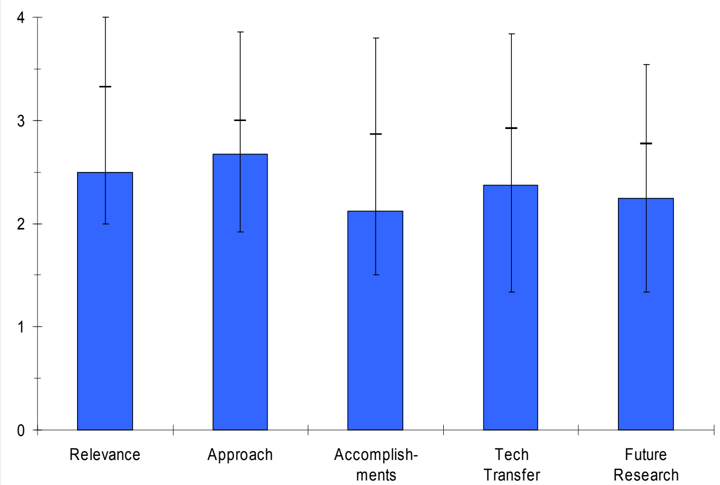
Project # FCP-29: Non-Platinum Catalysts

Xiaoping Wang; ANL

Brief Summary of Project

This Argonne National Laboratory (ANL) project is developing a non-platinum cathode electrocatalyst for polymer electrolyte fuel cells. The goals are to lower the cost and enhance the durability of the catalyst while maintaining and/or improving the performance as compared to the currently-used platinum-based catalysts. The approach this year focused on noble metal-base metal alloy nanoparticles and base metal macrocycles attached to polymer backbones.

Overall Project Score: 2.4 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- The project is irrelevant for the DOE 2010 target goals for the design of novel bimetallic cathode catalysts.
- Developing a non-Pt catalyst addresses the cost target. The use of precious metals prevents the relevance from being "outstanding".
- For practical application point of view, no other PGM with exception of Pd needs to be considered. If fundamentals of electrocatalysis are addressed than it is ok to work with Ir, Os, etc., but has to be clearly stated.

Question 2: Approach to performing the research and development

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

- The objective and approach are neither original nor well justified.
- The title is misleading; Pd, Ir and Rh belong to the Pt-group metals. Therefore, there is no justification that the synthesis process is scaleable at low cost.
- The PI has neither the knowledge nor required surface analytical tools for studying surface segregation processes.
- Of the two approaches, one is conventional (the modification of d-Bands), and the other is novel (attaching a metal to an electron-conducting polymer).
- Two unrelated topics are considered.

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

This project was rated **2.1** based on accomplishments.

- Pt-like catalytic behavior is not good enough to meet the DOE targets.
- In electrocatalysis, application of the Hammer-Nørskov theory requires more knowledge than simple analysis of the DFT predicted segregation trends.
- The d-band theory is, unfortunately, still not applicable for real nanoparticles.
- The O_2 reduction mass activities for the enhanced 1r and Pd-based catalysts are below the DOE target by an order of magnitude. Work on the other approach has not been completed.
- Some good work but difficult to judge.

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

This project was rated **2.4** for technology transfer and collaboration.

- The program needs strong collaboration with other partners because the PI does not have adequate knowledge in the field of the oxygen reduction reaction.
- Collaborators are mentioned, but the degree of collaboration is not well elaborated. What experiments were done with whom?

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Future work plan has no relevance toward eliminating the barriers.
- Testing in an MEA is an important next step.
- The bimetallic systems are failing, but there is no clear concept as to how to execute the electron-conducting polymer backbone approach.
- It seems that the focus is going to be exclusively on Pd-based catalysts in which case the stability should be immediately tested.

Strengths and weaknesses

Strengths
- The PI is a part of well respected group in the field of fuel cell technology.
- Systematic study with careful characterization.
- The investigators have the ability to generate ideas and apply recent knowledge to justify a research direction.

Weaknesses
- Whole program is weak and the PI has no experience in the electrocatalysis of a such complicated and demanding reaction such as the ORR.
- The PI did not show a path to meeting the chemical compatibility (durability) and cost goals. Pd-based system may meet cost target, but no data was given.
- The project has not produced a beneficial result.
- The only progress has been the enhancement of precious metal-based catalysts which will not satisfy cost targets.

Specific recommendations and additions or deletions to the work scope

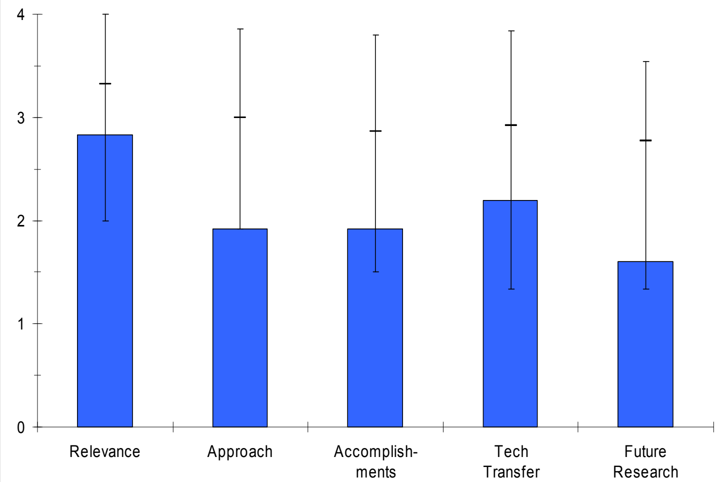
- Provided that a reasonable direction for the electron-conducting polymer backbone approach can be devised, the project should change to this approach.
- The bimetallic work should be stopped.
- Coordinated effort with other Labs will be beneficial not only to the fuel cell testing but on the fundamentals as well.

Project # FCP-40: Tungsten Oxide Cathode Catalysts
Joel Christian; OSRAM Sylvania

Brief Summary of Project

The objective of this OSRAM project is to produce a tungsten electrocatalyst catalyst with high specific power at a cost significantly lower than platinum. This effort includes evaluating the current catalyst in cathode applications and optimizing catalyst synthesis procedures to improve activity against the DOE technical targets for non-platinum catalysts. Performance evaluation is conducted at 250 hours and over 1000 hours.

Overall Project Score: 2.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Established as primary objective a performance metric (A/cm^3) that is not in the RD&D.
- The project is irrelevant for the DOE 2010 target goals for the design of novel non-precious metal cathode catalysts.
- This project attempts to find a stably active non-Pt oxygen reduction catalyst.
- It seems unlikely that the no-Pt route (as opposed to the low-Pt one) is the most probable means of achieving DOE's catalyst cost targets, but the decision to pursue no-Pt work was made by DOE (with additional direct input from Congress in some cases).
- The OSRAM Sylvania team is making progress with a low cost, non-platinum polyoxometalate catalyst. They have confirmed stability to 3200 h operation. A challenge with these catalysts is their low voltage.
- Development of active, low cost fuel cell catalysts are critical to the commercialization of fuel cells for transportation applications.

Question 2: Approach to performing the research and development

This project was rated **1.9** on its approach.

- Nothing new here. Tungsten-based catalysts were examined in great detail in the 1970's by numerous researchers and research groups.
- The approach has significant weaknesses; covering none of important aspects for the development of cathode catalysts.
- The PI must develop methods how to test a true specific activity of catalysts as well as to incorporate RRDE measurements to check the peroxide formation.
- The PI has no knowledge about the surface electrochemistry or the oxygen reduction reaction. The PI must to revisit old literature related to catalytic activity of the "Pt-free" tungsten catalysts for the ORR and to compare his data with the existing one.
- The PI must include theoretical models in discussing the possible active sites.
- This is an extremely difficult research area that demands great experimental care, in part because of the claimed need for total reduction of the material at very low potentials before it can exhibit full activity.
- This extreme pre-conditioning required, and prior operation as a hydrogen oxidation catalyst in at least some of the experiments, leads to a significant danger of Pt contamination of the cathode unless there is no Pt in the cell.

- The investigators properly have started no-Pt RDE experiments, but the initial data involve very low currents, only a quantitative, not qualitative, difference in the responses to O_2 and to Ar, and show plateaus at currents far too low to be real limiting currents.
- Having LANL do some experiments was a very good thing.
- While the appropriate activity target for non-precious catalysts is A/cm^3, data should still be shown in conventional electrochemical units of A/cm^2 to allow others to get a feel for the small magnitude of the currents measured here.
- The approach of this team is good in that they have teamed with LANL for fuel cell testing. They also have support from a lot of professors. Some of the materials analysis is a little inconsistent, and it's not clear how truly it reflects the *in situ* state of the catalyst. This might impede their progress, if they don't understand the mechanism.
- The activities of these materials are extremely low and a realistic path towards dramatic improvements in activity was not presented.
- The catalytic activity of POMs in general is very low. There is no fundamental background why such surfaces should be active for the ORR.

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

This project was rated **1.9** based on accomplishments.

- Objective testing at LANL shows 0.5 V at 10 mA/cm^2. This is said to be a 5x increase from baseline (!). To meet efficiency objectives there must be at least 20% rated power at 50% efficiency, i.e. at a cell voltage above 0.75 V. There is zero current at 0.75 V. 5x nothing is nothing.
- The presented activities are neither clear nor convincing. It is very difficult to separate capacitive components from a true catalytic activity.
- The best total activities achieved here are still quite small. If the electrode layer thickness of slide 11 is 10 microns, then the A/cm^3 scale is numerically equal to mA/cm^2. The activity in slide 11 appears to be roughly that of a Pt loading of 0.005 mg/cm^2 — far below a practical activity, and one wouldn't necessarily expect to be able to see such a small amount of Pt in the XPS measurements of slide 8.
- Los Alamos results did not reproduce any activity at 800 mV or above, admittedly with only a limited time spent trying.
- Because the strongly reductive pretreatment greatly increases the danger of Pt contamination of the cell, a more cogent explanation should be given as to why full reduction is essential to activation, and the additional chemical step that follows this reduction should be explained.
- The team met their own goals by passing their project milestone by 50%; however they still have a long way to go before they reach the DOE goals for non-platinum catalysts (p. 11 of presentation). The RDE performance of the catalyst seems very poor and they do not reach the limiting current expected for the oxygen reduction reaction. This result could be due to either peroxide generation or to insufficient supply of oxygen to the electrode. It's probably the latter, as they would not be able to achieve 3200 h stability if they were generating a lot of peroxide, but this needs to be sorted out. The stability of the catalyst at 0.24 V vs. 0.79 V is also not clear. The idea that the spacing between the W atoms enables the tungstate to mimic Pt is not really clear, as one could also argue that it is the d-band states of the Pt that make it special. It is not clear what the electronic configuration of the tungstate is. In summary, the team has made accomplishments, but the results are still not fully understood
- The activity was improved only slightly this year.
- Some improvement in the ORR activity has been shown but it does not make the system promising. 10 mA/cm^2 at 0.5 V is very low. Some data are not well understood.

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

This project was rated **2.2** for technology transfer and collaboration.

- What collaboration?
- This is not important because these type of materials will never be used as a catalyst for the ORR.
- This technology is in too early a stage to be transferred, so the lack of industrial participation outside the prime contractor for the work is not an element of concern.

- They have done a good job leveraging expertise at LANL.
- Collaboration with LANL is a good step. Need further input from someone with a strong background in electrocatalysis.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.6** for proposed future work.

- A thousand times nothing is still nothing.
- Future work plan has no relevance toward eliminating the barriers of advancing the program.
- The future work section of the presentation was dominated by a comparison of probable ultimate site densities that was hard to follow; the listed Pt metal utilization (assuming that is the ratio of surface atoms to total atoms) was 1/5 that of state-of-the-art commercial Pt catalysts.
- The proposed steps for 2007 are reasonable if the program continues.
- The path to improving the catalysts is unclear. Tungsten bronzes were first reported as cathode catalysts in sulfuric acid by Broyde in 1968, but they have still not "made it". The presentation does not clarify how they will move forward, i.e. Meet DOE's 2010 goals. Once concept on p. 15 is to disperse individual polytungstate molecules on the carbon, but there might be issues with agglomeration. How would the oxide be bound to the carbon? It seems like a good idea, but there are not specifics on how these ideas would be reduced to practice.
- A realistic path toward the vast improvements in the intrinsic catalyst activity was not presented.
- Some projections for future accomplishments look too optimistic.

Strengths and weaknesses

Strengths
- Tungsten resources.
- The PI is a part of great company which unfortunately has no experience in catalysts production for a such complicated and demanding reaction such as the ORR.
- Control over some very interesting tungsten chemistry has been demonstrated.
- Focused team, working well with LANL.
- Collaboration with LANL on testing of catalyst in MEAs at LANL.

Weaknesses
- Tungsten-based catalysts don't work as PEMFC cathodes.
- Whole program is weak and the PI needs simply to admit that tungsten based materials have no future as the OR catalysts.
- Quite small currents were measured; none at or above 800 mV at LANL.
- Overuse of the A/cm^3 units obscures how small the currents were.
- The basic concept may not be viable – others have looked at polytungstates and have never been able to make a significant impact on fuel cells.
- Low activity of electrocatalyst and overstatement of the promise of catalyst.
- Fundamental electrochemical behavior of POMs is not satisfactory.

Specific recommendations and additions or deletions to the work scope

- Never should have been funded in the first place. Terminate as soon as possible.
- Consider investigating these materials as special supports for Pt, possibly giving strong metal-support interactions yielding improved specific activity and/or resistance to voltage cycling. But be aware that in the flurry of activity on similar materials after the Bockris Pt-contaminated experiments, similar tungsten bronze materials were looked at as supports and, in systematic studies, showed no enhancement of Pt activity.
- The analysis of the tungsten XPS is a little funny. It seems to be integrated in reverse so that the W $4f^7$ peaks are 1/2 the size of the $4f^5$ peaks. Some work needs to be done to make the analysis make sense, as there might be more valuable information there. One interpretation is that there are two W(IV) species, one with a peak at 32 eV, and the other at 33.5 eV (corresponding to WO_2 and the other to the polyoxometalate).
- Project is due to end this fiscal year.

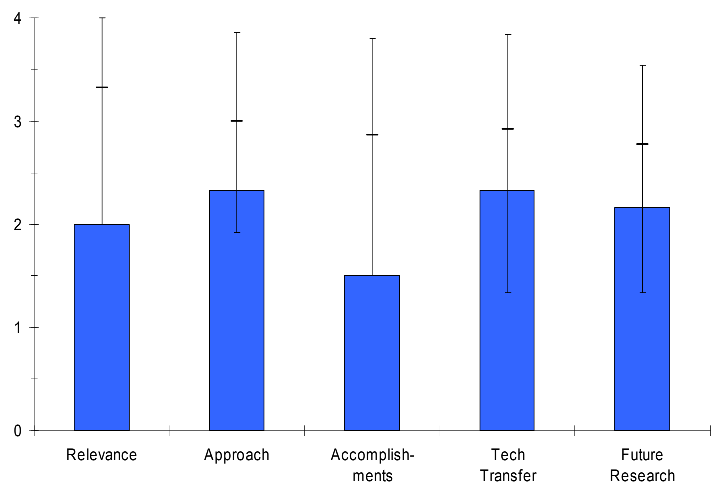
Project # FCP-42: Smart Fuel Cell Operated Residential Micro Micro-Grid Community Grid Community

Mohammad Alam; University of South Alabama

Brief Summary of Project

The University of South Alabama has completed an operational analysis of a multiple fuel cell power plant based mini-grid system. Novel materials suitable for efficient light harvesting photo-anodes are being developed. A methane reformer and a metal hydride based compression and purification process are being modeled. Fuzzy logic based energy management algorithms have been implemented to manage power demand around a threshold better. A similar neural network based algorithm is being developed to manage the water heater.

Overall Project Score: 2.0 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.0** for its relevance to DOE objectives.

- This project is identifying mechanisms to encourage the introduction of fuel cells into the residential buildings market.
- The relevance is not at all clear. This project does not address any major issues in the commercialization path. This work may result in minor improvements (e.g., improved control strategies) that will likely be done by industry anyway and would best be conducted on systems that are actually closer to commercialization (i.e., these are system optimizations that are best done later since there are not any real technical barriers being addressed). In any case, the work done here certainly does not justify the budgets allocated.
- The topics investigated under this effort generally support the vision and R&D objectives but do not appear to be focused on addressing critical path developments needed to meet specific measurable goals of the R&D plan.
- Most aspects of project align with the President's hydrogen vision and RD&D plan objects.
- This project will not address durability of stationary fuel cell systems, but may help lower costs by reducing needed installation capacity and mitigating market entry barriers.

Question 2: Approach to performing the research and development

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

- The approach is ok for the goals stated, however, it is mostly just modeling and data collection, with no real apparent attempt to do any model validations. Certainly, with the budget allocated, more work with actual hardware could be done.
- The approach to the research was not satisfactorily communicated.
- The approach is generally well thought out; however, inclusion in this project of Task II – "Production of Hydrogen Using Photo-Electrochemical (PEC) Solar Cells" and Task III – "Modeling of Hydrogen Production, Purification, and Storage" is questioned.
- Tasks II & III diffuse the main focus of this project, smart energy management for residential fuel cell applications.
- The task should focus solely on using smart energy management to reduce installed costs via load shedding, increasing potential fuel cell customer acceptance, and identifying optimal sizes, configurations, and control strategies.

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

This project was rated **1.5** based on accomplishments.

- Fair, but deliverables per dollars spent does not appear impressive. No real results shown, so it is difficult to judge. The lack of results is especially disappointing for a program so far along.
- The accomplishments do not appear to be appropriate for the level of funding applied to this project.
- Technical accomplishments 3+ years into the project are fair.
- Micro-grid community layout complete including sizing of fuel cells and potential energy management strategies.
- No data is available yet on the costs of a fuel cell powered micro-grid community nor other possible mechanisms for cost reduction besides lower installed capacity via load shedding.

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

This project was rated **2.3** for technology transfer and collaboration.

- What is going to be done with these results, besides being published? Are there any interactions with companies that make these products? If not, why not? If so, are they actually interested in this work? Stated interactions appear to be limited to fuel-cell testing houses (e.g., HARC), not OEMs.
- The investigator clearly shared results through publications, but the poster did not make evident any in-depth collaborations with other investigators from other organizations.
- Project has fair interaction with other institutions and projects. Efforts were made up front with Alabama Power to look at load management strategies and typical home load profiles.
- PI should bring into project discussions with residential home builders to gain insight as to most likely pathways of market entry for residential fuel cell systems. No discussions to date have been conducted with builders and what their interests are with regards to residential fuel cell systems and how market barriers may be overcome.

Question 5: Approach to and relevance of proposed future research

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

- It looks like some real hardware testing and model validation may take place in Phase IV.
- Proposed future work appears to follow on to the past efforts, but insufficient details were provided to assess the future plans with any degree of confidence.
- PI indicated future research (if funding is available) would focus on implementation of the fuel cell system and the energy management system into the university housing community. This is a mistake – implementation would be best in coordination with the advice of professional builders to gain insight to best market entry points.
- The market entry point for a fuel cell micro-grid system would be in the high end home community – not average 2,000 sq ft middle class homes.

Strengths and weaknesses

Strengths

- Approach has some merit and could contribute to mitigating market barriers to fuel cell powered residential homes.

Weaknesses

- No real obvious technical barriers being addressed. The project is principally limited to modeling work, which does not seem to justify the budget allocated. The combination of projects here is also quite odd and they are not clearly related.
- The poster did a very poor job of effectively communicating the intent, progress and future plans of the project.
- Tasks II & III are out of scope and should be eliminated.
- Rigorous cost analysis is needed.
- Coordination with building community is necessary.

Specific recommendations and additions or deletions to the work scope

- The investigation of semiconductor materials for application in photoelectrochemical applications does not fit with the rest of the scope of this effort.
- A comprehensive cost analysis of the fuel cell micro-grid strategy is an essential deliverable from this task.
- This task should coordinate closely with the Zero Energy Building activities of DOE's Building Technologies (BT) Program. BT's activities are aimed at significantly lowering the energy load of homes via integrated appliance management, solar applications, improved envelopes, etc. This approach significantly lowers the energy load of a house. Integration of fuel cell and advanced energy management should be looked at in the context of advanced energy home systems not coupled with homes exhibiting conventional energy load profiles.

Technology Validation
Summary of Annual Merit Review Technology Validation Subprogram

Summary of Reviewer Comments on Technology Validation Subprogram:

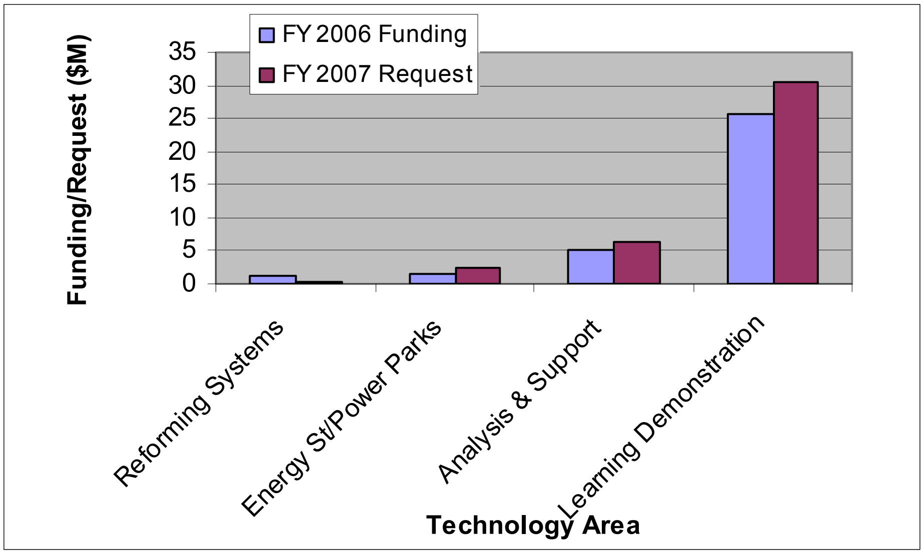
Reviewers considered the Technology Validation effort critical to the President's Hydrogen Fuel Initiative. Reviewers thought highly of the Learning Demonstrations, Natural Gas to Hydrogen and Energy Station projects. The Learning Demonstration project must interface with a wide variety of organizations by design to co-develop hydrogen infrastructure in parallel with hydrogen fuel cell-powered vehicles. Reviewers felt real time data monitoring of vehicle operations is impressive. Vehicle operations in diverse operating environments were considered a real plus. DaimlerChrysler's progress in the development of a maintenance facility in Long Beach, CA. and their use of a potential leaking model to design the facility was a significant contribution. Reviewers indicated that there was an impressive list of technical issues identified and addressed by the project teams. Lessons learned pointed to future areas of research including technical, insurance, user concerns and fueling issues. The reviewers felt larger vehicles would be required in the future to meet DOE targets for range.

The reviewers would like to see more public disclosure. However, they indicated that database development is necessary for technology transfer objectives to be furthered. The NREL Agreement is an outstanding method of facilitating interaction between principal hydrogen economy investigators. This Agreement is critical to validating whether the U.S. light duty fleet and fueling infrastructure are successfully meeting targets. This data allows DOE and OEM's to discuss program progress on an even basis. The public need to understand the progress being made using public money is an important aspect of these programs. Confidence in DOE oversight can be helped very much by this program. DOE's ability to communicate the essence of the data trends publicly will be very important. The program is clearly well thought out and appears to be managed in an organized fashion. Very well planned and executed.

The reviewers indicated that good technical progress was made in the Natural Gas to Hydrogen reformation systems. A new PSA system appeared to be of high value. Power Parks were considered limited by the size of the equipment used and therefore were difficult to apply to actual systems. Scale up factors that had to be applied lacked credibility. The high-temperature Energy Station was recommended for further development. However, the reviewers did not recommend future funding for the Solid Oxide Fuel Cell project. Congressionally directed projects were the four lowest-rated projects in this Subprogram.

Technology Validation Funding by Technology:

The funding portfolio for Technology Validation stresses the continuance for the 5 year Learning Demonstration project as it enters the third year of that effort. Second generation vehicles will be introduced in FY 2007 that will be instrumented to provide information on meeting 2009 fuel cell durability, vehicle range and cold start targets. Natural gas to hydrogen projects will be concluded and an assessment will be made on the status of the technology to meet \$3.00 per gge target for hydrogen production. A high-temperature fuel cell Energy Station will be funded to complete detailed design and enter into fabrication if a Go decision is made. Power Park activities will be continued as part of the Learning Demonstration or completed in FY 2007. A 2^{nd} generation cryo-compressed storage tank will be designed and parts ordered for installation on a vehicle in FY 2008. The 2007 funding profile is subject to Congressional Appropriations.



Majority of Reviewer Comments and Recommendations:

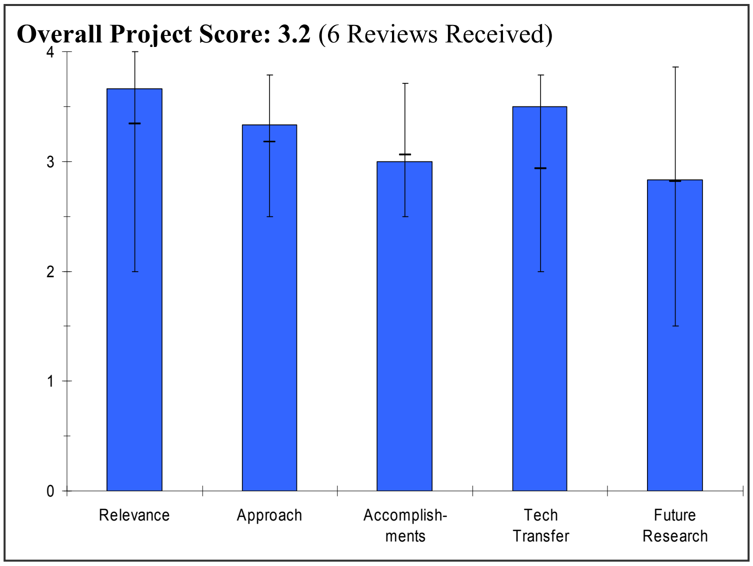
The Reviewer scores for the Technology Validation Subprogram were on average slightly higher or similar with those of other subprograms (the maximum, minimum, and average scores for Technology Validation projects were 3.7, 2.5, and 3.1 respectively). These compare to the overall maximum, minimum, and average project scores of 3.7, 1.4 and 3.0, respectively. The Technology Validation project portfolio includes a mix of projects with several projects nearing their completion date and others like the Learning Demonstration in the second year of a five year program. The major recommendations by the reviewers are presented below for each of the task areas. DOE will act on reviewer recommendations as appropriate for the overall Hydrogen Technology Validation effort.

- **Learning Demonstrations –** These were well-directed projects critical to the support of the President's Hydrogen Fuel Initiative. The NREL Agreement for analysis of the four teams' efforts is critical to validating whether the U.S. light duty fleet and fueling infrastructure are meeting program targets.
- **Natural Gas to Hydrogen Stations –** Useful data on all aspects of refueling station operation. Good technical progress in fuel reforming efficiency, fast-fill testing and fuel dispensing. Little progress made in hydrogen compression technology. New PSA appears to be of high value. Insufficient station operation for complete demonstration.
- **Power Parks –** Good projects to assess utility interest in the program. Limited size of equipment that impacts scale-up factors' credibility.
- **Energy Stations –** Molten Carbonate Fuel Cell project relies on mature technologies. Engineering design should be able to be implemented. Phase 3, detailed design and fabrication, of the project should be done.
- **Storage –** Reasonable plan to install cryo-gas tank on a hydrogen hybrid vehicle. 2nd generation cryo-gas tank hardware suitable for vehicle use. Need to provide design for manufacturing, production and investment.
- **Analyses –** The NREL Agreement is critical to be able to convey information to the public. On the Power Park analyses there was a good understanding of energy peak performance parameters. Strong effort to validate tools using real world systems for both efforts.

Project # TV-01: DTE Energy Hydrogen Technology Park

Rob Bacyinski; DTE Energy

Brief Summary of Project



In this project, DTE Energy will develop and test a working prototype of a hydrogen-based energy station concept that utilizes a combination of renewable and non-renewable power (including on-site solar) with electrolysis and stationary PEM fuel cell technology to take advantage of low-cost power during off-peak hours to generate hydrogen for on-peak power generation and vehicle fueling. Using state-of-the-art hydrogen generation, storage, regeneration and control technologies, the project will evaluate opportunities to reduce overall system cost and maximize performance. The project will also contribute to the development of relevant safety standards and codes required for commercialization of hydrogen-based energy systems.

Question 1: Relevance to overall DOE objectives

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

- Has all elements required for a power park.
- This program has broad education and demonstration capability and is providing essential H_2 delivery infrastructure facility and education in a key vehicle test area.
- The project objectives are right on the mark with key program goals.
- Excellent focus on co-production of electricity, H_2, use of renewable energy and stakeholders.

Question 2: Approach to performing the research and development

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

- The basic approach is too small for meaningful scale up analysis. It is good as far as it goes.
- Use of PV in Detroit is not a good initial choice of renewables.
- Use of multiple demonstrations is a good choice for this initial learning and re-fueling center.
- The project uses multiple energy sources, including renewable.
- Selection of technology options (PV, wind, biomass) are very useful.

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

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

- Many of the accomplishments were last years.
- The results are limited by the size of the equipment. The scale up factors lack credibility. The basic goals are being compromised by looking into such things as pyrolysis of peanuts.
- Installation is complete, that required full safety analysis and permitting; good accomplishment
- Data collection and preparation for reporting, has apparently been ongoing with good quality.
- 1500 Hr PEM life provides interesting comparison to manufacturer data for real world use of this generation of 5 KW PEM units.

- Good progress has been made on all tasks. Pitfalls have been identified and overcome.
- Good understanding of energy peak performance parameters, particularly Hydrogen and energy costs, availability, etc.

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

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

- Good public outreach.
- There is excellent collaboration with other teams such as the fleet demo's as well as the analysis groups.
- The one open house held for public view of project was a huge success. This indicates big pent-up demand for more education and outreach.
- The unacceptable operation of the Hydrogenics (Stuart) electrolyzer has been reasonably well publicized. This is an example of public demos identifying issues, and fixing them.
- Good information for industry stakeholders – needs to be transferred – as lessons learned report.
- The project relies on several different partners, but the coordination and collaboration appear to be very good.
- Use of academia and the national lab are very productive.

Question 5: Approach to and relevance of proposed future research

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

- The activities appear to be a kitchen sink of normal project management and there is nothing proposed that seems to be very productive.
- The reevaluation of a very small electrolyzer is of questionable value. Peanut pyrolysis is an area all its own.
- There is a clear need for this refueler in the area.
- Future work should also address the utility business model(s) and/or analysis of policy options incentives or means to reward the use of utility off-peak power for this application. What would be the impacts to cost and benefits with each policy option?
- The remaining work will lead the project to a good conclusion.
- Identification of a "high value proposition" scenario will be very useful.

Strengths and weaknesses

Strengths
- Frank and open presentation regarding analysis on costs.
- Well organized and executed. Strong technical abilities. Good experience with contemporary industry.
- Well positioned to demonstrate H_2 infrastructure in area that is key to H_2 vehicle options.
- Good electrolyzer refueling station demonstration. With lots of good data and real world information. This facility should be continued to operate to obtain more long term data on performance and degradation.
- The project includes H_2 production from renewable resources.
- The goals are very well aligned with HFCIT program goals for distributed production, vehicle fueling, and power park demonstration.
- The team includes all major stakeholders for an energy park.
- The analytical approach to evaluate "total cost" is very effective.
- The outreach and collaborative activities are excellent.
- Use of "off peak" and "peak" rates to improve the economics.

Weaknesses

- Few publications or broad dissemination of data planned.
- Scale is too small. Getting diverted with peripheral issues.
- Will be difficult to maintain "advanced tech Demos" in a rapidly evolving field. It's possible that this site will become stale in the near future.
- The mix of energy park elements includes a fuel cell, electrolyzer, PV wind, and biomass. How would you use the data collected so far to recommend a preferred use of these elements to promote the energy park concept?

Specific recommendations and additions or deletions to the work scope

- This project should revert to the utility's responsibility since it is deriving self serving information mostly.
- Get bigger equipment all around. Go to at least 1 Megawatt size. Let someone else look at pyrolysis of peanuts.
- Separate refueling from advanced technology demonstration function.
- Suggest deleting any future work on Analyses and recommended course of action for improving PEM fuel cell stack durability- this is better done by key vendors and OEMs – not at university. It is not clear how the pyrolysis part of this project fits into the power park demonstration. Determining the cost of 'cleaning' syngas product to 99.995% purity could be done in other DOE projects dealing with bio-gasification and proceses. This part of the project doesn't seem to fit here.
- May want to consider 10,000 psi fueling in future work.
- I fully agree with the scope as described.

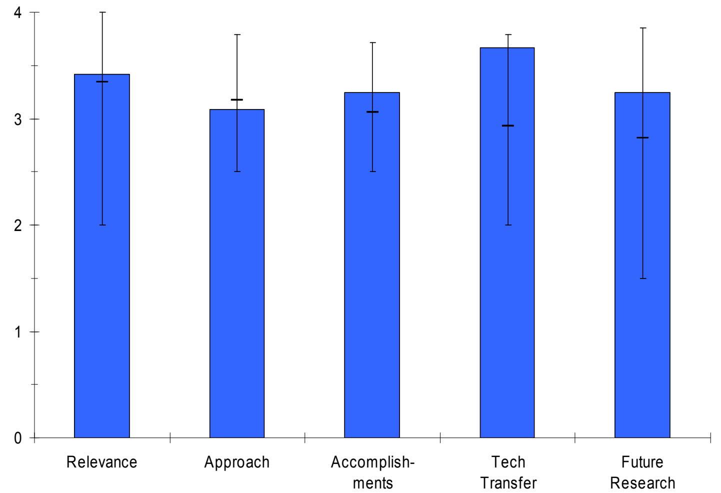
Project # TV-02: Power Parks System Simulation

Andy Lutz; SNL

Brief Summary of Project

Power parks combine power generation co-located with a business, an industrial energy user, or a domestic village. In this project, Sandia National Laboratory is developing a flexible power park system model to simulate distributed power generation in energy systems that use hydrogen as an energy carrier. The project analyzes the performance of demonstration systems for the Technology Validation program. Deliverables include a flexible computational tool to provide simulations of a variety of energy systems that produce hydrogen and independent analysis of system performance, thermodynamic efficiency and cost of hydrogen/electricity.

Overall Project Score: 3.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Develops a potentially valuable analytic tool, validated with field work.
- Still uncertain if modules can be validated.
- Simulation of real systems with validation is necessary for developing essential tools to aid developers and potential technology adopters. Availability of assessment tools is somewhat enabling for President's vision.
- Is a must to have a good analytical model. This can also be used to evaluate progress and results of other efforts.
- Design and optimization of integrated hydrogen systems will be important to facilitating the successful validation and deployment of hydrogen energy systems.

Question 2: Approach to performing the research and development

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

- Based on currently available modeling tools widely used in industry.
- Premise that present set of equipment demonstrations can be scaled to DOE target sizes is questionable; is a difficulty that needs attention.
- Modular Simulink code with Graphic User Interface. Assess against available data. Limitations of use of scaling laws and simplified models acknowledged – can be refined over time as more data become available.
- Good approach but scale factors are a weak point.
- Good effort to improve the user interface to target a wider range of users.
- Good expertise in system development.
- Strong effort to validate tool using real-world systems.

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

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

- State of model is quite good relative to stage of development of components it simulates.
- Seemingly robust conclusion that electricity costs are 1/2 of cost for electrolytic H_2 is interesting.

- Development of Graphic User Interface is significant new capability.
- Flexible, functional model available. Can be run by knowledgeable user, but user must make appropriate input choices (no "sensible" default).
- Good model but still needs to be validated at the large sizes.
- Progress on validating tool has been good, but the team needs to be more consistent with H2A/Programmatic efforts when doing potential cost and impact assessments. Need to benchmark component modules against H2A future case studies. For example, the PI presented conclusions relevant to the ability to meet DOE targets, yet there appears to be little basis behind the scaling assumptions used in the analysis.

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

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

- One of the (possibly) unintended results of this program is that it has promoted communication and comparison of results among projects and reviewers at APS, DTE, HNEI, and theoretical/ academic centers.
- Availability of the tool will promote more communication and comparison in future.
- Broad, appropriate collaborations.
- How will the model be delivered to the industry? Who are the anticipated end-users? Are you working with them on this project to insure their needs are being addressed?
- There is excellent collaboration with other teams such as the fleet demo's as well as the analysis groups.
- The team has done an excellent job of partnering with real-world hydrogen system users.

Question 5: Approach to and relevance of proposed future research

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

- Continue to develop this basic analytical tool.
- The collaboration with Standards project is a smart move.
- Project is nearly complete. Remaining activities appropriate to project closeout.
- Future work regarding collaboration with Stanford's Global Climate & Energy Project to "Implement 2nd-law exergy analysis to measure efficiency in terms of availably energy for a process" – it is not clear how this fits into the current research approach or the relevance?
- The expansion to include peanut pyrolysis and an exergy study is questionable.
- Good plans to further validate the tool, including the addition of technologies.
- Need to better address consistency of cost projections compared to other programmatic efforts.

Strengths and weaknesses

Strengths
- Developing a common, predictive analytic tool(s).
- By using DOE project programs as a source of validating information, this project provides another review opportunity for those other programs, and allows a common comparison platform as well.
- Honest and thoughtful analysis. Good value for the budget. Analysis not propagandized – refreshing.
- PI well qualified and appropriate collaborations in place.
- Well organized and executed. Strong technical abilities.

Weaknesses
- Difficulty of scale-up of presently available components to physical sizes needed to meet DOE future targets.
- Still requires too much specific technical information to be generally useful to technical persons not familiar with all aspects of H_2 and FC's.
- Limited data and immature technology inherently limit validity of early models.
- Who is the model simulation for? Who are the end-users? Technology Transfer plan needs to engage end-uses of the product simulation tool. Is this going to be an industry tool or is it intended for research and universities?
- The reliance of scaling factors with questionable validity is a weakness.

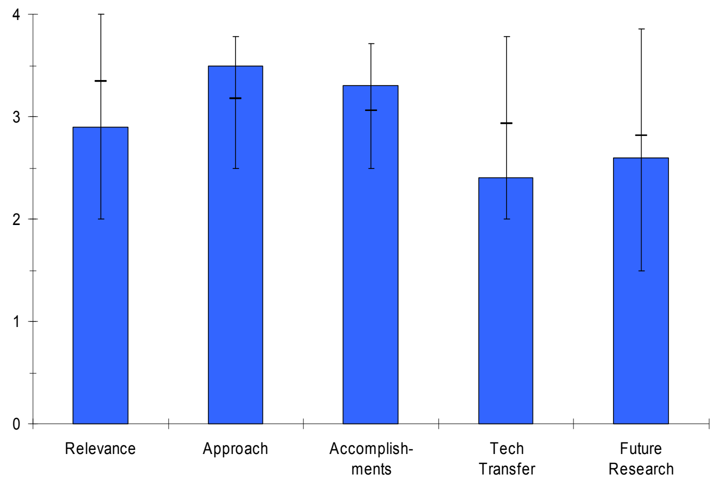
Specific recommendations and additions or deletions to the work scope

- Add sizing and other appropriate screens for input parameters to catch obviously out-of-range input requests.
- Add time of use energy costs; hourly, weekdays/weekends, seasonal.
- Proceed with plan.
- Project nearly complete. Continued refinement of models as technology matures and testing against real projects would be useful.
- The proposed future work: Modeling of fuel cells for electrical power systems and distributed generation power electronics modeling for electrical grid network integration; Testing methods for analyzing electrical performance in relation to the electrochemical reactions; Electro-Impedance Spectroscopy Load and Transient analysis, etc. – does not seem relevant to the H_2 program.
- Add some default cost numbers as baselines for more widespread use.
- The project should be more integrated with the Program's H2A efforts. Need to incorporate recommendations/links so that potential users can be directed to use assumptions that are consistent with other programmatic modeling/cost analysis efforts, when actual costs are not available.

Project # TV-03: Insulated Pressure Vessels for Vehicular Hydrogen Storage

Salvador Aceves; LLNL

Overall Project Score: 3.1 (5 Reviews Received)



Brief Summary of Project

The objective of this Lawrence Livermore National Laboratory project is to demonstrate long range (200 to 500 mile) hydrogen hybrid vehicle with an insulated pressure vessel. Insulated hydrogen pressure vessels have lower cost and safety advantages relative to compressed storage. The second generation insulated pressure vessels built by LLNL filled with LH_2 can meet the 2007 volume and 2010 weight DOE targets (neglecting accessories). Future work will include development of improved insulated pressure vessels that can meet the DOE 2010 volume goal using LH_2.

Question 1: Relevance to overall DOE objectives

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

- Strong relevance to DOE targets for storage volume/weight and vehicle driving range.
- The project goals line up well with program targets for H_2 storage and vehicle driving range. The Gen2 tank meets the 2010 systems weight target.
- Unsure if this technology will meet program objectives even if optimized.
- Also need to look at realistic use scenarios.
- Onboard storage is major challenge. This concept is a science based design option aimed at maximizing H_2 storage. This is insurance against possible public perception of range shortfall for Hydrogen Vehicles.
- Appears costly. Need production estimates to see if costs can be low enough to make this idea viable.

Question 2: Approach to performing the research and development

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

- Approach is good, unsure if technology should be pursued. Should evaluate against novel carriers as well as Gaseous Hydrogen.
- Established good theoretical basis for operational and design envelope.
- Followed with 1st generation proof of concept.
- Followed by 2nd generation hardware suitable for vehicle use.
- Design is directed at specific physical envelope.
- The approach is well thought out and makes sense as a path to meet the project objectives.
- Strong technical concept and rationale.
- Clear and logical rationale for the investigation of insulated pressure vessels.

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

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

- Good progress, just don't think targets will be met and intent of use once targets are met.
- Meets DOE Volume and Weight % goals.
- 2nd generation actually fits in backseat (not a foregone conclusion at outset!).

- The work is progressing well. Each of the tasks appears to be on track.
- Outstanding.
- Second generation insulated pressure vessel has significant improvements in orientation, weight, and volume, with further improvements possible.
- Hydrogen boil-off may be significantly reduced or eliminated.
- Insufficient information provided regarding tank cost and impact on cryogenic hydrogen cost compared to other storage alternatives; combination of potential operational benefits and costs makes it uncertain if the proposed cryotanks have economic viability.

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

This project was rated **2.4** for technology transfer and collaboration.

- Need to coordinate w/ others to determine technology feasibility in relation to others.
- Primarily a component development project.
- The extent of collaboration wasn't clear from the slides.
- High volume production study needed.
- Several presentations and papers during last year.
- Collaborations with several project partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- Opens new option available for specialized situations.
- High risk to possibility that new practical on-board storage option will emerge.
- Discussion of future work is very generic.
- The slides should have included the specific steps remaining to bring the project to a successful conclusion.
- Design for manufacturing, production plan and investment with resulting high volume unit cost is the big missing factor that needs to be addressed.
- Reasonable plan to install cryotank in a long range hydrogen hybrid vehicle to demonstrate thermal endurance and range.

Strengths and weaknesses

Strengths
- Technology development may show increase in liquid H_2 storage capability.
- Builds margin into quantity of H_2 onboard to mitigate holdover image difficulties with electric vehicle range. AND may develop option for range addition to typical gaseous storage for special cases.
- The tank designs are innovative and have been successful in terms of 2007 and 2010 volume and weight targets, respectively.
- Technically strong.
- Strong team with relevant expertise.

Weaknesses
- Liquid H_2 storage has problem with off gassing through lack of continuous use that is not feasible in a vehicular application.
- These tanks are expensive to manufacture.
- Unsure how leaks would be detected.
- No data on use in vehicular application.
- High risk in sense that it is not obvious that this is practical.
- Higher risk and complexity than Compressed H_2.
- The project seems to be open-ended. No mention was made about DOE's go/no go decision on cryo/compressed tanks or what the end result of the project will be. 2015 targets will be difficult to meet.

- Business case needs study – at least a conceptual forecast should be made.
- Insufficient attention to quantifying potential economic viability, including cryotank cost and cryogenic H_2 cost.

Specific recommendations and additions or deletions to the work scope

- Add go/no go with respect to technical validation against novel carriers both technical & economic. If both are not satisfied recommend finishing project w/o further work.
- This is worth pursuing and appropriate to DOE science based programs.
- Cost studies need to be added.
- Perform detailed analysis of system to determine potential economic viability.

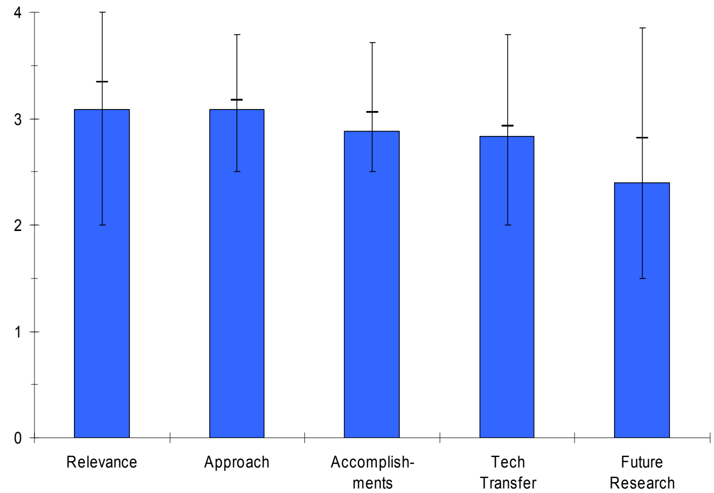
Project # TV-04: Development of a Natural Gas-to-Hydrogen Fueling System

Bill Liss; GTI

Brief Summary of Project

GTI is designing a competitive, fast-fill natural gas-to-hydrogen fueling system with 40-60 kg/day delivery capacity with nominal 350 bar (5075 psig) dispensing. GTI is developing and validating onsite, integrated natural gas-to-hydrogen fueling stations, developing or testing state-of-the-art subsystems that address integration, operation, maintenance, reliability, and safety. Pre-packaged system designs with simple installation requirements are favored. Compact and efficient hydrogen generation technology is an important component of the system.

Overall Project Score: 2.9 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Relevant to DOE objectives for hydrogen production efficiency.
- Important to a primary pathway.
- The project goals are in line with program goals for natural gas to hydrogen reforming.
- This type of project is very useful for "tech validation", but is not enabling for the President's vision when true breakthroughs are required elsewhere in the program.
- Distributed hydrogen reforming and dispensing is a very important part of getting early-on acceptance of hydrogen. This covers the development of such a system in detail.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- Logical plan, good scale.
- The approach appears to have been successful. Components were selected only after thorough consideration of possible options.
- Five generations of fuel processor built and tested. Multiple vendor PSA units assessed, along with some more advanced membrane approaches. Reformer efficiency is a design goal. Simple, robust fill algorithm sought.
- A well thought-out and thorough approach. It covers all aspects of making and dispensing hydrogen and describes both the theoretical and experimental components of the project in detail.
- Well thought out.
- Unclear if the H_2 cost target can be met; insufficient infomation provided.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- Good technical progress in fuel reforming efficiency, fast-fill testing, and fuel dispensing.
- Areas such as hydrogen compression appear to have little progress, and insufficient detail provided on areas such as hydrogen purity.
- Some limited cost data provided for cascade storage, but insufficient cost information provided for other tasks.

- Issued a patent relevant to the project which is good validation.
- The project is nearly complete. The reformer design is on track to meet program targets for reforming, efficiency over all the progress was slow and several barriers were encountered.
- Fairly refined system developed. Reformer efficiency target achieved. Fill algorithm developed and will be made available through non-exclusive licensing at "a relatively nominal cost." Successful project.
- Definite progress, it seems that they have gotten some good results for all their components.
- I would have liked to see a little more detail in some areas such as comparison of data of various purification configurations.
- The answer to my safety question was only partially satisfactory. However, while experience in working with similar experience is certainly valid and welcomed, statements such as that often indicate that there is too much reliance on past experience and not enough focusing on the present task and determining what the safety risks are and how they are mitigated.
- Need to show more detailed results – comparison between tank types, fuel processing, etc. PSA / Fuel purification testing with various systems is extremely beneficial. Would be better to list findings between tested systems.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Insufficient information provided regarding tech transfer and collaborations; however, several partners were involved in the project.
- Good commercialization intent, good connection to the natural gas industry to help build bridges to the infrastructure.
- Co-ordination with Greenfield is good. Co-ordination with others and dissemination of results were not adequately addresses.
- Broad, appropriate collaborations.
- The ability to partner with so many component manufacturers and get comparative data on these components is most desirable.
- Partners seem to be used to supply equipment. Little new work or R&D completed.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- It appears that most the future work is outside the period of the current project; some important work will not be completed, such as long-term operation of the fuel processing system and demonstration of the mobile hydrogen system.
- Close to complete.
- The final steps make sense to bring the project to a logical conclusion.
- Project is essentially complete. Remaining activities appropriate to project closeout.
- They are just about finished with this project. Their plan to use their storage system for demonstrations and working with others toward commercialization appears to be the correct focus.
- No future work given, even finishing of reports with detail data would've been good.

Strengths and weaknesses

Strengths

- Accomplishments made in hydrogen production/fuel processing and fuel dispensing (CHARGEH_2, fill control algorithm, commercial dispenser, and cylinder filling).
- GTI is well placed to pursue these efforts and seems to make valid efforts to get the technology into marketplace.
- The project was successful in developing a complete natural gas to hydrogen fueling system, which is a key goal of the program.

- PI and company well qualified and appropriate collaborations in place.
- Great amount accomplished with existing funding , made good decision to keep project and achieve greatest amount of relevant results. Data can be used by others.
- Very nice well thought-out system. A thorough job.

Weaknesses
- Project is too diverse, and it appears that few accomplishments were completed in some areas.
- Insufficient information was presented in key areas, such as hydrogen purity and system/hydrogen cost.
- None evident.
- Need to show efficiency from an overall system perspective not individual components, using NG w/o CO_2 gas clean-up.
- Not much in the way of weaknesses here. I would like to be assured of the adherence to safety that I mentioned above.

Specific recommendations and additions or deletions to the work scope

- Would like to see additional efforts to get operational data into the knowledge base.
- Project nearly complete. Continued operation, data collection and subsequent system refinement by company will benefit product development.
- Scale should include a data report with pros/cons of various vendor's technologies both technically and economically. Should request recommendations for components improvements, not system improvements; systems seem to have enough evaluation completed.
- Get the system into commercial use!

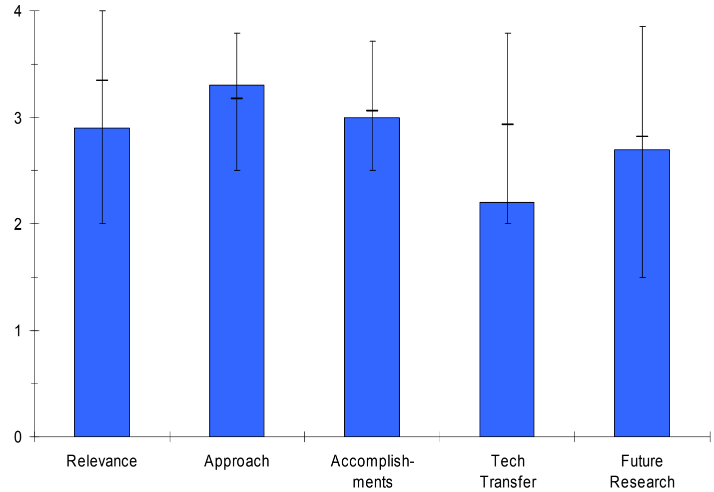
Project # TV-05: Development of a Turnkey H_2 Refueling Station

David Guro; Air Products

Brief Summary of Project

Air Products and Chemicals, Inc. is working on a project to demonstrate the economic and technical viability of a stand-alone, fully integrated hydrogen fueling station based on the reforming of natural gas. Building on the lessons learned from the Las Vegas H_2 Fueling Energy Station project, this project seeks to optimize the system, advance the technology, and lower the cost of hydrogen. The demonstration will be done through the operation of a fueling station at Penn State University with the purpose of obtaining adequate operational data to provide the basis for future commercial fueling stations. The top priority of the fueling station is maintaining safety standards in its design and operation.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- The intellectual property approach of APCI is designed to prioritize the distributed reformer to H_2 pathway. This undermines deployment necessary for the success of DOE's H_2 program.
- Distributed generation makes sense in next (early) infrastructure development.
- The customer's interface experience is vital. Not sure converting natural gas has long term relevance. (i.e. shortage of supply and need to import, and CO_2 production.)
- Relevant to several technical objectives related to storage, refueling infrastructure, O&M, and codes/standards.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Engineering work first rate; however engineering selection very conventional.
- Wonderful.
- Appears to be a well-designed project that should provide useful data on all aspects of refueling station operation.
- Clearly focused on technical barriers.
- What will be the approach to scale-up to the 1500 kg /day system?
- Project seems primarily designed to position Air Products to market its fueling station.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- New PSA system appears of high value.
- Progress seems excellent but need to report issues or any incidents of interest.
- Appears to be a large project budget for the work accomplished.
- Six months of demonstration for the H_2 generator is insufficient, particularly when efficiency optimization will still be underway.
- H2A results indicate potential for meeting H_2 cost target.
- Significant progress has been made in all aspects of the station.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.2** for technology transfer and collaboration.

- APCI tightly controls this project.
- Only apparent interaction was with Penn State.
- Stronger interface with current fuel distributors and sellers is needed.
- Collaboration with Penn State, although specific contributions from Penn State were not clearly described other than providing site.
- Few publications or presentations during last year.
- 1) Great collaboration with a state-run academic institution – Penn State University (slide 4). 2) Please explain Penn's activities relative to project in addition to involvement in fabricating and installing the unit (slide 4) – Any additional shared learning and technology transfer? 3) Please report related filed patents. 4) Please name the catalyst supplier (slide 12) – they seem to be a key collaborator. 5) Excellent technology transfer of a DOE tool to your research when you ran the H2A model for a cost comparison (slide 33) – results reproducible for others. 6) You made H_2 refilling station compatible with existing CNG filling site – very useful.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Vital to continue.
- Insufficient station operation time for complete demonstration.
- Continue to demonstrate performance, durability and reliability. Have the critical cost factors been identified which are required to achieve the $3 per kg goal? Does any of the future work address these critical factors?

Strengths and weaknesses

Strengths
- Competent and safe implementation of proprietary design.
- Excellent chemical engineering by a credible team.
- Looks at real world customer interface.
- A well-designed project that addresses all key areas of a hydrogen fueling station.
- Has potential to meet DOE H_2 cost target.

Weaknesses
- Task seemed very similar to earlier Air Products funded (DOE) efforts continual selection process to select components seems unnecessary.
- Natural gas hasn't a long term future as H_2 source.
- Insufficient time allowed for complete demonstration of system operation, reliability, maintenance, H_2 purity, etc.
- I would have like to seen more information on the Economics. What does H2A estimate for this scale of a system? It would have been helpful to see the base case economics and Air Products estimate for the 1500 kg/day system vs. H2A Model; Also it would have been helpful to see where the key cost reductions are going to come from in going from $ 13 per kg to $ 3 per kg. It would also have been useful to show the sensivities to power and natural gas costs to the overall economics. For example power costs of 8 cents per kWh were used – future electric rates may be 12-18 cents. Natural gas costs at the LDC may be on the order of $10-12 per MMBtu.

Specific recommendations and additions or deletions to the work scope

- Nothing at this late point in program.
- Extend demonstration time to at least one year to obtain maximum benefit from this project.
- Are there any issues with siting and installing such refueling systems within cities and communities? What lessons learned are being documented to enable future H_2 refueling stations to be permitted within communities?

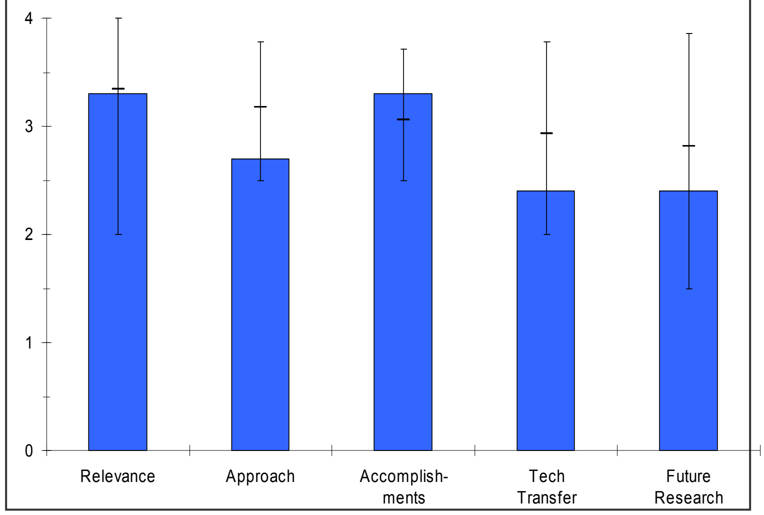
Project # TV-06: Validation of an Integrated Hydrogen Energy Station

Greg Keenan; Air Products

Brief Summary of Project

Air Products and Chemicals, Inc. is conducting a project to demonstrate the technical and economic viability of a hydrogen energy station using a high-temperature fuel cell to produce hydrogen and electricity. A total system design and engineering development effort will be completed with the goal to economically recover hydrogen from the anode of a high temperature fuel cell. The project will conclude in a year long demonstratioin of the system at a suitable site. Safety is the top priority in the system design and operation.

Overall Project Score: 3.0 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Interesting and some what novel for US energy development.
- Captures development opportunities for several key technologies needed for stationary H_2 infrastructure, while enabling H_2 refueling for transportation.
- Identifying near-term opportunities to drive hydrogen infrastructure will be important to enabling the transition to a hydrogen economy.
- We are running out of natural gas already – this makes no sense.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- Sound engineering.
- The 4 phase approach is a good, conservative design progression appropriate to this complex integration.
- It would have been helpful to see more detail on system performance. For example anticipated power (kWe or MW) size of the MCFC unit. The net AC power export; the net H_2 export. The overall efficiency of 49% seems high given that a MCFC unit on natural gas alone (with out H_2 export) is about 44-45%. It is also difficult to review the economics. Does the cost of H_2 reflect the capital cost of the MCFC unit.
- Good phased approach, including go/no-go decisions.
- Using a commercially-available MCFC was a good choice for validating the concept. This will serve as a good benchmark for when SOFC has matured.
- I'm sure it will work but it's not worth the trip.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Only an engineering design, but not a high risk design. Looks well done.
- Work in progress is on schedule
- Analysis led to selection of PSA as separation technology to use.

- PSA turns out to give much better H_2 recovery than thought at much lower pressure differential; This is the type of new discovery that can occur when we push into new operational regimes, in this case recovery of H_2 from dilute stream.
- Significant improvement in hydrogen recovery and system throughput compared to phase 1 projections.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.4** for technology transfer and collaboration.

- While this has led to good communication between FCE and APCI, there is no apparent additional collaboration or information available.
- Partnership development will be critical during the next two phases (site selection and implementation).
- Seems mostly self serving.
- A closed shop.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- Obviously phase 3 needs to be done.
- What is "unexpected" effect discovered in design of Pressure Adsorption Swing in this case?
- Is additional cooling from excess Hydrogen production in FC potentially useful in SOFC's as well?
- Need to clearly identify criteria for the 12 month operating period. Similarly, site selection will be important for determining the potential to replicate this type of system elsewhere (i.e. Lessons learned for permitting, community outreach, system configuration, demand profiles, etc.).

Strengths and weaknesses

Strengths
- Technically sound.
- Integrated FC/H_2 recovery provides opportunity to utilize internal reformation step to provide product for electricity production as well as H_2 Production.
- Synergistic effects may prove more valuable than initial latchup would have indicated; e.g., supplements FC cooling, work with dilute H_2 stream has apparently led to new discovery in PSA design.
- This project relies on essentially mature technologies which increases the likelihood that it will be a validation of an integrated hydrogen and electricity co-generation system that could be replicated to drive hydrogen infrastructure development during the transition.
- This is a very strong team with a good understanding of integration issues, hydrogen safety, and infrastructure needs.

Weaknesses
- Needs alternative to use of natural gas.
- Risk involved in this sort of combining of flowsheet. The FCE unit is not being re-designed to accommodate this new capability, so possibility that it will perform as good or better than without the H_2 scavenging is a risk.
- I would have like to seen more information on the economics. The total capital cost of the entire package installed; the value or cost of electricity; the cost of hydrogen production in a life cycle analysis. For this to be a viable co-production solution; the MCFC unit has to be a compelling and competitive distributed generator production electrical power at better than grid prices – which are in the 8-12 cents per kwh range.
- As the project progresses to the next phases, emphasis will need to be placed on understanding demand profiles and how this may impact the system optimization.

Specific recommendations and additions or deletions to the work scope

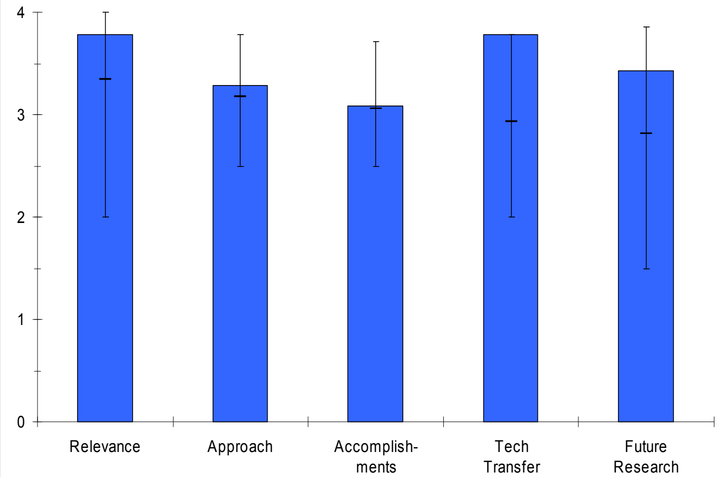
- Need to show more economic analysis to support this approach. To be able to compare with distributed SMR or distributed electrolysis.
- Need to revise economic parameters to match up with H2A standards.
- Need to be clear (on charts) that cost projections are for a plant-gate hydrogen cost, not for a delivered hydrogen cost.
- Eliminate natural gas use.

Project # TV-07: Hydrogen Vehicle and Infrastructure Demonstration and Validation

Roz Sell; General Motors

Brief Summary of Project

Overall Project Score: 3.4 (7 Reviews Received)



General Motors and energy partner Shell Hydrogen are deploying a system of hydrogen fuel cell vehicles integrated with a hydrogen refueling infrastructure to operate under real world conditions to: 1) Demonstrate progressive generations of fuel cell system technology; 2) Demonstrate multiple approaches to hydrogen generation and delivery for vehicle refueling; and collect and report operating data. This project will demonstrate two generations of fuel cell technology deploying forty fuel cell vehicles fueled with hydrogen from stations in five locations.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Program targets DOE technology validation objectives including education and outreach.
- Totally relevant.
- The presence of all of the Vehicle Demonstrations projects is critical to the path to the hydrogen economy.
- Outstanding core goals.
- The project objectives are right in line with program goals. The project should remain a high funding priority.
- Significant advances in vehicle technologies will be imperative for realizing the potential of hydrogen to meet the transportation needs. A critical element to achieving this will be experience and insights gained through operation of these vehicles in variable climates and under variable operating conditions.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Need to present more information in this area. For example, what are the lessons learned in fueling and has the nozzle and associated filling apparatus performed adequately? Have hydrogen sensors operated properly? Has hydrogen flaring created any issues? Are you using a database to report lessons learned?
- More on actions to resolve issues would be desirable.
- It incorporates all that is needed to demonstrate hydrogen fuel cell vehicles and the infrastructure to support them. The presence of the fueling facility in DC lends an extra appeal for this project in its visibility. I don't know if 40 vehicles are necessary.
- Appears to be well thought out but as it is early in the implementation, it is difficult to determine focus.
- The approach is solid and will likely lead to a successful project. Hydrogen generation technologies were not discussed.
- It was impressive that they have 40 fuel cell vehicles in the program but only 8 have the data collection equipment on the road today.
- Testing of multiple on-board storage options is good.
- More emphasis needs to be placed on ensuring that operating conditions and cycles will have sufficient variability to ensure that data gathering and lessons learned are sufficiently robust.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- Development of automated systems such as that employed to track fueling time is progress toward validating technical targets.
- Good start.
- Seems to be doing well; we don't have the individual technical data, of course, but GM seemed less forthcoming than the other projects. The data collection methodology and progress in C&S and training seems fine.
- Good "lessons learned" but it is still very early in the program.
- Vehicle deployment seems to be on track. Site selection is ongoing, but seems to be lagging behind the vehicle deployment. Maintenance and training is progressing well.
- It's very impressive to see how the Benning Road station is used as a learning facility. Their local outreach is outstanding. They have gone beyond what is required.
- Only one fueling station is currently operational and site selection for other fueling and maintenance facilities remains underway. The goal for this project is to have 5 fueling stations. Can this goal be met within the timeframe of the project?
- Good emphasis on community outreach and first responder training at the Benning Road station.
- Although it was accepted that data reporting would be handled under the NREL presentation, it would have been appropriate to have some discussion on vehicle and infrastructure performance. Have there been any issues? Is there any general reading?

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- Collaboration with maintenance and training personnel is critical. This concept carried forward could set the stage for the creation of high skilled labor in the U.S. Collaboration with urban planners is excellent. Zoning officials and local planning commissions and boards of county supervisors would also be worthwhile groups to target for collaboration and outreach.
- Seems very well coordinated.
- Excellent set of partners representing vehicle, energy, government, maintenance, data and regulations industries.
- Good communications with other team members. Good use of visitor center at refueling center. Good coordination regarding C&Ss.
- Collaboration with station operators and other stakeholders appears to be well coordinated.
- She identified the data validation requirements but provided little results. She explained the complexity of data collection, including the engineering effort on the vehicles.
- Good emphasis on partnership development and on gathering lessons learned from experiences to date.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- Although there was insufficient information provided to determine the strategy for future work, the scope seems adequate.
- Good coverage.
- Looks good. NYC will be another high visibility locale.
- Very broad program which limits focus. Slow expansion of user base. Limited supplies of hydrogen to refuelers.
- The plan for future work is comprehensive and aimed at meeting the project goals.
- They will establish 2 more refueling stations by 2006. She clearly explained the lessons learned of establishing the infrastructure.

- Presentation would benefit from showing a multi-year timeline with key milestones. In particular, firm deadlines should be established for fueling station site selections, designs, construction, and operability, as well as delivery of second generation vehicles.
- More emphasis should be placed on validating hydrogen infrastructure. Targets need to be set for numbers of vehicles fueled, hydrogen deliveries made to the station (or hydrogen produced if on-site hydrogen production is to be included), storage and dispensing cycles, etc.

Strengths and weaknesses

Strengths
- Very good set of lessons learned. Especially like the non-self serving cadre of hydrogen experts to talk safety issues to localities.
- I really like the lessons learned, especially the call for third party safety experts.
- Good team cooperation. Good vehicle support. Good refueling station experiences.
- The project includes strong elements of safety and community outreach.
- They provide good explanation of infrastructure lessons learned. They are working on data enhancement. They provide good explanation on the learning part of the project.
- This is a good partnership with a strong emphasis on safety and on codes and standards development.
- Multiple vehicles in multiple locations should lead to valuable insights into technology status and RD&D needs.

Weaknesses
- Need more detailed plans on failure response and resolution.
- None really, just that GM appeared less forthcoming than the others.
- Very broad based approach. Slow start and few vehicles so far.
- Production and delivery options were not adequately addressed. Site selection seems to be behind schedule. The only operating stations are outside the project scope.
- They need to explain how the data enhancements will be made and when the results will be provided and systematized?
- Infrastructure development appears to be lagging. Decisions need to be made on site-selection and on hydrogen delivery options that will be employed if the benefits of the infrastructure validation are to be realized.
- PI should be able to report more on the general performance and operability of the vehicles and the infrastructure without having to go into the detailed data.

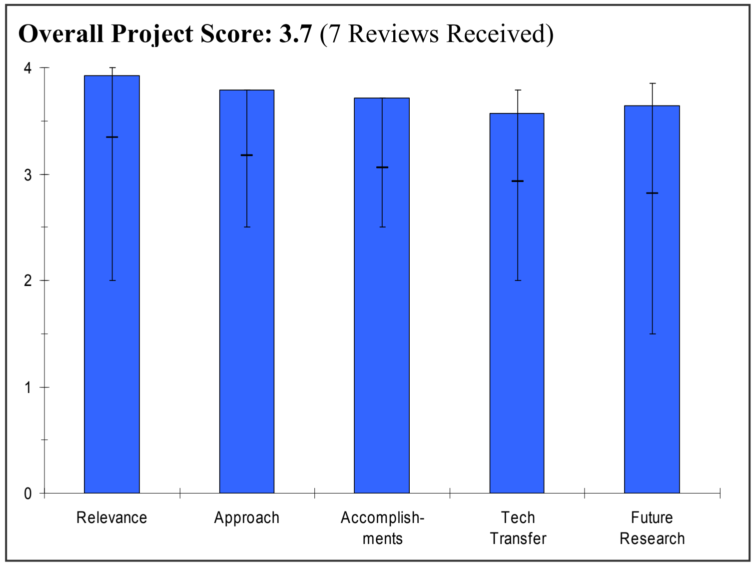
Specific recommendations and additions or deletions to the work scope

- More about customer self fueling experience is needed.
- The good work being presented by this and the other vehicle demonstration projects are huge boosts for the acceptance of hydrogen.

Project # TV-08: Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project

Klaus BonHoff; DaimlerChrysler

Brief Summary of Project



In this project, DaimlerChrysler deployed 30 Gen I fuel cell vehicles in three ecosystems to validate current status of: 1) Durability of fuel cell stack and system; 2) Range of operation with compressed H_2; 3) Cost of H_2 from various production methods; and 4) Performance degradation over life via dynamometer and on-road testing. All 30 vehicles were equipped with a customer friendly Fleet Data Acquisition (FDA) system that will automatically collect statistically relevant data for submission to NREL as well as engineer analysis for technology improvement. As the energy partner of the project, DTE Energy opened the first public hydrogen refueling station in Southfield, Michigan with BP. DaimlerChrysler, BP and DTE Energy will also test emerging technology with the potential to meet DOE hydrogen cost target while evaluating emerging and renewable technologies to produce hydrogen and co-generation technologies to produce hydrogen and electricity. Data will be provided from Gen II vehicles under the same operating conditions as Gen I vehicles to compare technology maturity over the project duration.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.9** for its relevance to DOE objectives.

- Highly appropriate to get real world experiences.
- The focus on collecting data to validate critical performance measures was obvious in the presentation. For example, describing the range of ecosystems in which the vehicle is operated was good, however in the future briefly list those environments.
- He clearly outlined the goals and objectives for the project and they are the same as DOE's
- The presence of all of the Vehicle Demonstrations projects are critical to the path to the hydrogen economy.
- Have elements critical to success.
- Project goals are aligned with HFCIT programs goals for the learning demonstration.
- The project should remain a high funding priority.
- Significant advances in vehicle and infrastructure technologies will be essential for realizing the potential of hydrogen to meet the transportation needs. A critical element to achieving this will be experience and insights gained through operation of these vehicles in variable climates and under variable operating conditions.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- Good, but needs some feedback on current status compared to objective.
- The approach taken to accumulate vehicle mileage is good as is the strategy to ensure the approach is optimized (getting them to drive as much as possible). The Work on nozzle/receptacle communication is critical.
- They have a well thought out plan to address the barrier.
- The Michigan/California combination covers the weather extremities. I also like the customer perception and project crisis management plans.

- Seems to be self-focused but it is still early in the program.
- The infrastructure approach includes multiple hydrogen technologies. Safety, codes and standards, and maintenance are all included in the scope.
- Breadth of regional and drive cycle variability should yield valuable insight into fuel cell vehicle performance.
- CFD modeling a good approach for facility design.
- Good emphasis on deploying second generation vehicles into comparable operating conditions so that performance improvements can be verified.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.7** based on accomplishments.

- Diverse operating environments are a strong plus for program. Details on things gone wrong and lessons learned should be reported.
- The addition of the Long Beach facility vehicle bays, Computational Fluid Dynamics to model potential leaking contribute to important accomplishments. Good presentation of software developments to improve vehicle reliability.
- All 30 vehicles in customer's hands are equipped with data collection.
- 35000 miles driven.
- They have done some work on safety for buildings in case of an H_2 leak.
- I was happy to see some data – even if it was just cumulative and daily driving miles. It opened up a very interesting anomaly on weekly driving miles differences.
- I would like to learn more about the software to improve reliability.
- Very rapid deployment of 30 vehicles. Good maintenance facilities. Good feedback from operational experiences to improve reliability i.e. software modifications.
- Vehicle deployment is outstanding (30 vehicles and 35000 plus miles).
- A comprehensive service facility has been completed.
- Infrastructure development seems to be lagging behind vehicle deployment.
- Project has made significant progress in the deployment of vehicles, including the vehicle miles traveled.
- Showed clear design improvements that have resulted from the experiences gained through the project.
- Even though some of the fueling stations have been delayed, clear progress has been made on site selection and hydrogen delivery/generation choice.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- Internal collaboration with employees is critical. Also, coordination with DOE education and outreach good. Collaboration with DTE and NextEnergy is a good match and leverages existing research efforts.
- Customer profiling work is valuable
- Status, technical data provided for each station
- This was very comprehensive
- A strong team that covers all the key areas.
- No public educational accomplishments or plans stated. Good collaboration with team and DOE.
- Collaboration with fleet partners has been good.
- Strong partnership development efforts on both the vehicle and infrastructure side.
- Ability to meet timelines for permitting of stations demonstrates good attention to stakeholder development at individual locations.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- Suggest large cars and SUV's in future.
- Unclear what the strategy is behind future work and how future work will further DOE targets. In the area of customer perception however, the need is clear and the proposed work critical.
- The customer acceptance and perception study could be valuable
- Good set of future work. It seems that they are moving right along.
- I really like the customer perception and acceptance study that will be done.
- Future work stated as expansion of existing implementation.
- The plan for future work is good and will lead to meeting project goals successfully.
- Would benefit from a multi-year timeline identifying critical milestones for vehicles and infrastructure.

Strengths and weaknesses

Strengths
- Well planned and executed.
- Over 35,000 miles driven.
- Good explanation of data collection systems
- Key findings on safety were clearly explained
- Very good set of training programs.
- Good vehicle technical team. Good operational experience.
- Vehicle deployment is outstanding.
- Good balanced emphasis on both vehicle and infrastructure validation.
- Breadth of hydrogen delivery options and site locations should yield valuable insights into technology status and future needs.
- Demonstrated that the experience gained through this project is directly feeding back into vehicle and infrastructure performance improvements.

Weaknesses
- More details on status of range, cost, and durability needed.
- None.
- Lacks educational components.
- H_2 production and delivery options were not adequately addressed.
- Critical performance milestones should be set for the hydrogen infrastructure, such as target number of fills, volumes of hydrogen delivered/produced, storage/dispensing cycles, etc.

Specific recommendations and additions or deletions to the work scope

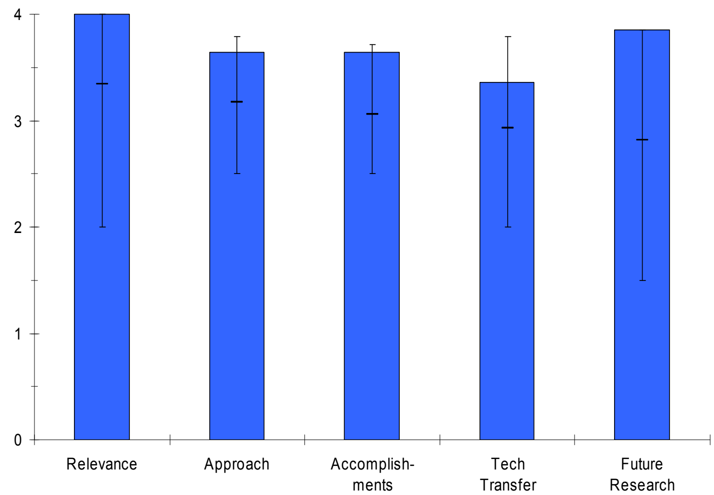
- Larger vehicles in future – to be more main stream for typical U.S. user.
- Instead of simply stating that "hydrogen fueling stations were designed, constructed and operated add in parenthesis the type of hydrogen station…i.e. (SMR, Mobile Refueler, Liquid).
- Excellent work!
- The good work being presented by this and the other vehicle demonstration projects are huge boosts for the acceptance of hydrogen.
- Add visitor centers and some public PR.

Project # TV-09: Hydrogen Fuel Cell Vehicle & Infrastructure Demonstration Program Review

Greg Frenette; Ford

Brief Summary of Project

Overall Project Score: 3.7 (7 Reviews Received)



To date in this project, Ford has placed 18 Gen I hydrogen fueled vehicles in fleet user service in three varied climatic regions to demonstrate the efficiency, reliability and durability of the fuel cell power concept, and to validate the concepts through the collection of real world data. In parallel, hydrogen fueling stations have been sited (City of Taylor, MI, Jamestown, FL and Sacramento, CA Airport) to establish an initial hydrogen infrastructure, demonstrate alternative hydrogen production concepts, and evaluate production technologies for cost effectiveness. Emerging technologies in vehicle and hydrogen infrastructure will be validated in separate, advanced engineering vehicles (Gen II) and fuel cell system designs that demonstrate improved functionality, range, durability, economy, weight and cost.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- Outstanding match between DOE and collaborative objectives (especially when compared to other DOE research areas).
- The technical data and user perception information is critical to understand the consumer acceptance of hydrogen as a fuel.
- The presence of all of the Vehicle Demonstrations projects are critical to the path to the hydrogen economy.
- This is the core of the effort.
- The project goals are aligned with the HFCIT program goals for the learning demonstration. The project should remain a high funding priority.
- Just about perfect.
- Significant advances in vehicle and infrastructure technologies will be essential for realizing the potential of hydrogen to meet the transportation needs. A critical element to achieving this will be experience and insights gained through operation of these vehicles in variable climates and under variable operating conditions.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- Outstanding approach to vehicle deployment including the completion of the maintenance and training program. Quick assessment of vehicle technology shortfalls and rapid corrective measures taken – i.e. investigation of systems module valve.
- An impressive list of technical issues are being identified and addressed by the project team.
- Like the others, a good vehicle/infrastructure approach. Adding an Orlando station provides a more tropical region as well. Perhaps a more economical use of vehicles.
- Emphasis seems to be on vehicle data and analysis and infrastructure operation at the expense of Codes & Standards and public awareness education.
- Community outreach was not adequately addressed in the approach.
- Terrific plan and execution to date.

- Good phased approach for implementing infrastructure; good that the opportunity for validating more advanced delivery options (namely on-site generation) is not sacrificed because of the need for fueling infrastructure now.
- Interesting approach to both validate vehicles in the field under real-world conditions, while also conducting controlled durability/performance dyno-testing. Being able to tie the lessons from the two studies together will be important.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- Description of the production method helps the reviewer determine the actual extent of progress. Excellent description about the lessons learned in miles accumulated. While 125,000 were targeted, 78,000 achieved. The rationale behind falling short was adequately described.
- The project has a good data gathering system and appears to use this data well. The project manager appears to have a well thought out plan of what they are doing.
- Very good assessment of difficulty of achieving their mileage goal. Nevertheless they accumulated more mileage than others. Very good discussion of lessons learned.
- Good number of vehicle miles (78,000 miles) experienced. Good feedback to vehicle design. Have restricted users in their use which may inhibit experience. It was stated that maintenance and training activities were "complete" for the project. This is a continuing thing and should not be considered finished.
- Vehicle deployment has been very good (18 vehicles) Significant training and maintenance effort has been completed.
- Two 2nd generation vehicles have already been built.
- Results and lessons learned are well reported here.
- Would like to see presentations at 6 to 10 opportunities over past 2 years (i.e.: SAE, H_2 Conference, other DOE).
- Performance improvements as a result of testing and lessons learned are evident.
- Good progress on infrastructure development and planning.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- The transfer of fuel cell vehicles from the laboratory to the field is progressing rapidly.
- Collaboration with others was not apparent in the presentation.
- Seems to be more of a collaborative effort with data coming form DOE and non-DOE resources.
- Good team cooperation. Should expand to have more public disclosure/education.
- Good coordination with fleet operations and other stakeholders.
- Good collaboration among partners and suppliers.
- Good focus on emergency response training and community engagement events.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.9** for proposed future work.

- Description of the next phase was helpful and the specific description of work proposed in phases outstanding. The lessons learned also pointed toward future areas of research.
- It was clear from the presentation that the project team is responding to issues as they arise. There appears to be good effort to continuously improve.
- Appears to be right on track, following this year's work.
- Should consider more vehicles. The "lessons learned" were limited to vehicle experiences.
- The plan for future work is solid and will lead to a successful demonstration.
- Well planned.
- Good focus on specific targets. Would have liked to see these presented for future years, as well.

Strengths and weaknesses

Strengths
- Intellectually honest in assessment of progress.
- The gathering of real world data on almost 20 vehicles is a great strength. A good number of lessons learned in a wide variety of areas – including technical issues, insurance issues, user concerns, and fueling issues.
- They seemed to be a little more forthcoming with issues than some of the other vehicle projects.
- Good vehicle useage. Good feedback into vehicle design (i.e. software improvements).
- Vehicle deployment has been very good. Safety considerations have been given importance on the project.
- Outstanding in general.
- The PI did a very good job of reporting a breadth of lessons learned from the project. The presentation was well-balanced on both the vehicle and infrastructure sides.
- Clearly a well-qualified and well-integrated team.

Weaknesses
- Community engagement events are listed as technical accomplishments and perhaps should fall under collaboration
- Slow ramp-up of vehicles and operational experience on them. The support infrastructure (fueling) has contributed to this and is an issue as well.
- Focused too much on vehicles at the expense of other objectives such as public awareness.
- H_2 production and delivery options were not adequately addressed.
- Need to share results with the world!
- Need to better explain how correlations will be drawn between the vehicles in on-road service compared to the advanced vehicles undergoing controlled dyno-testing.

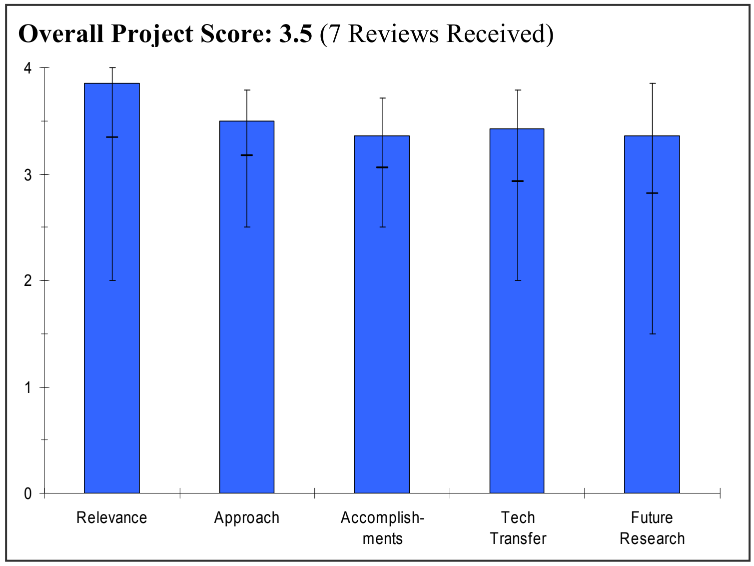
Specific recommendations and additions or deletions to the work scope

- Leave a few more minutes for questions from reviewers, however perhaps the additional information provided contributed to the outstanding score this reviewer provided.
- The good work being presented by this and the other vehicle demonstration projects are huge boosts for the acceptance of hydrogen.
- Suggest that visitor areas be established with refueling sites. Suggest that maintenance and training scope be continuing.

Project # TV-10: Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project

Dan Casey; Chevron

Brief Summary of Project



Chevron Corp., Hyundai-Kia Motor Co. and UTC Power are conducting a five-year demonstration and validation project designed to showcase how fuel cell vehicles (FCVs) and hydrogen infrastructure can be designed to work together to fuel vehicles of the future. The primary goal of this project is to demonstrate up to six hydrogen energy stations (primarily in Southern California, with one site elsewhere to test cold climate conditions) and up to 32 FCVs as well as inform key audiences about hydrogen as a potential vehicle fuel. In addition, important safety and legal codes and standards for hydrogen refueling technologies will be developed in conjunction with the federal government and other authorities. Hyundai will provide a fleet of up to 32 vehicles, powered by UTC power plants. Hydrogen at the refueling stations will be generated using different types of natural gas reformer technologies and electrolysis. Other collaborators include Southern California Edison, Hyundai KIA America Technical Center, Inc., Alameda Contra Costa Transit and Tank Automotive Research, Development and Engineering Center, who will serve as vehicle fleet operators and site hosts for hydrogen fueling and power generation stations.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.9** for its relevance to DOE objectives.

- The relevance of work could be explained better to relate to the DOE multi-year plan.
- Very relevant.
- The demonstration of different methods of on-site hydrogen production was not lost on this team. Description of production details outstanding and allows reviewers to assess relevance beyond vehicle data collection.
- The information gathered on this and other similar projects is critical to understand the consumer acceptance and viability of hydrogen as a fuel.
- The presence of all of the Vehicle Demonstrations projects are critical to the path to the hydrogen economy.
- This is core to the effort.
- Significant advances in vehicle and infrastructure technologies will be essential for realizing the potential of hydrogen to meet the transportation needs. A critical element to achieving this will be experience and insights gained through operation of these vehicles in variable climates and under variable operating conditions.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- Good explanations of 32 vehicles deployed and infrastructure but relevance to barriers need more explanation.
- Very practical.
- Very clearly laid out in each area. Clearly stated rationale for geographic site selections.
- The presentation concentrated on operations and not as much of development. This is the nature of the project. The project appears to have a well thought out master plan.
- Their approach seems slightly more infrastructure based as they are emphasizing different kinds of stations.

- Too much emphasis on infrastructure and not enough detail about vehicles presented.
- Good activities related to climatic impacts on vehicle performance. Would like to see multiple data points, under extreme conditions, as the vehicles age.
- Good focus on complex infrastructure.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- Good explanation of safety record & stations.
- Good explanation of vehicle testing and monitoring systems.
- Seems a bit slow.
- Need report on things-gone-wrong and lessons learned.
- Should share story with other groups (i.e. SAE, etc.)
- Somewhat vague description of actual accomplishments. For example "worked on safety plan" doesn't mean a great deal. However, specific data was provided on bus refueling although teams had agreed to allow NREL to present detailed data. Good summary of percent of target reached.
- The project has shown significant progress. The presentation did not elaborate on details concerning technical hurdles.
- I like the fact that their first set of results feature safety plans. Some good testing under extreme conditions shown. Making some good progress in addressing vehicle range.
- Slow start on the number of vehicles deployed. Good variety of infrastructure examples.
- Accumulating good experience from vehicle fueling infrastructure.
- Would like to have seen more reported on lessons learned and improvements made as a result of conducting the learning demonstration.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Great use of transit companies (i.e. AC transit).
- Somewhat limited.
- Unclear what major collaborations are on this effort, although Chevron's efforts to collaborate across the country in the emerging hydrogen economy are increasing at an impressive rate! For example, sponsoring the NHA Power Park Student Design competition, funding the NREL renewable liquids feedstock study, chairing DOE's hydrogen production technical team
- There appears to be substantial collaboration with the direct participants. The presentation did not elaborate on technology transfer or collaboration beyond there.
- Another good team.
- Good coordination with other team members. Also has involved other projects such as bus demonstration projects.
- Good collaboration among partners.
- Appears to be limited community interactions beyond first responder training.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- They presented clear vision for the project which builds on past progress.
- Not as broad as it could be.
- Somewhat confusing where future work was not sponsored by DOE and the strategy to be employed for conducting future work.
- The main goal of the project is to gather data and experience. A system to use this information for continuous improvement could be better described in future presentations.

- Seem that they will be adding more stations of different configurations and more vehicles. Looks good.
- Good on-site demonstrations. Should expand vehicle participation.
- Would benefit from multi-year milestones and targets.

Strengths and weaknesses

Strengths
- Real time monitoring of vehicle operations is impressive.
- Thorough, well staffed, and well planned – it appears.
- Listed the types of hydrogen production employed rather than making a generic statement about the provision of hydrogen. Excellent response to prior year reviewer comments.
- The program is demonstrating multiple fuel production methods to show capability and performance. A good project roadmap is in place and progress appears to be good. The program has a high number of vehicles with which to gather data.
- Good description of safety at stations.
- Good extreme condition testing – high altitude and high temperature conditions.
- Collaboration with other projects, i.e. AC Transit. Good infrastructure experiences.
- Good experience being gained with advanced on-site hydrogen generators.
- Good breadth of testing of vehicles.

Weaknesses
- Need to show the relevance of DOE targets to the project better.
- More sharing of results (expected and unexpected) needed.
- The presentation appears to have a strong focus on fuel infrastructure, to the detriment vehicle information.
- Slow start.
- Unclear how results are feeding back into advanced system development.
- Multi-point, multi-vehicle test at high altitude and cold/hot temperature would be required to identify statistically significant trends in vehicle performance/lifetime as a result of climatic variability.

Specific recommendations and additions or deletions to the work scope

- Future plans should reflect results publicly reported and corrective actions.
- The good work being presented by this and the other vehicle demonstration projects are huge boosts for the acceptance of hydrogen.
- Expand use of vehicles.

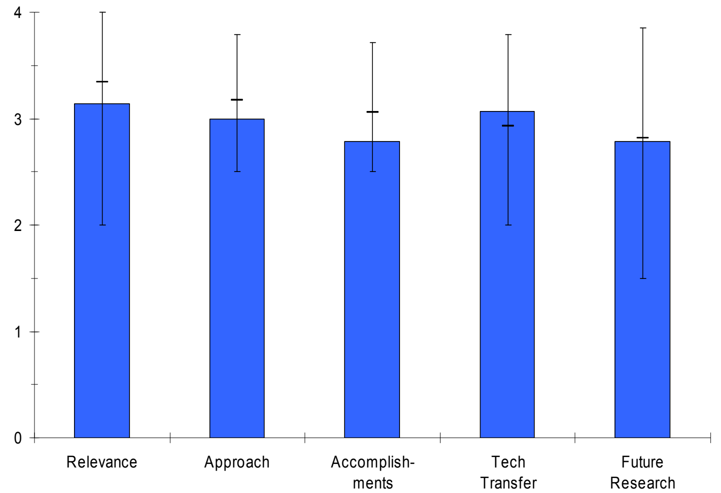
Project # TV-11: California Hydrogen Infrastructure Project

Mark Pedersen; Air Products

Brief Summary of Project

This project is focused on demonstrating a cost effective hydrogen infrastructure model in California for possible nationwide implementation. It includes the design, construction and operation of seven hydrogen fueling stations; collection and reporting of operational data; documentation of permitting requirements and experiences; and validation of expected performance, cost, reliability, maintenance, and environmental impacts. This project will also implement a variety of new technologies with the objective of lowering the cost of delivered hydrogen.

Overall Project Score: 2.9 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Lower cost of delivered hydrogen by using existing infrastructure during the transition will help the Program achieve its goals. Focus on installation of infrastructure in a particular region, however may be premature as critical research needs have yet to be met that may determine transitional infrastructure.
- Not clear what is novel, what is to be learned, and why it's important.
- Limited to transitional cost factors using existing delivery infrastructures.
- They use current infrastructure and economics of scale to lower the delivered cost of H_2.
- The technical delivery cost target was not described.
- We've been talking about the need to get pipeline data for a long time; it's good that we're about to get some. You can't argue distributed vs. centralized hydrogen until you get data; this is a necessary project.
- Hydrogen delivery cost is a major barrier to cost-competitive hydrogen fuel. Understanding the opportunities and trade-offs of various infrastructure options will be importing for the ultimate design of the national hydrogen infrastructure.
- This project includes the first pipeline-supplied fueling station.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Work with OEM's station operators, and other objectives similar to the learning demonstrations, is good. The descriptions of phase I and phase II were adequate. Although it is a most interesting concept, demonstration of the "world's first" pipeline fueling station is not included in the HFCIT delivery RD&D plan.
- Objective is vague.
- Very focused on delivery factors using "current hydrogen infrastructure." May be too transitional.
- The novel concepts for reduced costs, Hydrogen Based Unit (HBU) – lower capital and LH_2 – New Delivery Concept (NDC) are worthy ideas but need to have more clear milestones to determine what success will be.
- Do not understand the value of developing a hydrogen fueler, as this is not a long-term concept for H_2 supply.
- Energy requirements for liquid hydrogen supply should be evaluated ... Is this a long-term path from an energy efficient standpoint?
- Working with OEMs to decide station locations and vehicle needs is good. Interesting that they don't list the availability of hydrogen as being as important.

- The NDC approach is interesting, but I am not quite sure as to what is being addressed here as far as a permanent hydrogen infrastructure is concerned.
- I do like the additional purification of high purity H_2.
- Will gain good insight on multiple delivery options.
- Will be important to determine how the results of this project will feed into a California-wide and Nation-wide infrastructure model.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Relatively recent project, however some work seems to have occurred apart from the DOE contracted tasks. The Hydrogen Based Fuelers for example was developed independent from the project and will be worked into the effort and is being counted toward the Agreement's technical progress. Also, interim design is almost complete yet the project is relatively new.
- Too soon to really tell.
- Only 15% of project completed. Too early to show specific accomplishments.
- They have not had enough time to achieve many accomplishments.
- They do however have a good plan of action.
- Mainly design progress so far – this is what would be expected for a project of this magnitude that has only been operating for less than a year.
- Would like to see specific performance/cost targets being set of each station/technology option.
- Good progress on station site selection and on the suite of technology options that will be included.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- Collaboration with State and Local officials is adequate.
- Focus seem to be California only.
- Good collaboration with vehicle people was indicated.
- They have identified who their collaborators are but the real work is yet to be seen!
- Good representation of partners from OEMs academia, etc.
- Although the project covers a large group of site-hosts and collaborators, it would be useful to better define the roles and responsibilities of the major players.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- The project is a State deployment effort and the plan for future R&D is unclear.
- Not a clear plan.
- Expansion and continuation of project appears to be appropriate.
- This work needs to better explain how the technical barriers will be addressed.
- Only addresses the "coming months" Some layout of the rest of the project would have been helpful in the future plans section. The next steps, however, look good.
- Need to establish multi-year, target-driven milestones.

Strengths and weaknesses

Strengths

- The pipeline concept provides a low cost hydrogen production option while securing delivery capacity.
- Excellent background with similar industrial experiences. High technical capabilities.
- This project takes into account the current work being done in California.
- Being involved with the hydrogen safety program, I am very pleased to read the contractors response to a reviewer of last years peer review who stated that cost reduction is the first priority by stating that safety is the first priority. Keep up that mode of thinking!
- The project is designed to gain knowledge and establish the viability of multiple hydrogen delivery options.
- There is a good likelihood that the project will identify options for achieving hydrogen delivery targets.

Weaknesses

- The provision of hydrogen via "mobile refueling" could pose safety risks that if realized may impact public perception during the most critical stage of hydrogen economy development.
- Needs a clear definition of why, what exactly, when and how.
- Too focused on transitional factors. Only uses current infrastructure.
- They need more progress. Perhaps a timeline with milestones would be helpful to explain where the project is going.
- Project needs to emphasize a more target-driven approach, including baselining against current standards (cost, efficiency, etc) for hydrogen delivery.

Specific recommendations and additions or deletions to the work scope

- Clarify.
- Go to higher capacity site capabilities. (Lighthouse refueling concept)
- The report that is written on the stations should incorporate and compliment the work being done under the California H_2 Highway.

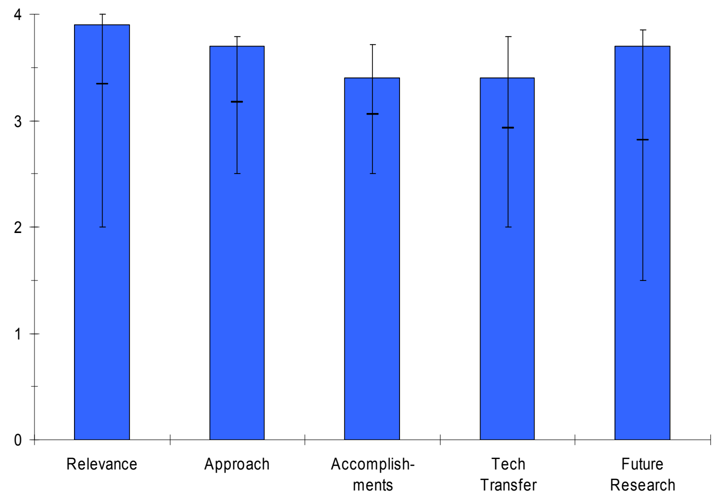
Project # TV-12: Controlled Hydrogen Fleet & Infrastructure Analysis

Keith Wipke; NREL

Brief Summary of Project

Under this multi-year validation project the National Renewable Energy Laboratory will assist DOE in demonstrating use of fuel cell vehicles and H_2 infrastructure under real-world conditions, using multiple sites, varying climates, and a variety of sources for hydrogen, including renewables. The objectives of this project include: 1) Validation of hydrogen fuel cell vehicles and infrastructure in parallel; 2) Identification of current status of technology and its evolution; and 3) Re-Focusing hydrogen research and development.

Overall Project Score: 3.6 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.9** for its relevance to DOE objectives.

- Compiling and analyzing this operating data in a third party location will be helpful to understand the viability of fuel cell vehicles.
- Validation of both FCV and infrastructure in parallel is in line with reducing U.S. dependency on foreign oil since the majority of foreign oil use is in the U.S. light duty fleet.
- Government statistics on this information that has high public confidence is essential to development of public education on fuel cells and hydrogen.
- The method of data collection in a way that assures transparency and an accurate depiction of real performance and progress is an essential yardstick for society as we weigh alternate fuel options.
- Critical to know what progress is being made.
- Very thorough and well focused for this audience.
- This project will help validate the results from the Controlled Fleet and Infrastructure projects and will enable a broader look across the projects.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- The data gathering appears to be robust and comprehensive. An impressive amount of thought has gone into its design.
- The use of tax payer dollars to support an impartial team of national scientists and engineers is ideal for the purpose of third party data collection, processing and evaluation.
- Data collection technique appears adequate, and has sufficient markers to assure that data sets are complete and not altered by participants.
- With real data now starting to populate the database, it is an important time to think critically about the kinds of data being received. Based on data being received is there additional (different) data that's needed or data not needed.
- Suggest that fleet operators be consulted as well as OEM's on data being taken and how it is used. Local conditions may give additional meaning to data adequacy not apparent to data gatherers or OEM's.
- Outstanding!
- Very appropriate.
- Providing lessons learned from the data collection and analysis back to the projects is important.

- Disappointing that the team is not employing more robust, multi-variate data analysis approaches.
- Team would benefit from analytical chemistry expertise to better understand the capabilities and limits of some of the monitoring that is being employed (e.g. hydrogen purity measurements).

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- The project has a clear set of analytical data to compile and clear goals against which progress can be measured.
- Aggregate conclusions presented to date are impressive. The Fleet Analysis Toolkit, if developed in a cost effective manner shows good progress. Actual results are demo accomplishments rather than NREL accomplishments.
- Things are in place, and real data is coming in. That's real accomplishment.
- Geared up to rapid increase in data.
- Status is thought provoking but mostly anecdotal so far.
- Project is doing a good job of benchmarking the current status across the different projects.
- Good effort has been made in establishing the systems for collecting and reporting the data.
- Conclusions presented on some of the slides are not nearly as robust as some of the conclusions presented by the individual projects. For example, what does "alarms could be improved" mean? Is there a root cause that has been identified?

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- The project must interface with a wide variety of organizations by design. The project appears to have done an excellent job collaborating with partners.
- Collaboration is required by participating OEMs. Other than database development, it is unclear how technology transfer objectives are furthered.
- This data allows DOE and OEM's to discuss program progress on an even basis.
- The public need to understand the progress being made using public money is an important aspect of these programs. Confidence in DOE oversight can be helped very much by this program.
- DOE's ability to communicate the essence of the data trends PUBLICLY will be very important.
- Top notch collaboration with all teams.
- Don't see much evidence of a 2-way communication.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.7** for proposed future work.

- Comments from the past have been addressed and there seems to be a plan forward.
- This criteria does not apply as directly, however the future of the data center is obviously to continue collecting data and validating the station and vehicle performance. The next stages of data collection and analysis were communicated adequately.
- Clearly this program must continue, and the public data will be closely watched.
- Validity and relevance of this database is currently high. Maintaining both of those characteristics with large data sets in a rapidly evolving field will be a challenge.
- Future work is more of the same. It may be too early to identify more specific focus areas.
- No change.
- Project needs to focus on identifying underlying data trends that might not be evident to the individual projects.
- The plans are important for benchmarking progress and for enabling public dissemination of the results. However, this is a significant effort and it will be important to demonstrate the value-added compared to what is being provided individually by the project themselves.

Strengths and weaknesses

Strengths
- The program is clearly well thought out and appears to be managed in an organized fashion. Very complete presentation that summarized objectives and status very clearly. Presentation material matches very well with review goals.
- This NREL Agreement is an outstanding method of facilitating interaction between principal hydrogen economy investigators. In addition, this Agreement is critical to validating whether the U.S. light duty fleet and fueling infrastructure will successfully be changed out.
- Objective perspective. Non-affiliation with data generators. High technical competence.
- Very well planned and executed so far.

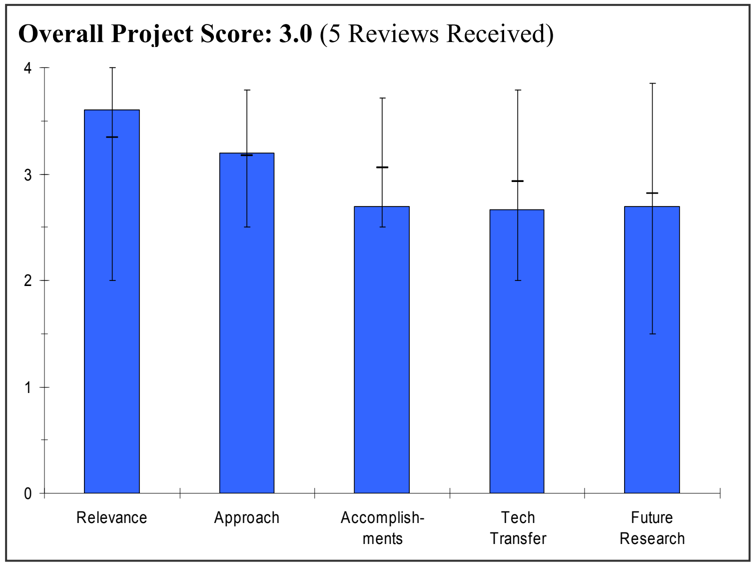
Weaknesses
- The data is only as good as its incoming quality from the source. Despite an attempt to standardize, different organizations will report at different thresholds and at different quality levels.
- Detection of only 10 micrograms per liter with existing test equipment is not sufficient to provide a complete analysis of hydrogen purity data.
- Lack of transparency to public.
- Everybody has a car, so everybody considers themselves an expert on mileage, performance, range requirements, etc and maybe they are. In any case, DOE needs to learn how to give good reports of this accumulating data; reports that have public meaning and trust. With the apparent reticence of OEM's to make this stuff public, that may be difficult.
- Has proprietary restrictions on data disclosure.
- Can't really think of any.

Specific recommendations and additions or deletions to the work scope

- Tech Val assessment reports should be distributed more broadly with appropriate web-site links.

Project # TVP-01: Hawaii Hydrogen Center for Development and Deployment of Distributed Energy Systems

Richard Rocheleau; Hawaii Natural Energy Inst.



Brief Summary of Project

The objectives of this Hawaii Natural Energy Institute (HNEI) project include developing and operating a test bed to validate and characterize hydrogen technologies in a real world setting; characterizing the effect of trace level contaminants on the performance and durability of PEM fuel cells; and investigating critical steps for hydrogen production from biomass, including biomass gasification, tar reforming, hydrogen purification, and feedstock preparation. One component of this project is to integrate a renewable energy source with an electrolyzer, hydrogen storage, and a fuel cell to power a building.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Hawaii is a good laboratory for demonstrating energy independence.
- The project includes three unrelated tasks, but each is fairly well aligned with HFCIT program goals. Each task addresses a need specified in the hydrogen program MYPP.
- This energy program has developed a broad technical base both of physical infrastructure and people knowledgeable of Hydrogen, its characteristics, and uses. This is a good example of a hydrogen technology center with a good track record capable of reliable hydrogen and renewable energy technology project deployment, evaluation, and education.
- All three tasks are highly relevant to Hydrogen Fuel Initiative objectives.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Safety is noted as a barrier but is not addressed in the approach. The approach lacks details on the specific work to be one for the fuel quality assessment task 2.
- If Biomass is the most effective source of H_2 on the Islands – increase focus on biomass effort.
- The approach has been further sharpened and focused by inclusion of economic and engineering analysis for the Power Park task.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Task 1 appears to be on track except no progress on economic analysis is reported. The construction and operation of the test stands is on track, but is not clear what types of fuel cells have been or will be tested and what will be done with the results. Not clear what work has been done to meet the objective of characterizing and preparing feedstocks.
- The program has progressed well on all three of its diverse tasks in this program.
- Good progress on all 3 tasks.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- The amount of communication products seems modest.
- Not clear if or how the task 2 results will be shared with fuel cell makers.
- Clear evidence of published work by Graduate and Post Doctoral researchers in collaboration with HNEI on the several topics in this current program are presented.
- Work with a private wind farm owner, and developers of biomass gasification technology are evidence of the breadth of hydrogen related fields regularly in contact with the Center.
- The only holdback is whether, beyond providing tours for the curious, there is community outreach to non-energy related industry and academia. This is a minor knock on a quality tech center.
- Good complement of university, city, state and industrial collaborators. To the extent that this is successful it should serve as a good example for other programs.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The presenter says the power park portion is appropriately phasing out. It would seem the biomass gasification effort is timely.
- The future work will bring the project to a conclusion but it's not clear how results and findings will be shared with others.
- Good plan for future work. These efforts should be completed.

Strengths and weaknesses

Strengths
- Project has capable teammates.
- The individual tasks are well aligned with program goals.
- Broadly developed hydrogen knowledge base and hydrogen energy capable facility.
- And the same is true for renewable energy resources of interest to Hawaii except maybe geothermal.
- This appears to be a good demonstration, however there is very little data shown to enable one to understand just how well this system actually performed, and to economics of such a co-production system.
- Good integration of skills between academic and national labs.
- Good collaborations.

Weaknesses
- The dissemination of data, learning and results seems marginal.
- The project involves three unrelated tasks and the expertise at HNEI is not clear in all three areas.
- Very limited data or results on how well this system performed including: net ac power (did the unit ever achieve 5 kWe net AC?); Electrical efficiency at start of test and at end of test. The unit only operated for 2255 hours and only 1400 hours was in the field. This is not very much demonstration time- given the funding. The power generation availability (98%) seems high. Please provide the supporting data for this figure. It is difficult to evaluate the viability of this concept from the Key Metrics: What is the net ac efficiency from the SOFC Power Module? The economics of this approach is unclear based on the information presented: What are the near-term prospects for competitive distributed co-production systems? What will be the optimal scale for such systems – seems like 5 kWe is too small? Difficult to compare this transitional co-production option with others such as pure distributed SMR and distributed electrolysis.

Specific recommendations and additions or deletions to the work scope

- Suggest more communication output.
- Keep unit running and report on more performance data. Expand story on economics.
- It may be more appropriate from a technical management perspective to separately manage (and review) these 3 distinct projects.

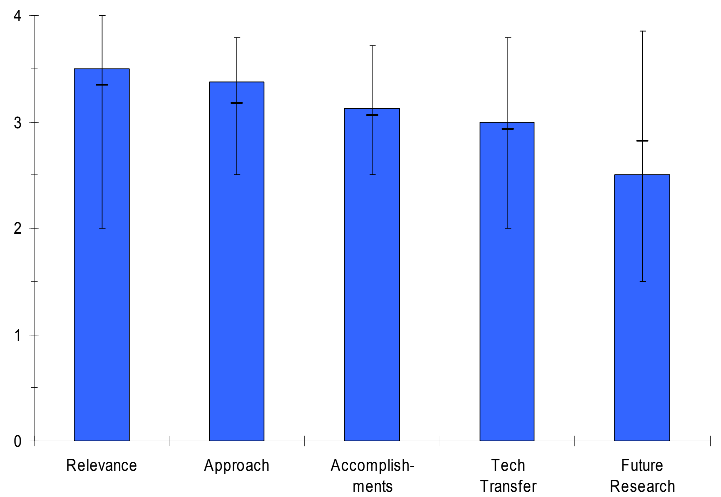
Project # TVP-03: Novel Compression and Fueling Apparatus to Meet Hydrogen Vehicle Range Requirements

Todd Carlson; Air Products

Brief Summary of Project

The objective of this project by Air Products and Chemicals, Inc. (APCI) is to develop a novel compression and fueling apparatus to meet hydrogen vehicle range requirements, as well as dramatically lower the cost, maintenance, and power requirements for fueling. A 700 barg dispensing system has been developed, and a 700 barg compressor has been built and is undergoing testing. Other components to support 700 barg hydrogen refueling are also being investigated.

Overall Project Score: 3.1 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- A high pressure 14,000 psi compressor will enable a lot of transportation applications.
- Successful development could help President's Hydrogen Fuel Initiative
- Delivery costs are a major barrier to cost-competitive hydrogen fueling, with compression being a major cost contributor.
- The potential of the technology to compress hydrogen up to 15,000 psi at a low cost is significant.
- Critical to user acceptance.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Logical.
- Approach is well thought out. Good engineering that appears to address most of problems. Need to emphasize issue of fluid mixing with hydrogen gas and low cost means to separate hydraulic fluid from hydrogen.
- Importance of safety has been well-recognized in the consideration of materials for the system.
- As identified by the PI, the issue of oil contamination must still be addressed, in particular how this will impact the O&M costs.
- Work should continue on determining the equipment configuration and cost for alternative fueling applications, namely 6500 psi storage and higher inlet pressures.
- Impressive pumping system.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- The project seemed some what slow but the presenter made a good case for the extent of challenges faced.
- Program appears to moving forward at proper speed and solving technical issues as they develop. Program should move fast because it is based on previous design for non-hydrogen system that has similar characteristics.

- Good progress in assembling and testing the unit. Through this, additional opportunities for improved throughput were identified.
- Still some technical issues to be addressed, including cooling and oil contamination.
- More attention to fueling time is needed. Greater than 20 minutes for 10 kg is unacceptable.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- It would be helpful if an effort could be made to encourage production of the product in U.S.
- Limited technology transfer reported. This activity should increase since supported by federal funds.
- Good partnerships with material suppliers. This remains important for evaluating the best component options and the current and future costs.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- This project is close to completion.
- Critical issue of hydraulic liquids in hydrogen, as identified in discussion with researcher, does not appear on plans for future work.
- Once the major technical issues are addressed, the system cost and operability needs to be validated in an integrated fueling application. PI has identified some opportunities for this; these plans and the cost of completing the validation need to be better defined with key milestones.
- The identified plans to evaluate configuration requirements for alternative fueling scenarios are important and should continue.
- Need effort on cooling to reduce fueling time – but at minimal energy use.

Strengths and weaknesses

Strengths
- Air products can validate this compressor in context to several different H_2 operations. This is an important element to giving the innovation legs.
- Good technical approach that should lead to lower O&M requirements.
- This is a very focused project with strong emphasis on cost reductions for compression while maintaining and even improving compression safety.
- PI has done a good job of identifying technical hurdles and approaches to addressing them. In the next year, it will be important to demonstrate that reasonable solutions to these hurdles can be implemented and to identify the costs associated with this.
- Great direct simplistic approach.

Weaknesses
- It is unclear how the scientific learning and core technology will be shared.
- Technology is nearing readiness for validation in a fueling application. Need to firm up plans on where the unit will be integrated and on defining a performance/durability test plan to validate the cost targets and better understand the full implications of the technology on cost-competitive hydrogen delivery.
- Need to reduce fueling time at minimum cost increase.

Specific recommendations and additions or deletions to the work scope

- It would be worthwhile to have the eventual license; do a poster on their adoptation (non recurring effort) at this innovation so as to faster commercialization lessons learned back into the knowledge base.
- Add low cost fuel time reduction.

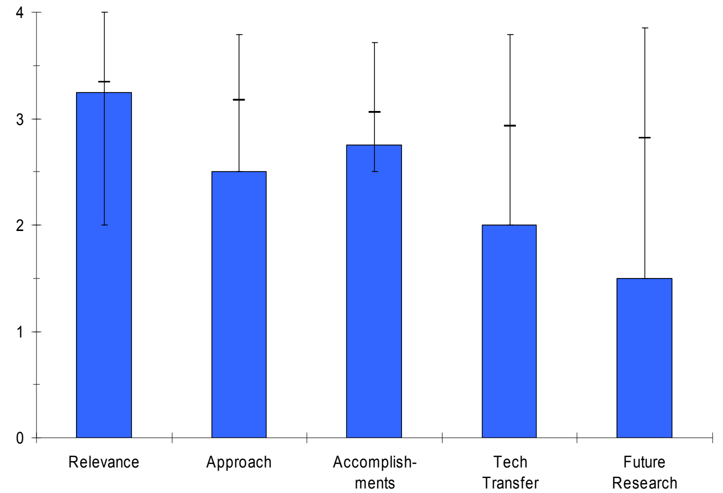
Project # TVP-05: Chattanooga Fuel Cell Demonstration Project

Joe Ferguson; City of Chattanooga

Brief Summary of Project

Through The Enterprise Center and its Connect the Valley Initiative, the City of Chattanooga is facilitating cooperative efforts between Ion America of Moffett Field, California, the City of Chattanooga, and the University of Tennessee at Chattanooga (UTC) to develop and demonstrate a prototype 5 kW class, grid parallel, solid oxide fuel cell system that co-produces hydrogen. This project provides technology validation of a near-term economical pathway to help build out the hydrogen infrastructure. The system being validated operates with high capacity factor even when the demand for hydrogen is relatively low.

Overall Project Score: 2.5 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Promotes high temperature stack deployment.
- Provides local first responder education and training.
- Provides safety education through safety evaluation at a new site.
- Co-production of hydrogen and electricity (hydrogen energy station) concept using high temperature fuel cells is very important to ultimate success in developing economical hydrogen infrastructure.
- This is an earmark according to the presenter.

Question 2: Approach to performing the research and development

This project was rated **2.5** on its approach.

- Project could have been more valuable if H_2 end-uses were integrated with original design.
- Next years operation will provide good opportunities for design of follow-on projects to utilize H_2.
- This project does not appear to be well integrated with DOE SECA program – the leading US SOFC program effort.
- This program was a demonstration of technology and not technology development per se so comments on approach not meaningful
- The University was paid to house the fuel cell with the 20% of the budget but never attempted to apply H_2 community knowledge.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The SOFC is a whole system and it's delivered, installed and operated in a grid connected mode. Not bad!
- The site from the picture looks adequate without flourish, and a local site safety analysis was done.
- The project accomplished the goals set out at the start.
- No attempt was made to rationalize this project in terms of accomplishments.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- The project seems well situated for broad local collaboration.
- The apparent lack of end uses for the Hydrogen is an opportunity to expand into new collaborative areas.
- How will the lessons learned from this project get transferred to DOE SECA vertical team players?
- No information presented that would enable the reviewer to comment on this aspect of the program.
- One might assume that a University would try to disseminate information from its projects, therefore the universities inaction undermines confidence in the commitment to a core mission.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.5** for proposed future work.

- Evidence is still largely ambiguous. Successful deployment bodes well and is the reason for the "good" rating. Active attention to operational data and it's relevance to future SOFC and Hydrogen production efforts will be the minimum next requirement. Actively communicating the collected information to the community and Hydrogen program stakeholders, as well as soliciting H_2 uses will round out the project nicely. Failure to do so would leave the work half done.
- Unclear what future work is – there were no clear recommendations presented for key areas which still need to be worked on to make this concept ultimately viable in the market place.
- No future work or next steps described.
- Never another cent should be provided to this team.

Strengths and weaknesses

Strengths
- It's operational with apparent good characteristics in the early going. The project principals have proved competency in getting the hardware and permits in place.
- This appears to be a good demonstration, however there is very little data shown to enable one to understand just how well this system actually performed, and the economics of such a co-production system.
- Achieving the objectives within the time frame laid out.
- A useful demonstration of technology.
- The presenter agreed to publish the lessons learned in regulatory compliance.

Weaknesses
- Data and other presentation information appears largely supplied by ION America, the FC supplier, and is not yet complete enough to be of real value to the engineering and project evaluation community. This is understandable given the short operational period prior to this review, but operational data that covers the full range of operational characteristics including specifically the electrical and H_2 production capability is needed. This data should transparently depict parasitics, cost and quantity of fuel as a function of product output, and any operational and control issues that were of note.
- Very limited data or results on how well this system performed including: net ac power (did the unit ever achieve 5 kWe net ac?); Electrical efficiency at start of test and at end of test (i.e. was there any noticeable degradation of SOFC performance); The unit only operated for 2255 hours and only 1400 hours was in the field. This is not very much demonstration time- given the funding. The power generation availability (98%) seems high. Please provide the supporting data for this figure. It is difficult to evaluate the viability of this concept from the Key Metrics: What is the net ac efficiency from the SOFC Power Module? The economics of this approach is unclear based on the information presented: What are the near-term prospects for competitive distributed co-production systems? What will be the optimal scale for such systems – seems like 5 kWe is too small? Difficult to compare this transitional co-production option with others such as pure distributed SMR and distributed electrolysis.
- No description of the learnings from the program.

- No description of performance of the system components and areas for improvement.
- The presenter admitted he was propagating data provided by the hardware vendor without ANY analysis or sanity checks.

Specific recommendations and additions or deletions to the work scope

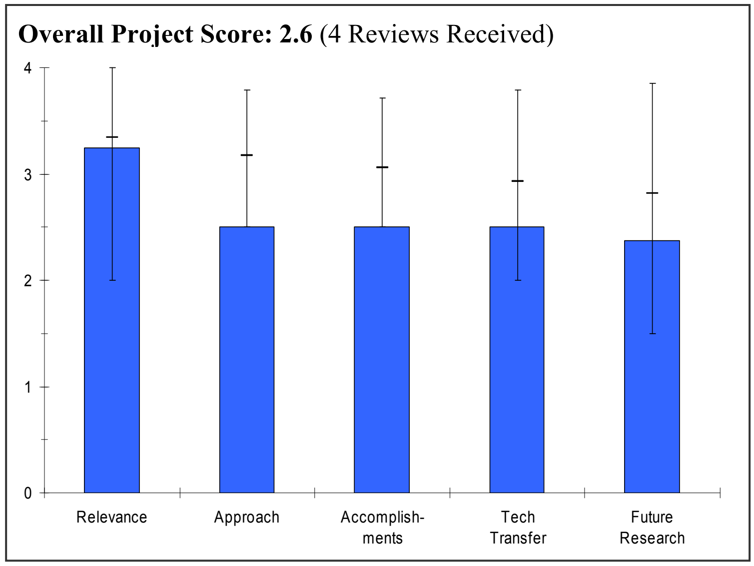
- Add some Hydrogen uses for the product hydrogen.
- Keep unit running and report on more performance data. Expand story on economics.
- While this was an apparently successful demonstration program within the DOE program, the reviewer questions what learning's are being disseminated.
- Why was the cost share less than 10% for a demonstration program?
- The university should be directed to at least attempt to contribute to the intellectual baseline.

Project # TVP-06: NextEnergy Microgrid and Hydrogen Fueling Facility

Dave McLean; NextEnergy

Brief Summary of Project

This NextEnergy project is developing a hydrogen station in 5 phases: (1) supply hydrogen to the NextEnergy Center Microgrid via tube trailers to fuel hydrogen-based fuel cells and engine-generator sets; (2) supply hydrogen to a packaged vehicle fueling system via tube trailers; (3) install permanent storage and the associated equipment such as the Gas Control Panel, the hydrogen compressor, the electrical switch gear, and control and communication equipment; (4) install five hydrogen generator “test bays” and fill one test bay with equipment that will allow NextEnergy to produce ultra-high purity hydrogen on-site for use in OEM “fuel cell” vehicles; (5) install one additional high purity on-site hydrogen generator.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- May be valuable for public education.
- Relevance is that it supports the overall goals of the Technology Validation program.
- This places hydrogen knowledgeable people and re-fueling facilities in a key transportation geographic node.
- This is a "shotgun" project trying to be all things to all people but when probed it is an attempt to gain goodwill.

Question 2: Approach to performing the research and development

This project was rated **2.5** on its approach.

- The plan is not innovative.
- As an engineering design and construction project in support of the Technology Validation Program, this project is well defined and being implemented.
- There are no particular goals or efforts to overcome any key technical barriers.
- Project vision is articulated well and the need to maintain flexibility of configuration is a likely good decision in this case, but a tighter definition of what would fill out the equipment and function of the facility would guard against the possibility, (NOT a problem in this case) of poor project management. Things are going well, but this is a high profile project and clearer more specific descriptions of planned capability would help a little.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- Apparently coordinated with the DOE TVP schedule.
- Despite the many competing needs at a facility of this type, good progress has been made. I understand that the facility permitting went very smoothly and was completed in less than four weeks.
- The difficulty of bringing competitors in the transportation sector together to accept hydrogen from a single facility is quite hard, and the progress in that direction is commendable if not yet finished.
- The ability to build a building with tax payer money is not an accomplishment.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- The group proudly declares it is in service to the big 3. It is important that the big 3 allow the dissemination of meaningful information.
- No description about this aspect of the project to make relevant comments.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- Nothing innovative. No commitment to sharing information. No commitment to training or education.
- Adequate target dates provided for remaining phases.
- The basic need to have a good hydrogen refueling center with capability to evolve with the rapidly emerging hydrogen vehicle technologies in the area is really the key feature of the facility. The fact that they are planning for a diverse set of hydrogen test and demos on site is a good feature.

Strengths and weaknesses

Strengths
- Might help bring the public along with learning and confidence.
- When completed, this site could serve as a useful resource as a test bed for various DOE supported distributed hydrogen production technologies.
- Apparently good interaction with the DOE Hydrogen Safety Review Panel
- Stakeholders in the success of this facility includes a large electric utility, a large (are there any other kind?) petroleum based energy company, three vehicle manufacturers, merchant hydrogen companies, and component suppliers and a University.

Weaknesses
- There is nothing available in the form of objective information and no commitment to fix that problem. The presenter was ignorant of the projects.
- No apparent goals to advance the state of the technologies.
- The goals of this project are limited to providing the test facilities for other technology development efforts, and that certainly serves a useful purpose.
- Many competing interests and the need to manage the project in a publicly transparent process. It could be a success.

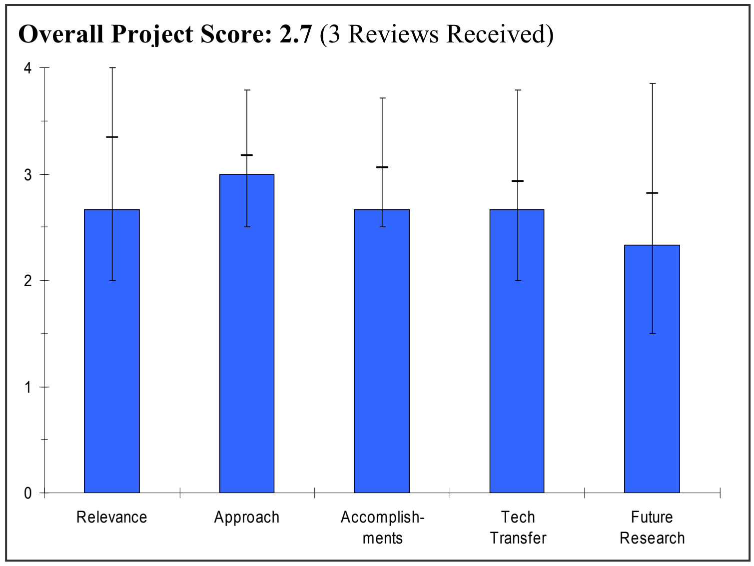
Specific recommendations and additions or deletions to the work scope

- Require a clear set of reporting and communication products. Cancel any sponsorship with taxpayer funds.
- What will the test protocol be for dispatch of the hydrogen generators? To what extent will the operational benefits of this micro-grid be documented and related to the local utility?

Project # TVP-08: Hydrogen Filling Station

Robert Boehm; UNLV

Brief Summary of Project



As a first step in the development of a hydrogen utilization network, the University of Nevada-Las Vegas Research Foundation is installing and analyzing the performance of a hydrogen generating and fueling system powered by solar energy. Objectives included development of the requirements for the generating and fueling system, survey of potential sites for the filling station and determining favorable /unfavorable characteristics of each, selection of the site with site plan and support to the site permitting process, design of the system layout, construction of the filling station in Las Vegas, monitoring operation of the system, and characterizing its performance. In the second step of the process, the filling station is being supplemented with a high-pressure electrolyzer that was developed for this project. Two utility vehicles are being converted to use hydrogen as fuel. One of these is an electric vehicle that will function as a hybrid full cell vehicle; the second is a hydrogen-fueled internal combustion engine system converted from a gasoline-fueled ICE system. Finally, engineering and performance demonstration of tandem solar cell systems is taking place as well as some basic science studies.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.

- The project appears to include multiple unrelated tasks, though each has some applicability to program goals.
- No plans to evaluate economics of chosen technologies / systems against others; No plan to provide capital and operating cots of systems.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The approach doesn't say anything about what work will be done on the project or what technologies will be investigated.
- A plan for data collection, analysis, and dissemination is not discussed.
- Barriers are not adequately discussed.
- Although not the most cost effective option, some information can be gained from pieces of the project. Cost of PV / H_2 generation option; Safety / permitting / construction lessons learned if included in results.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The project timeline shows 78% complete, but the funding is only 55% expended.
- For being three years old, the project doesn't seem to be that far along.
- With amount of funding and numerous focus areas encompassed, more results should have been realized most accomplishments to date don't add to existing done knowledge .
- It would have been useful to present some results which relate to the 2009 Targets.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Collaborations aren't adequately addressed in the approach on the future work discussion.
- Industry partners providing hardware; Proton Energy seems to be the only subcontractor involved with overall project; project had no definite collaboration outside the project and limited with NREL.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Not clear how fundamental studies with proton adds values to this particular project.
- Project is working on areas that don't fit together and would be better if focused on particular process improvements such as: pressurized electrolyzes, solar / H_2 development economics, lessons learned on permitting.

Strengths and weaknesses

Strengths
- Vehicle conversions include both H_2 /ICE and FC/ICE hybrid vehicles.
- Project works with leading fueling station partners. Project has lessons learned in siting fueling stations. Project looking at novel low cost vehicle such as ICE / FC.

Weaknesses
- The scope includes multiple disjointed tasks. Funds would probably be better spent focused on vehicle conversions, the filling station, and data collection and analysis.
- Project spends funding on traditional hybrid / Fuel Cell vehicle development that is better accomplished with Freedom Car program. Project focused on creating Nevada road map that primary benefits a State. Project hasn't partnered with service business to use Fuel Cell vehicles only with the water district which has a limited need.

Specific recommendations and additions or deletions to the work scope

- A task should be added for data collection, analysis, and dissemination.
- Add economic analysis of proposed system against others. Delete Roadmap effort. Delete hybrid / Fuel Cell development effort as little to no new information is being generated. Add Go / No-Go on proposed system and if technically / economically infeasible look at completing more feasible options.

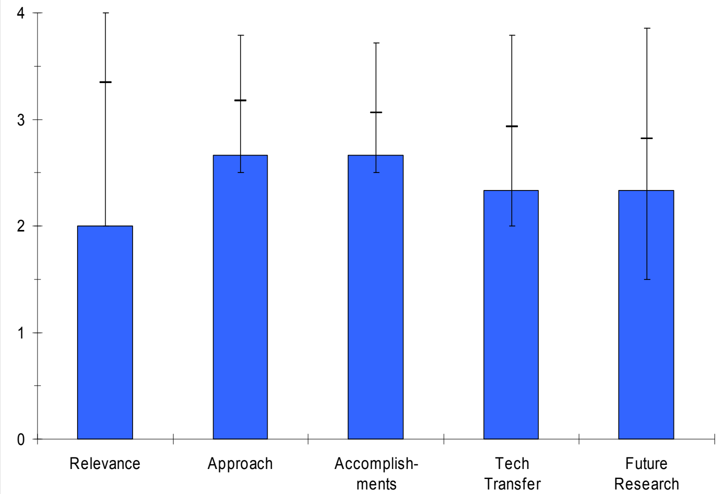
Project # TVP-10: Fuel Cell Powered Underground Mine Loader Vehicle

Arnold Miller; Vehicle Projects LLC

Brief Summary of Project

Vehicle Projects LLC is developing a zero-emissions, 23 metric ton, 160 kW, fuel cell-battery hybrid mine loader. Three fuel cell stacks will provide 90 kW of continuous power. Nickel metal-hydride batteries will provide peak power as well as the ability to recover energy through regenerative braking. Hydrogen will be stored onboard as a metal hydride. Vehicle Projects is evaluating the loader's safety and performance, primarily in surface tests, and evaluating its productivity in underground mines in Nevada and Ontario.

Overall Project Score: 2.5 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.0** for its relevance to DOE objectives.

- Niche application for developing near-term demand for H_2.
- The concept of saving lives in a mine by preventing asphyxiation from diesel combustion would be useful – but is NOT articulated.
- Project was an early and significant program requiring difficult vehicle integration tasks. Program has shown that hydrogen and fuel cells can be deployed successfully in specialized applications.
- The breadth and target of DOE's H_2 & FC programs has changed since conception of this project. The need to develop consumer transportation alternatives has overtaken the need to develop niche specialized demonstrations, thus rendering this project a successful, but somewhat less important project for fulfilling the President's H_2 Initiative.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- Seems like a serialized, slow deliberate approach that is focused in satisfying the industry. Does not make a contribution to H_2 community.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- The project is unimpressive in its simplicity. The progress is poorly documented.
- Technical accomplishment is quite high for this project; a large, heavy, underground ore loader/hauler travels with a PEM prime mover. However, follow-on developments have not apparently emerged.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- There is a big list of participants but they appear to be superficial relationships.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- This is supposed to be the last year and much of the work is done.
- Not clear what follow-on development has revealed itself, or is being pursued.

Strengths and weaknesses

Strengths
- The possible commercial result would be timely and on excellent illustration of H_2 power at work.
- Good technical follow-through on a well defined and difficult project.

Weaknesses
- This project doesn't appear to have committed industry participation.
- Project not leading to broader deployments or new FC capabilities.

Specific recommendations and additions or deletions to the work scope

- I suggest requiring this project conduct information exchange, public lessons learned, regulatory compliance, safety, and environmental impacts. There seems to be little inclination to share information by the project manager.

Safety and Codes and Standards
Summary of Annual Merit Review Safety and Codes and Standards Subprogram

Summary of Reviewer Comments on Safety and Codes and Standards Subprogram:

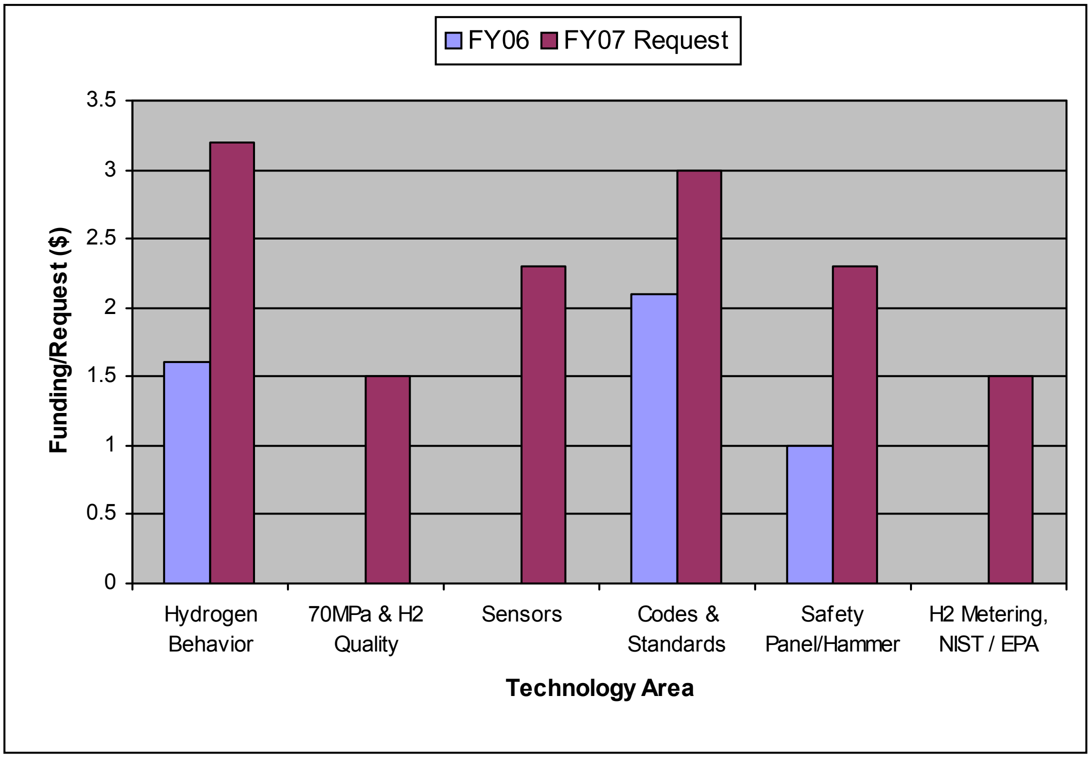
In general, Safety and Codes and Standards Subprogram reviewers stated that projects were productive and successful, especially given their levels of funding. The Reviewers were impressed by the breadth of activities and the ongoing commitment to safety, codes, standards and information-sharing activities. They stressed that successes in this subprogram touch every other DOE hydrogen-related activity by fostering acceptance, collaboration and communication with critical stakeholders and consumers.

Reviewers stressed the importance of continuing efforts in critical areas such as hydrogen fuel quality, risk assessment and materials research, gaps in hydrogen codes and standards and regulation coordination efforts, but lamented the limited budget. Suggestions for maximizing progress included leveraging the efforts of universities, standards organizations, national labs and industry. International consensus on codes and standards is very important, but there was some uncertainty about the effectiveness of the GTR activities.

Five safety projects were reviewed. The Hydrogen Codes and Standards work was praised for its clear objectives, well designed National Template and broad access and interaction with global stakeholders, SDOs and CDOs. Reviewers mentioned that the funding seemed inadequate to meet the Project's goals and timeline. Research and Development for Hydrogen Safety, Codes and Standards, is focused on hydrogen behavior, materials research and risk assessment to support the development of technically sound codes and standards and was praised for impressive industry/SDO/CDO coordination. Some reviewers thought there was room for more coordination but cautioned against developing a quantifiable risk assessment tool, lest it be misapplied by courts. The Global Technical Regulations work was praised for the dialogue fostered with other countries, particularly Japan. However, there was concern about the mission and direction of the project, as well as the lack of demonstrable progress. The Incident Reporting Database was thought to be an effective tool for sharing lessons learned and was progressing quite rapidly, given the low funding. Some concern was expressed about lack of mechanisms to ensure that information was extracted. Finally, the Safety Panel was regarded as strong in concept and in qualified membership. Fostering the collaboration and communication of safety experts has helped to promote and ensure safety across hydrogen-related projects. Some concern about lack of focus was expressed, as well as the difficulty in translating the safety panel's recommendations to commercial settings.

Safety and Codes and Standards Funding:

Safety and Codes and Standards funding includes international activities as well as national development and coordination among several agencies. A large number of reviewers expressed concern over funding gaps and were concerned that lack of funds would delay future activities. The recently issued National Academies' report recommends additional funding for safety, codes and standards and to increase public awareness of hydrogen safety issues to facilitate the commercialization of hydrogen technologies.



Majority of Reviewer Comments and Recommendations:

Subprogram scores were average to high, with an overall average of 3.3. As in 2005, the lower scores reflected dissatisfaction with the progress and direction of the Global Technical Regulations activities at Los Alamos National Laboratory. Reviewers also indicated that the Safety Panel has a range of high quality representatives but the Panel should evaluate its mission and strategy to maximize its effectiveness.

There was continuing distress about low or reduced funding for the Safety and Codes and Standards subprograms, due to budget cuts and Congressionally-directed projects. The low funding could jeopardize overall DOE objectives and commitment to the Hydrogen Fuel Initiative.

Recommendations included:

- More R&D is needed to fill in the gaps in hydrogen codes and standards; more focus should be made on following the roadmaps presented by the Codes and Standards Technology Team.
- Safety needs to be highlighted and tracked during the DOE demo program. Recommend DOE consider ways of focusing safety as an absolute.
- Inclusivity and international collaboration are strong elements of the DOE Hydrogen Safety strategy, but there is potential for redundancy and diluting efficiency. The Subprogram must continue to work to ensure communication and efficient collaboration on the range of safety activities.

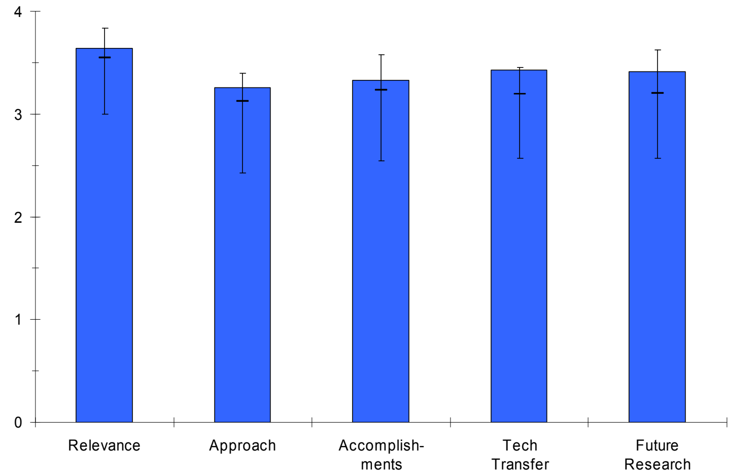
Project # SA-01: Hydrogen Codes and Standards

Jim Ohi; NREL

Brief Summary of Project

In this project, the National Renewable Energy Laboratory is working on hydrogen codes and standards to expedite hydrogen infrastructure development, coordinate such development activities for the Hydrogen Program, and incorporate hydrogen safety considerations into existing and proposed national and international codes and standards. This will be accomplished by bringing together experts to address key issues, coordinating collaborative national and international efforts between government and industry, and by serving as the central point of contact for up-to-date information on codes and standards activities.

Overall Project Score: 3.4 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- As the presenter noted, there is considerable need to focus on the research needed to support and develop consensus standards. Greater emphasis should be placed on furthering these efforts.
- This project's support of the codes and standards efforts is important to provide overall direction.
- It is absolutely necessary that safety be maintained throughout the R&D and that affordable, uniform codes and standards be developed.
- Without codes & standards, there will not be a hydrogen economy.
- Codes and standards are a must and an enabler for the hydrogen economy to develop.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Being inclusive is good, but the effort may be spread too thin given the resources. The various coordinating committees and widespread partnerships such as that with the NHA may be creating redundancy and diluting efficacy. Future efforts should consider focusing on those gaps that are best addressed by government R&D.
- Need better prioritization between focus areas to identify key codes and standards to be developed, while understanding the technology gaps restricting their development.
- The project has a well defined road map to address a complicated overlap of responsibilities.
- Coordination through national and international organizations is excellent. The move of the C&S development effort to a private sector manager may or may not have a positive impact.
- Should make clear in the National Template that SAE has responsibility for storage systems as part of Containers. SAE does all vehicle standards (and has contributors from all of the global OEMs). Grounding of refueling stations is a red herring. If the station is built properly, there is no grounding issue. NextEnergy's design with grounding pipes, etc., should not set design criteria, for example. Other installations have been implemented successfully in current retail locations.
- The National Template has enabled a complex system of codes and standards to be effectively managed.
- The approach being followed is sound and has the support of industry.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- In areas where concise focus was applied, the most meaningful and measurable progress was achieved. A good example is in fuel quality. These efforts have more value than some of the general overarching and widespread coordination activities which are not resulting in meaningful progress.
- Information on website is good.
- The project has had some notable successes. The HIPOC team is a good example where a need was identified and a means to fix implemented.
- Accomplishments relative to funding that has been available is excellent. It is unfortunate that more funding has not been available to accelerate national and international efforts.
- H_2 quality work definition has been well done. Alignment of all interested parties is in place. 70 MPa work plan also important and in place. Performance based standards are the goal. NREL's web-based hydrogen bibliographic database will be useful to the H_2 industry. Good job.
- Energy company participation in the spec development process will enable cost consideration to be factored into the specs.
- The accomplishments in the model code area are as good as can be expected considering it is a consensus process with a number of competing agendas.
- The accomplishments in the product standards area are in step with the progress within the industry.
- The progress on a hydrogen fuel standard and applicable test and sampling methods are critical path and need to be accelerated to keep pace with other activities and meet the DOE stated goals.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Greater emphasis should be placed on transferring the R&D to the standards/code language, particularly in the non-vehicle codes. This program would benefit from greater participation of the technical researchers such as SNL participating directly in the TAG or standards committees. There is a disconnect between the implementing or user groups such as code officials and safety engineers and SAE and the high level coordinating effort from select industry and technical experts.
- Need better collaboration with other projects and institutes. Much work applicable has been accomplished in other projects but has not been integrated in this project.
- The project interfaces with a wide variety of industry, government, and standards organizations. An improvement could be advertising the efforts more widely.
- All appropriate (and even peripheral) SDOs and CDOs are involved. Stakeholders are well covered.
- NREL is doing a good job coordinating and following all the diverse groups involved in this effort.
- Collaboration with OEMs, energy companies, FC providers, national labs, C & S groups has enabled gaps to be identified and closed.
- Collaboration in this area is excellent. The automotive, fuel cell, specialty gas, energy and safety industries are engaged, focused and cooperating.
- The information transfer methodology is also excellent and appears to be world class. The information is easily and readily available in real time.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- Proposed work is focused, and if completed, answers near term needs that will advance the state of the industry.
- Future work proposed shows no prioritization and therefore advancements will be slow. Focus is on "in the Woods" work instead of high need areas and focused developments efforts.
- The project has some important objectives for this forthcoming year. This was clearly communicated in the presentation.

- Plans to transition this work to a private contractor could be a marginally positive to very negative move. This change will be highly dependant on the contractor selected and their motives for taking the task. Would like to see a test plan for H_2 quality and prioritization of codes and standards to be developed.
- Great plans need to be funded.
- Future plans are aggressive and focus on the critical areas.
- The proposed research matches the stated industries needs and is current. This project has demonstrated the ability and willingness to adjust and redirect activities as the developments dictate within budgetary constraints.

Strengths and weaknesses

Strengths

- Great coordination w/NHA/FC C&S coordinating committees good getting ICC and NFPA to talk.
- The PI clearly is knowledgeable in this area. He has developed a comprehensive plan.
- Partnerships and interactions with stakeholders, SDOs, CDOs; both nationally and internationally.
- Work on H_2 quality has been very important in bringing the area out as an issue to discuss.
- National Template, active participation of energy companies in spec development process, and DOE coordination of a complex system of code and standard development are strengths.
- The industry, national lab and SDO/CDO collaboration is the key strength. The focus on performance based solutions over the historical proscriptive methodology has allowed rapid advancement in several areas.
- An equally important strength is the project leader's patience with industry members who are aggressively pushing to complete the various initiatives to maintain the DOE schedule and contain costs. Much of this friction is due to the funding not matching the schedule and work load.

Weaknesses

- Project needs better focused direction in specific C&S development specifically in the SAE / vehicle area and fueling station areas. Need more coordination with industry.
- The funding appears to be inadequate to address all of the needs in this area. The project is in a position to influence, but not direct the activities of standards organizations. These organizations often have conflicting interests.
- The biggest weakness is that many of the standards should have been developed already, so this effort started out behind schedule. Unfortunately, funding limitations have caused this and are generally out of the control of HFCIT.
- Lack of funding has pushed back the timeline.
- The weakness is the lack of funds for applied research to resolve outstanding, critical path items. Paths need to be identified that will generate the required data at a greater rate and for less money. However, this weakness is out of control of the project and, for that matter, the DOE leadership.

Specific recommendations and additions or deletions to the work scope

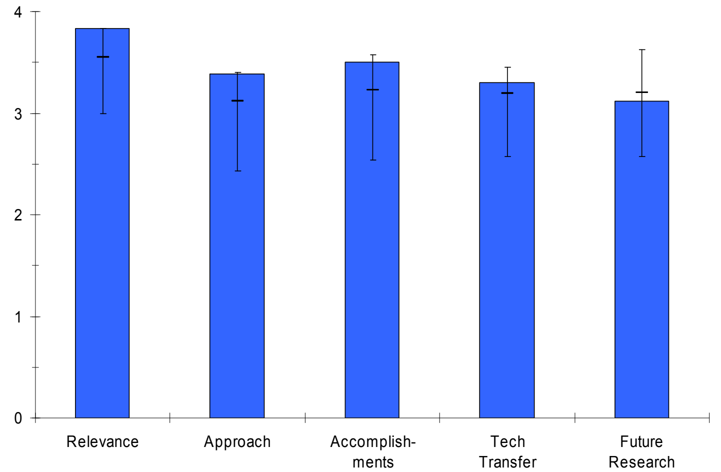
- Template development seems to be completed with little additional development per year. This suggests little to no further effort is required. Needs to integrate with other projects in delivery, tank validation, storage, etc.
- Additional funding that can spur additional industry and standard organization support.
- Presentation of fuel quality R&D should have included an overview of the test plan.
- Note that on the National Template, SAE responsibility for Vehicle Containers includes Storage Systems. Drive to consensus on grounding "issues," and gather viewpoints of all OEMs (global).
- Project scope is appropriate.
- Increases in funding for hydrogen quality and high pressure vehicle fueling.

Project # SA-02: Research and Development for Hydrogen Safety, Codes and Standards

Chris Moen; SNL

Brief Summary of Project

Overall Project Score: 3.5 (6 Reviews Received)



This project contains two major elements that address 1) risk assessment and consequence analysis of unintended hydrogen releases, and 2) compatibility of hydrogen with engineering materials. The purpose of the hydrogen safety scenario element is to develop a scientific basis for evaluating credible safety scenarios, providing technical data to codes and standards developers. Safety scenarios are used to map unknowns in the codes and standards decision making process into R&D on hydrogen behavior in engineered systems. Sandia National Laboratories (SNL) is developing benchmark experiments and a defensible analysis strategy for risk assessment of hydrogen systems, including experimentation and modeling to understand the fluid mechanics and dispersion of hydrogen for different leak rate regimes. The purpose of the materials compatibility element is to create a Technical Reference to guide material selection and methods of construction in codes and standards development for the hydrogen economy infrastructure. Material testing is being conducted to fill information gaps identified during the literature search. Material systems include pressure vessel steels (stationary storage and transportation of hydrogen gas), pipeline steels (hydrogen gas transportation), stainless steels (ancillary components in the storage, distribution, and consumption of hydrogen gas such as piping, pressure relief devices, and valves), aluminum alloys (hydrogen gas storage vessels on vehicles), copper alloys (high-pressure hydrogen seals), and composite systems.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Objectives are good; execution may need to be revised
- This project is directly answering critical identified priority needs and is addressing clear technical barriers.
- This project will supply some of the basic research building blocks on which industry can build.
- Understanding H_2 compatibility with materials is critical to assuring safety.
- Important topics to support data based standards.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Material work is good, need to evaluate existing H_2 plume data. Much data already exists; it seems this hasn't been integrated into current work.
- Need much better direction on how to address risk.
- Risk analysis work is technically sound. There are opportunities for improvement if the work includes more coordination and input from code officials and/or regulators depending on the application. Risk analysis is difficult in larger multi-component/ complex systems for which unexpected incidences occur and are difficult to incorporate into risk assessment.

- Several barriers were described in the presentation, along with the project team's ability to overcome those barriers. The approach for the basic technical research appears sound. The approach for other areas, such as risk assessment is less clear.
- SNL's materials expertise is being well utilized on this project. Their knowledge has enabled timely adjustment of test procedures and understanding of research data.
- Good idea to look for gaps in material compatibilities for hydrogen embrittlement.
- The approach to generating the data is excellent.
- The intent to generate a risk assessment methodology raises serious concerns. Anything that quantifies a qualitative concept, such as safety, and is used in the public venue may be misapplied and end up as a litigation tool with personal injury lawyers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- Material progress good.
- H_2 plume and risk work and accomplishments seem less than anticipated with existing data and risk information available.
- Program is providing technically sound and detailed answers for each of the topics being investigated.
- It's not clear how risk assessment will be done considering variation of possible construction techniques amongst vendors.
- Adjusting priorities to meet the needs of ASME is appropriate.
- Great accomplishments-very important work. The most important discovery is regarding the duplex steels investigation.
- The technical accomplishments on material properties are outstanding.
- The accomplishments on hydrogen flame characteristics are also outstanding.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Need better collaboration between SNL and others doing material testing.
- Other labs have flame plume & hazard data; need better collaboration.
- Some collaboration has been described, such as with ASME. It was not clear if the project is working with organizations such as NFPA that are establishing setback distances. This collaboration could identify additional areas for testing.
- Immediate Web publication of chapters assures information is available in a most timely manner.
- Impressive industry/Codes and Standards coordination.
- The collaboration on high pressure materials with ASME and CGA is very good.
- The collaboration on the pure science of flame characteristics is very good.
- The collaboration on the applied science of flame characteristics is directly supportive of the model codes effort.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.1** for proposed future work.

- Need better plan to get most data to ASME.
- Need better use of existing data from places other than SNL (H_2 plume and risk).
- Need better collaboration to determine risk approach using existing/previous examples.
- An impressive list has been provided for next year's work. It's not clear how this work is selected or prioritized.
- Prioritization of future work is generally good. More work is needed to establish an acceptable risk level.
- Important areas of research.
- The plans for generating the applied science on hydrogen flame characteristics are excellent.

- The plans for work on metals exposed to "pure hydrogen" at storage pressure are also excellent. However, it is hoped that this work would also address the potential effects of the impurities expected in a fuel grade hydrogen.
- The plans on generating a "risk analysis" methodology are of concern. However, this may be an issue with semantics.

Strengths and weaknesses

Strengths

- Great capabilities for material testing.
- This project is doing basic, fundamental research that will support all other programs. In particular, the materials work is critical to the safety of multiple other projects. It is difficult to obtain funding for this in industry, so DOE support is important. This is one of the few areas providing that information.
- SNL's materials expertise is this projects greatest asset.
- Excellent R&D activity. Making important technical discoveries regarding materials compatibility & risk assessment.
- The strengths of the flame research are that it addresses the pure and applied science needs (mechanisms and real world applications like set back distances).
- The strength of the materials research is that it addresses the pure science needs (mechanisms).

Weaknesses

- Need better collaboration outside the project in all parts of project.
- It's not clear how work is selected and prioritized. It would be helpful to understand this process to make sure that the most important research work is being done to aid the hydrogen industry. This work could also be better publicized to maximize value of that information.
- I am not convinced that "no greater" risk is adequate. Introduction of a new technology, such as this, would be set back significantly if there were an "equal" accident.
- No gauge of the impact that the work done in this area has on the codes and standards.
- The weaknesses of the materials research is in the applied science area. These materials will be subjected to a fuel grade hydrogen. This means impurities. Are there additional issues associated with the impurities?

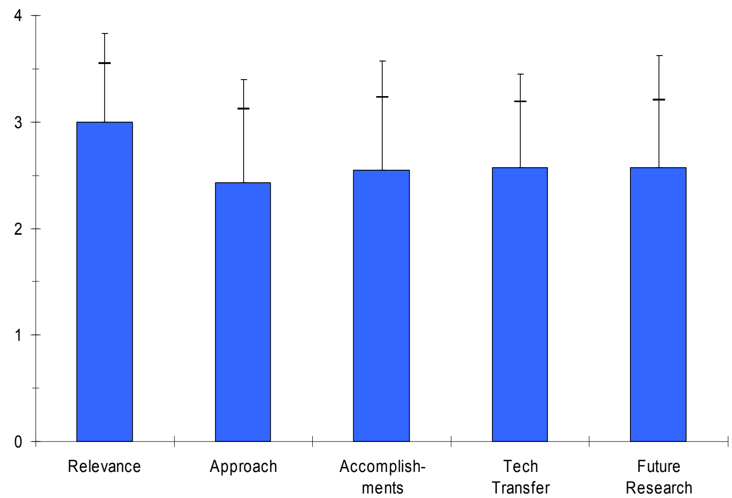
Specific recommendations and additions or deletions to the work scope

- Need approved test matrix to meet ASME needs.
- May be able to reduce H_2 plume R/D or eliminate based on previous work completed outside SNL.
- Need firm risk assessment directions.
- Providing the basic research data appears to be where this project can add the most value. Keep the project focused in those areas.
- Need to reexamine the allowable risk level and programmatic impacts of a potential accident.
- Recommend reviewing the influence that the data generated from the R&D work / risk assessment has had on the relevant codes and standards. It would be good also to survey the Standards industry, specifically ASME, ASTM & SAE regarding materials use for stationary & mobile uses of hydrogen.
- Do some applied research at the transmission and lower storage pressures on metals with a fuel grade hydrogen.
- Evaluate composite materials at all transmission and storage pressures on pure and fuel grade hydrogen.
- Evaluate various plastics and polymers at building and appliance pressures on pure and fuel grade hydrogen.
- Attempt to engage this researcher as a voting member in the various technical committees on hydrogen in the model and design code activities (ICC, NFPA, and ASME).
- Publish suitable test methods through the appropriate SDO (ASTM).
- Publish the combined results in a government manual, readily available to the public. Example that could be followed would be the US Bureau of Mines report and NASA NSS 1740.16.

Project # SA-03: International Projects: Global Technical Regulations

Cathy Padro; LANL

Overall Project Score: 2.6 (7 Reviews Received)



Brief Summary of Project

This Los Alamos National Laboratory (LANL) project provides technical support for the EPA/DOT/DOE joint effort in the development of global technical regulations for hydrogen and fuel cell vehicles. Working collaboratively with DOT/NHTSA (principally), LANL participates in the global effort to develop performance-based standards and regulations for hydrogen vehicles. This project also provides technical support to the International Partnership for the Hydrogen Economy and its efforts related to regulations, codes and standards.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The concept behind the effort has merit. This project however does very little to support the President's Hydrogen Fuel Initiative.
- It will be helpful to the hydrogen industry to have common design standards throughout the world.
- Important to assist in the coordination activities in GTR process.
- GTRs are a necessary stretch goal.
- This project shows good alignment to the goals of the MYPP to ensure alignment with the UN/GTR process and to ensure the appropriate U.S. influence in the GTR process.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- The approach does not facilitate US leadership. It prevents the representing bodies for the US from being able to actively engage and lead in the necessary work under the GTR process. The effort does not seem to effectively engage industry or create a mechanism for DOT to better represent US interests.
- The presentation doesn't show how the barriers to progress listed will be addressed.
- Not very clear how the DOE will use R&D to assist in the GTR process.
- Active participation in the European and Japanese code and standards development is a must.
- Continue to ensure that DOT has the appropriate information to take to the UN/GTR process.
- Good progress overall. Need to keep in mind that DOT/NHTSA has responsibility for vehicle safety and EPA has responsibility for emissions. The two responsibilities do not overlap, so there is no need for "coordination" by DOE between the two. Monitoring among the two is OK. Work to ensure that the three active bodies (Japan, US, and EU) are all proceeding along the same path without unfair advantages gained by any one region.
- The approach appears sound. However, the apparent preference for European initiated product standards with regards to hydrogen over domestic product standards is surprising. It might be expected that the domestic, performance, based standards generated by North American industry would be championed as the basis of international product standards. It is also surprising that of the international standards organizations, ISO would seem to be given preference over other international SDOs, which are more frequently adopted, who are working in this venue (e.g. ASME, ASTM, SAE)

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- No new information was presented. No technical accomplishment that met the needs of EPA and DOT were presented, and it is not clear that the work is responsive to the stated needs.
- The project has shown progress. It's not clear how this progress compares to overall objectives of the program. A timeline showing activities out to 2010 would be helpful.
- No measurable accomplishments in report.
- Key contacts have been made and key meetings have been attended.
- Progress has improved for this project this year including the GTR development process proposal.
- Clearly understanding European and Japanese direction and regulations with respect to hydrogen related codes and standards is critical.
- Facilitating DOT interactions and participation in GTR process is good.
- The progress of this process seems to be limited to a minimal organization tree in which the US is not listed as leading in any activity. This passive position seems to be in conflict to the President's goals. Additionally, a summary of the key 'hot items' based on a preliminary review of the Japanese regulations which was published seven months ago should be available.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- This project should focus heavily on tech transfer/collaboration and support for DOT and EPA yet fails to do so.
- It was not clear from the presentation how collaboration within the US would be handled to support US position on final GTRs.
- Interfaces at the international level have been established which should allow US data and standards to be part of the GTR process.
- Close collaboration with DOT and other codes and standards bodies is essential to the success of this project.
- The collaboration on this project appears to need some attention. The response to a query from the previous years review was disappointing.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- The presenter did not enumerate any real specifics or details for which to evaluate the value of the proposed future research.
- It's not clear what approaches will be used to overcome the barriers listed on the first slide. For example, what can overcome the barrier to US industry participation?
- Not very clear how the DOE will be assisting the GTR process to bring the US/ German and Japanese contingents together.
- This is a long term process and constancy of purpose will be necessary to stay the long course.
- The proposed future research is sketchy. This is probably due to the dysfunctional nature of the GTR process and bureaucratic inertia.

Strengths and weaknesses

Strengths

- The initial structure is in place to build upon.
- US will have a place at the table as GTRs are developed.
- P.I. knowledge of the key issues is very good.
- Good working relationships with the codes and standards community.
- Difficult mission and DOE & LANL have done a good job balancing all the players.
- The interfacing with the various members states involved in the GTR process.

Weaknesses

- There do not seem to be clear goals for success. This might be a difficult area for specific goals, but some effort should be applied to do so.
- Need to work on putting together a clearer presentation on activities. Not a lot of movement from this activity in the GTR. Not a clear understanding regarding how budget was spent.
- This project has been lumped into a general fund which could make it too easy to slip away in future years.
- Direct role in UN/GTR process lies with DOT, not DOE.
- I think LANL understands the current GTR process and recognizes current bumps along the road, but to reiterate, the EU directive E1HP that has moved through Parliament and now at the European Commission is the biggest issue right now. It is too regulatory and not sufficiently performance based. DOE/LANL should work with the Japanese and ensure that a set of GTRs results that benefit everyone equally.
- The weaknesses with this project are the lack of detailing activities.

Specific recommendations and additions or deletions to the work scope

- This project should be considered for deletion. The Performing organization is not addressing the specific requirements of the Federal agencies being supported, and may in fact be impeding progress.
- Inquire with NHTSA on how to better support them with data, organizing meetings to bring consensus together. Better utilize funding to support NHTSA activities.
- Recommend the project be broken out and receive separate funding.
- Ensure appropriate codes and standards information is advocated by DOT in the GTR process.
- Make sure that the E1HP proposals are monitored and get global OEM input on how best to do the GTR; probably NOT using E1HP as the model. Keep in mind that what appears to be "lack of sustained domestic industry support at international technical committees" listed as a barrier is somewhat misleading. All the OEMs are global companies and input to the GTR process occurs at many levels and in many forums. Just a mind-set shift needed when discussing and presenting DOE work in this area. It might be worth briefing the C&STT on some of the ancillary activities, such as the funding for BP's station in Beijing, supporting the Olympics. What are we learning, etc.
- The objectives of DOE are unclear in this area. From this report, it appears that a lot of money is being spent to support minimal activity. It is understood that this project covers other administrative tasks not discussed. The lack of definition on the other tasks skews the perception of the accomplishments of this project. Rescope this task to address the administrative tasks, including GTR's, and relaxing the requirement for peer review may be justified.

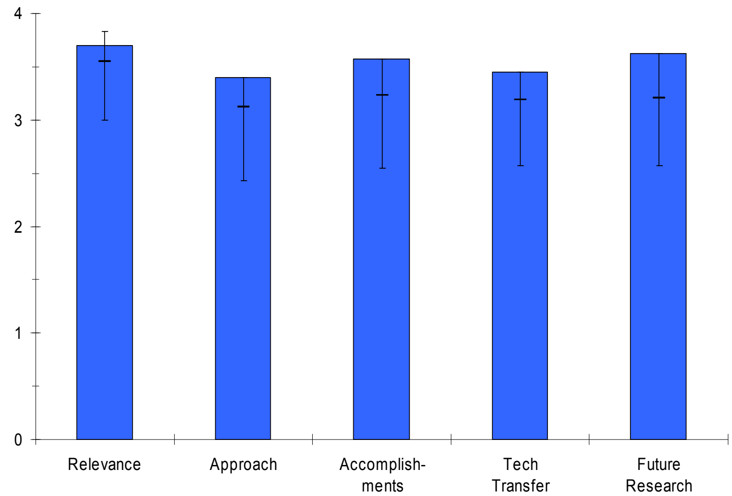
Project # SA-04: H_2 Incident Reporting and Best Practices Database

Bruce Kinzey; PNNL

Brief Summary of Project

The objectives of this project are to: 1) Establish a web-based system for open sharing of lessons learned from hydrogen incidents and near misses, and provide a confidential tool for reporting any occurrence of same; and 2) Provide a Hydrogen Safety Best Practices document to enable widespread benefit from the wealth of knowledge and experience already attained in industry, aerospace and elsewhere.

Overall Project Score: 3.6 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Incident reporting and best practices databases will be helpful in disseminating safety information and identifying gaps in codes and standards activities.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. Applied correctly, this Initiative could improve consumer safety across the board. Applied incorrectly or out of context, the results from this activity may hinder the transition to hydrogen as a transportation fuel.
- Everyone's experience must be shared. This could be one of the greatest resources for the Program.
- This project (database) is highly important for communicating lessons learned in and out of the Hydrogen program.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Databases are well designed.
- The approach appears sound but is missing one relevant component. The approach is to generate two public databases. The first is of industrial and commercial best practices. The second is of industrial and commercial hydrogen incidents. A similar activity should have been concurrently conducted on other commercial transportation fuels (petroleum, natural gas, etc) as points of reference and comparison.
- I am not convinced that the team has done everything necessary to get all organizations willing to share information. Consider searching out more organizations that have established H_2 related procedures and compare to further develop best practices.
- Approach is fairly solid, though it is not clear if voluntary input will capture all important issues.
- Sanitizing process may help to promote usage once those inputting information become more comfortable with it.
- No process currently to verify accuracy of input.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- Databases well designed, easily searchable, and already populated with existing data.
- The project has made very significant progress in a number of areas which meet the goals proposed and support complimentary projects.
- Considering the short timeframe of this project, progress has been excellent.

- Incident database has just launched.
- Project has only been running for approximately 6 months
- Pulling information from several databases.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- Not clear whether enough contacts are established to collect all incidents, also there may be some liability issues. Suggest establishing contacts with NTSB, PHMSA, and NHTSA incident reporting systems.
- The project consisted of drawing information from the H_2 safety review panel, privately submitted reports, and probably public news sources. The review panel is a diverse group drawn from government and industry. This in of itself demonstrates close coordination.
- Cooperation received so far is very good, but results next year will be a better indicator.
- Working closely with the Hydrogen Safety Review Panel.
- Participation from other organizations is voluntary.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- Continued enhancement and data collection recommended. Useful tool for safety planning.
- The further work plans generally address the objectives stated for this program. However, at the risk of scope creep, the lack of objective benchmarking of similar industries begets the question of how to interpret the information. Safety and acceptable risk are subjective by nature. Reference points are necessary for objective evaluation.
- Plans for future work are very good.
- Future work appears to be very relevant towards capturing lessons learned with respect to hydrogen incidents.

Strengths and weaknesses

Strengths

- The strengths of this project lie in the diversity and experience of the research panel and the extensive media sources in this country.
- Greatest asset is Web based sharing of information.
- This is a valuable tool for communicating incident related lessons learned.
- In a short period of time the project has come on line and information is being captured.

Weaknesses

- The weaknesses are the lack of reference to safety incidents in similar industries resulting in the potential to misapply the collected data to support a political agenda (i.e. Hydrogen is too dangerous, natural gas is safer, and gasoline is safer...) And the need to depend on voluntary reports on hydrogen incidents.
- Lack of process/procedure to maximize input from external organizations.
- Participation by those having incidents is voluntary.

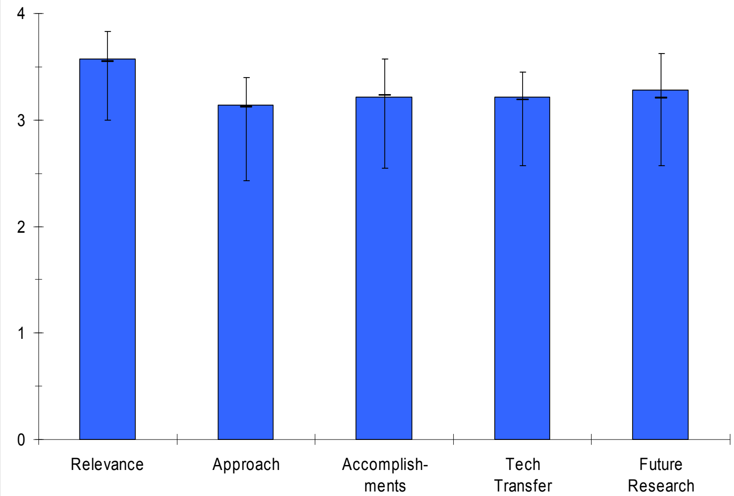
Specific recommendations and additions or deletions to the work scope

- Funding should continue to maintain and enhance data collection.
- A potential source of relevant information on hydrogen, natural gas and petroleum incidents might be OSHA. Many industrial and commercial incidents need to be reported in detail to OSHA.
- An additional barrier is the reluctance of an organization to share information on an incident or near miss, because it might reflect badly on them. Motivating all organizations to share all information will be difficult but important. Conduct a stakeholder workshop to bring out concerns of sharing information and addressing those concerns that might be beneficial.
- Continue to look for new sources of data to populate the database.
- Develop a process to protect input and non-sanitized information from FOIA requests.

Project # SA-05: Hydrogen Safety Review Panel

Steven Weiner; PNNL

Overall Project Score: 3.3 (7 Reviews Received)



Brief Summary of Project

The Hydrogen Safety Review Panel supports the DOE Hydrogen Safety Program, focusing on the development and implementation of practices and procedures that will help ensure safety in the operation, handling and use of hydrogen and hydrogen systems for all DOE projects. Bringing together a broad cross-section of industrial, government and academic expertise, the panel provides guidance and review of safety plans for project teams, conducts safety review site visits and telephone interviews and helps capture best practices and lessons learned for the benefit of the Hydrogen Program as a whole.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Safety is a must and the Safety Panel supports the mandate.
- Some day the technology may be sufficiently mature that this activity is no longer needed, but we are not there yet.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. Applied correctly, this initiative could improve consumer safety across the board. Applied incorrectly or out of context, the results from this activity may hinder the transition to hydrogen as a transportation fuel.
- Integrating safety into funded projects is extremely important, as any incidents causing injury or death in the DOE funded programs would have a negative impact on deployment of a hydrogen economy.
- Project strongly supports DOE goals and President's initiative and goals of the Hydrogen program.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- The approach to putting the Panel together should be revisited. Greater utility and benefit might occur if more independent specialists and experts were engaged beyond the industry experts. The board would then serve as a more effective third party review and sounding board. In too many cases the safety specialists from the industries performing the work are part of the panel. Oversights may be repeated both by the industry team and the safety review panel. The loss of FM global as a member is pretty severe.
- Site visits, phone audits and the two new Websites help with developing a safety culture.
- Reviewing safety plans and providing good examples is valuable to the community.
- The approach appears sound but is missing one relevant component. The approach is reviewing industrial best practices and applying them to commercial practice. A similar activity should have been concurrently conducted on other commercial transportation fuels (petroleum, natural gas, etc) as points of reference and comparison.
- Panel of experts is diverse and about the right size.
- It seems that some of the activities of this panel are duplicated by the NREL effort in the demo program. Both groups are collecting safety data. At least in the demo program, criteria for reporting leaks are set the by the reporting party, so how is the safety panel going to avoid a comparison of apples and oranges as they review incidents?

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- The project has produced some useable results and continues to make progress. However some of the accomplishments continue to highlight prior shortcomings of the effort.
- Seven on site reviews is a step in creating a safety culture.
- Progress is somewhat dependent on the willingness of others to cooperate, which appears to be good.
- The project has made very significant progress in a number of areas which meet the goals proposed and support complimentary projects.
- Panel has conducted a good number of safety reviews, and site visits, and apparently each funded project is required to submit safety plans for review to the panel of experts.
- Team has made very good progress in addressing multiple barriers with respect to hydrogen safety.
- It is important that lessons learned are communicated within the Hydrogen program.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Improvements to the team are necessary. The project is interacting with the DOE projects.
- Outreach with two new Websites is broad.
- This is really (and should be) a technology pass through project. The more the information can be shared the better it will be for the Program.
- The project consisted of a diverse review panel drawn from government and industry which generated a DOE data base on a number of items. This in of itself demonstrates close coordination.
- Unclear whether this project interacts with others directly or if this might be accomplished through another avenue.
- Not clear what the end result is. Need to answer the "So What?" question. Clearly the interactions with the PNNL Hydrogen Safety Program are important. But, is there a clearly articulated vision? What will result from the questionnaires, telephone interviews, etc.? How will best practices be rolled out? For example, under Equipment Maintenance and Sensor Calibration, the accomplishment contains nothing new.
- Great collaboration is apparent within the team in order to accomplish the amount of activities they have.
- Helping DOE to integrate safety planning into the Hydrogen Program.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Additional planned site audits and safety review will help ensure a focus on safety.
- Continuing this activity is very important.
- The further work plans generally address the objectives stated for this program. However, at the risk of scope creep, the lack of objective benchmarking of similar industries begets the question of how to interpret the information. Safety and acceptable risk are subjective by nature. Reference points are necessary for objective evaluation.
- Proposals listed under "Responses to 2005 Reviewer's Comments" should be implemented.

Strengths and weaknesses

Strengths
- On site safety audits, phone audits, and the two new Websites (incident reporting and best practices) help focus on a safety culture.
- Sharing of information.
- The strengths of this project lie in the diversity and experience of the Panel.

- Good overall effort and training for guidance for emergency responders. Review of safety plans for small groups new to H_2 is valuable.
- High level of expertise of team members.
- Diverse background of team members.

Weaknesses
- Safety was not a key take away at either the Plenary Session or the lunch addresses. The PI mentioned an audit was not completed due to a lack of funding - not a good message.
- The weaknesses are the lack of reference to safety practices in similar industries resulting in the potential to misapply the collected data to support a political agenda (i.e. Hydrogen is too dangerous, natural gas is safer, and gasoline is safer...)
- An annual report should be published and distributed throughout the program so that lessons learned can feed into future improved safety plans in each program area.
- Very important that the panel members are there to ensure safe use of hydrogen, not to look for opportunities for consulting contracts. Should not be sensationalizing H_2 risks any more than is done for gasoline or anything else. The accomplishments reported this year in many areas are really standard operating procedures that apply to any potentially hazardous process. Can the panel do more than that?

Specific recommendations and additions or deletions to the work scope

- Recommend safety be highlighted at the Plenary or a luncheon. May want an Award for best safety improvement. Lack of funding should not be mentioned at a public meeting.
- Generate similar databases on safety practices from other similar industries.

Education

Summary of Annual Merit Review Education Subprogram

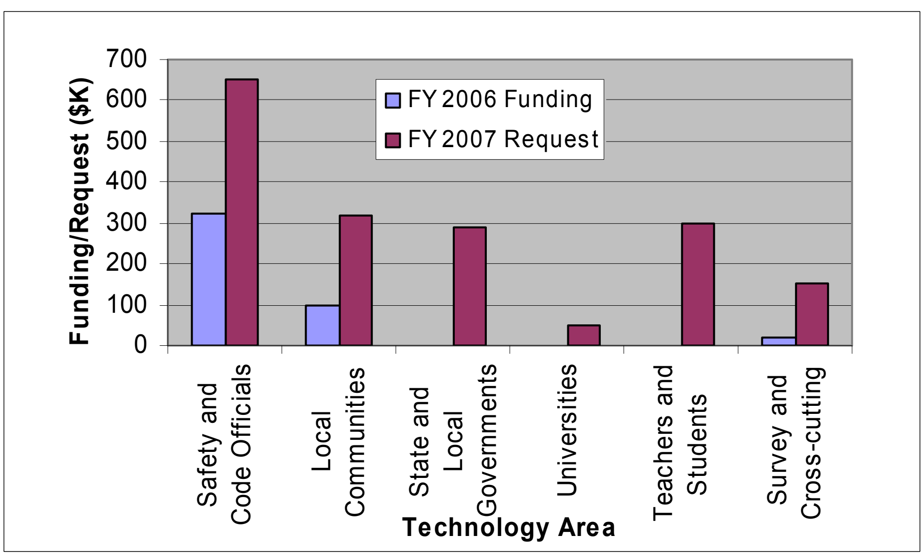
Summary of Reviewer Comments on Education Subprogram:

Reviewers felt that the hydrogen education subprogram is important to the success of the President's Hydrogen Fuel Initiative, and they commented on how the DOE Hydrogen Baseline Knowledge Survey results point to the need for a robust education effort to build the currently low levels of knowledge and awareness. Reviewers expressed continuing concern over the lack of funding to support education activities; they recognized that budget constraints have created a significant challenge and required difficult decisions regarding project funding.

Education projects funded this period focused on emergency responders and communities hosting demonstration projects. In accordance with previous merit review recommendations, both projects have connections to the safety codes and standards and technology validation subprograms. Projects to educate teachers and students were also included in the merit review despite not being funded this fiscal year due to subprogram budget constraints. Reviewers recognized these projects for their importance over the long-term and particularly for their efforts to find additional partners and cost share that have enabled their progress this year. Reviewers also commented on the need to focus Congressionally-directed projects more specifically on achieving DOE Hydrogen Program goals and avoiding redundancy with other efforts.

Hydrogen Education Funding:

The Education sub-program has prioritized its efforts to focus on target audiences involved in the near-term use of hydrogen technology. The fiscal year 2006 budget for Education included funds to support only two projects – an introduction to hydrogen safety for emergency responders and a community information project that focuses on areas hosting demonstration projects. Previously-awarded projects to create Hydrogen Technology Learning Centers at universities are closing out, due to lack of funding. Previously-awarded projects focused on middle school and high school teachers and students, although unfunded in 2006, will continue in fiscal year 2007, pending Congressional appropriations.



Majority of Reviewer Comments and Recommendations:

The reviewer scores for the Education projects reviewed were average, with scores of 3.5, 3.0, and 2.1 for the highest, average, and lowest scores respectively. The scores reflect progress made over the past year. Key comments and recommendations are summarized below. DOE will act on reviewer recommendations as appropriate to the overall scope, direction, and coherency of the Education effort.

Emergency Responders: Project shows clear relevance to the DOE Hydrogen Program and meets a critical near-term need. Good approach and progress so far. Coordinate with other entities and take advantage of the many outlets available for sharing this type of information.

Local Communities: Good project concept; a strong tool for the Program. Coordination with other entities will be critical to project success. Need to be focused and maintain realistic expectations for the scope of the project given ongoing budget constraints.

Middle Schools and High Schools: Important topic to address long-term need. Both projects have made excellent progress has been made despite lack of funding. Consider revising scope, as appropriate, given availability of funds. Focus on increasing availability of materials.

Community and Technical Colleges: Both efforts are important and relevant. Both projects are limited to certain states and should coordinate with others to avoid redundancy and expand reach. Linkages with other universities and organizations are critical.

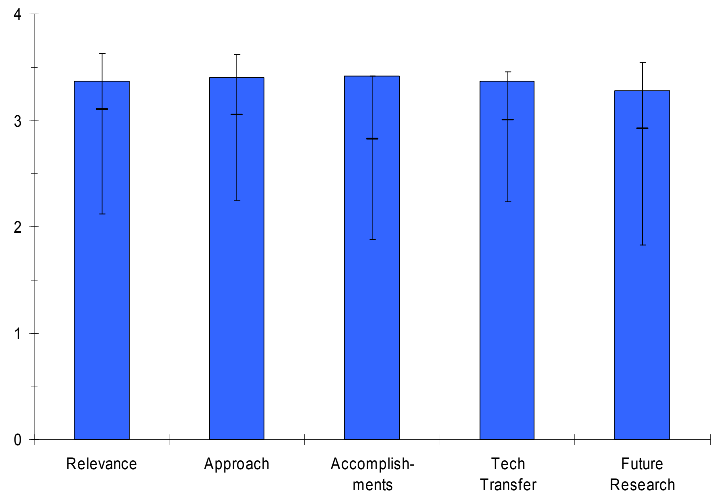
Project # ED-01: Hydrogen Technology and Energy Curriculum (HyTEC)

Barbara Nagle; UC Berkeley

Brief Summary of Project

The Hydrogen Technology and Energy Curriculum (HyTEC) project will develop and test hydrogen and fuel cell curriculum for high school students. The materials include student handouts, teachers' guides, kits for student investigations and experiments, and strategies for professional development of teachers. The project advances awareness of hydrogen technologies and the future path to a hydrogen economy throughout the United States.

Overall Project Score: 3.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Good relevance but presenter should address specific goals of DOE and how this effort connects.
- Curriculum development for high school teachers and students is very important to the goal of improving awareness.
- This fits squarely with the long-term education objective. Near-term to longer-term budget priorities are tough choices. Finishing the first module is important. It needs full commercialization.
- Highly relevant.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Good idea to put together a "Setup" Instructional Model.
- Good use of teacher input and curriculum developers. Good use of scientist input. Should do a better job on literature review (what is already out there.)
- Structured approach that has been significantly scaled back because of funding shortfalls.
- Approach is very mature and fits in a broader structure of science curriculum.
- The approach of showcasing the electrolyzer making hydrogen separate from the fuel cell is very applicable to real world transportation use.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- Good example of pilot testing in Berkeley.
- Good progress despite funding shortfall. Good use of pilot testing to develop materials.
- Good progress.
- Pace of project is tailored to student learning curve timing - end of school year. This is an understandable barrier to faster progress. There is a window for once a year pilot testing.
- Excellent job of prioritizing work effort given funding limitation AND showing progress. Integrating video clips into curriculum is excellent.
- Project is progressing well despite two-year lack of DOE funding. Feedback from teachers and students using curriculum is positive.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Need to determine a plan to make the materials publicly available at end of the project.
- Working well with teacher groups. Good set of partners.
- Teacher workshops are a great way to share what's available.
- Good collaboration.
- Working with teachers, curriculum developers, engineers, and project partners – and the field trips to AC Transit - brings the real world to students.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Pre- and post-testing will be critical to determine effectiveness of materials.
- Would like to see a fully field-tested module in a variety of settings.
- The project team has effectively thought out future efforts for several funding scenarios. Additional modules, national field testing is well-focused.
- The project partners have mapped out a strategey to continue and expand the project regardless of DOE funding.
- Good plan.

Strengths and weaknesses

Strengths
- Good, well thought out plan for hydrogen education.
- Continued work despite lack of funding. Good use of expert input. Good use of sequencing in the class development.
- A very strongly integrated approach. The review of what was on the market was a key step. The recycling of Lab-Aids royalties speaks very well to commitment.
- Excellent and enthusiastic presentation and delivery of results.

Weaknesses
- More information should be given regarding the approach.
- Materials and kit development are proprietary. This does not allow for easy duplication by others. Should be more explicit about how this program is better than existing materials. Should address the question about hydrogen production methods that will be covered.
- Not enough funding for this project.
- Expanding project nationally may be challenge with support and funding.

Specific recommendations and additions or deletions to the work scope

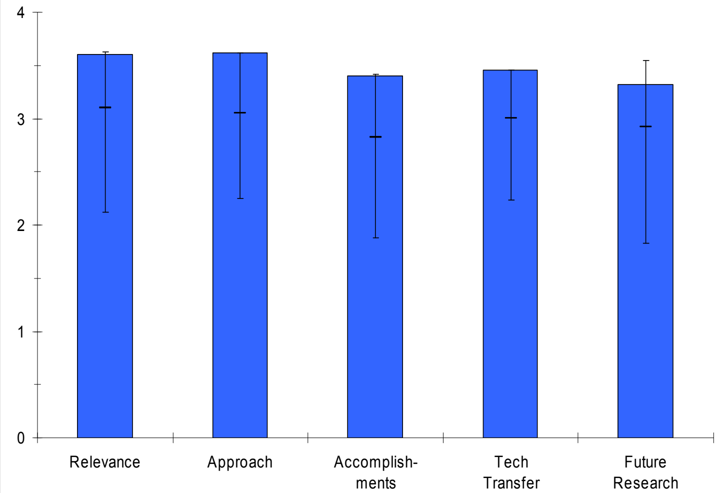
- A science subject area seems expansive (and impressive). With reduced scope, it might be necessary to revise the areas. Should evaluate which science subject areas are best suited for the program. Understand which work best. Consider shifting development towards publicly available materials and guidelines for equipment purchase (as opposed to equipment development).
- As noted by the speaker, including discussion of numerous production technologies in curriculum materials is a good idea. Continue to seek feedback on material/products being developed.

Project # ED-02: H2 Educate!
Mary Spruill; NEED

Brief Summary of Project

The National Energy Education Development (NEED) Project is working with DOE on the H2 Educate project designed to provide hands-on kits, classroom curriculum, and teacher training on hydrogen and fuel cells. The project will reach a network of classrooms with one-day workshops, week-long conferences, and strategic partnerships around the country. The project is targeted at middle schools, but also has applications in the elementary and secondary schools. The program and materials were designed by teachers, for teachers, with support from SENTECH, Inc., the National Hydrogen Association, Los Alamos National Laboratory the U.S. Fuel Cell Council, NYSERDA, the Fuel Cell Store, and State Energy Offices.

Overall Project Score: 3.5 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Need to better articulate how these materials relate to the DOE education goals.
- This project fits the long-term education goal but is not a near-term goal. This is a tough budget situation.
- Important topic at the right time.
- Focusing on middle schools is highly relevant.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- Need to better explain how this program differs from competitive materials (lit review.) Objectives should be more quantitative, tied to DOE education goals. Asking people what they want to know about hydrogen is not necessarily the best way to understand what needs to be taught. Good use of state standards to match against program. Good use of "Train the Teacher."
- Needs network of teachers helps ensure maximum reach.
- Appropriate level for middle school. Key messages thought-through. State level standards engaged.
- Obtaining data on whether the project is working and the relevant to teacher standards are two important aspects of the approach.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- 550 modules deployed, 1000 print materials deployed. Need additional quantitative metrics to show progress.
- Record over 2,800 teachers' projects and a broad scope/range.
- The supplemental funding speaks volumes. Glad to see that "it's done." Project moved ahead of schedule with adjustments as needed.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- Good partnering with industry (BP) to provide materials.
- Expanding to other classes (besides science) very useful. A report on what has been learned by teachers and students who participate will be extremely valuable.
- The unexpected broader use beyond science classes is a strong advantage. The annual teacher workshops give great long-term life to the work.
- Ability to get funding from others speaks to worthiness of project and effectiveness of the collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Need to be clear about what you are trying to measure in terms of success metrics. Not clear what is planned going forward.
- Project is nearly complete and materials are available on web. Good closeout.
- Not research.
- Moving ahead in spite of zero budget for future programming.

Strengths and weaknesses

Strengths
- Progress on project despite funding shortfall. Good anecdotes of success (but still need key metrics).
- Program addresses all pathways of hydrogen production as well as all of the different fuel cell applications, and ensures that the curriculum fits ties to national and state standards.
- Lots of outreach and workshops. This is super. The project highlighted the "up from scratch" level of hydrogen education in the U.S.
- Focused program – target audience seems to be engaged. Feedback survey appears to be a good way to gauge success of program. Scope of program appears to cover most forms of hydrogen production.
- Excellent presentation. Delivered with enthusiasm.

Weaknesses
- Presenter mentioned effect (30% increase in understanding), but it's not clear what this means.

Specific recommendations and additions or deletions to the work scope

- Add a section to gauge perceptions of hydrogen (positive/negative).
- Need to benchmark these materials with other available materials. Show the specific value added.
- Project is well focused on middle school students, but nonetheless it might be worth investigating whether some interaction with ED-01 Nagle project which focuses on high schools could provide some benefits.

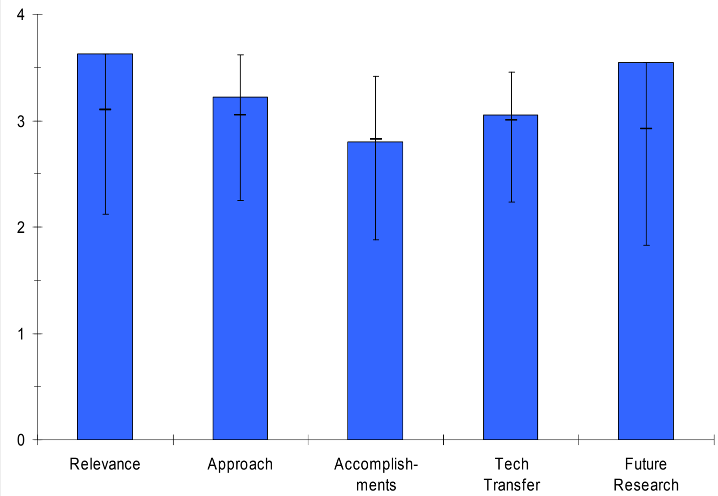
Project # ED-03: Hydrogen Safety Education and Training for Emergency Responders

Bruce Kinzey; PNNL

Brief Summary of Project

The objectives of this project are to: 1) prepare emergency responder and other related safety communities for near-term hydrogen demonstrations and the long-term hydrogen economy; 2) develop hydrogen safety educational materials for first responders and code officials that can be used alone or "dropped in" to a wide variety of existing training activities, depending on the needs of the audience (must be accomplished in close collaboration with relevant hydrogen and safety communities); and 3) complete and release awareness-level educational materials for emergency responders who must be able to recognize a hydrogen incident and take initial protective measures.

Overall Project Score: 3.2 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Clear relevance to the goals of the DOE program. Safety official training is a clear near-term need for demonstration and deployment.
- Training and education of first responders are critical to the safe introduction of hydrogen applications.
- Training of emergency responders is a key step to fear reduction of hydrogen. A trained cadre of first responders is very important.
- This is an extremely important area where synergy exists between Education and Safety, Codes & Standards. It's fantastic to see DOE funding a cross-cutting project like this, especially in a year of such reduced Education funding.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Good process description. Good description of expert input.
- Good approach.
- 2008 for full complete set of materials is a good pace. Good tie to Safety, Codes and Standards and to an existing facility. The broad stakeholder input and review strengthens the finished product. This is probably the first key audience of Hydrogen Education.
- Since the project is not very far along, it's hard to rate this higher. In theory though, the planned approach looks good.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Pilot tests, information collection, revision process well described.
- Good progress.
- Glad the Awareness-Level is almost finished. This module should have broader audience appeal.
- Scored lower simply because there was not yet much progress reported.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- Good partnering with HAMMER. Good plan to make the materials publicly available.
- Outreach to target audiences has been well-received.
- A good set of partners for further collaboration efforts will be needed for acceptance. Need the state fire marshals. Web materials are needed quickly.
- The project talks about how this information will be disseminated among the intended audiences, but not how the training materials may be shared with colleagues or other institutions which may be interested in using this material. Very vague mention as to who the information is being shared with and other partners.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- Good description of products that will be released by FY06.
- Good plan.
- The completion of the whole project will provide a full set of training materials.
- What needs to be done next is obvious and well spelled-out. I look forward to seeing more progress.

Strengths and weaknesses

Strengths
- Good partner with HAMMER. Good articulation how this project interacts with other research activities
- The web-based approach is a way to reach very broadly.
- The concept of this project and its cross-cutting value is significant. This project addresses well-known needs in a way that looks like it will be very effective.

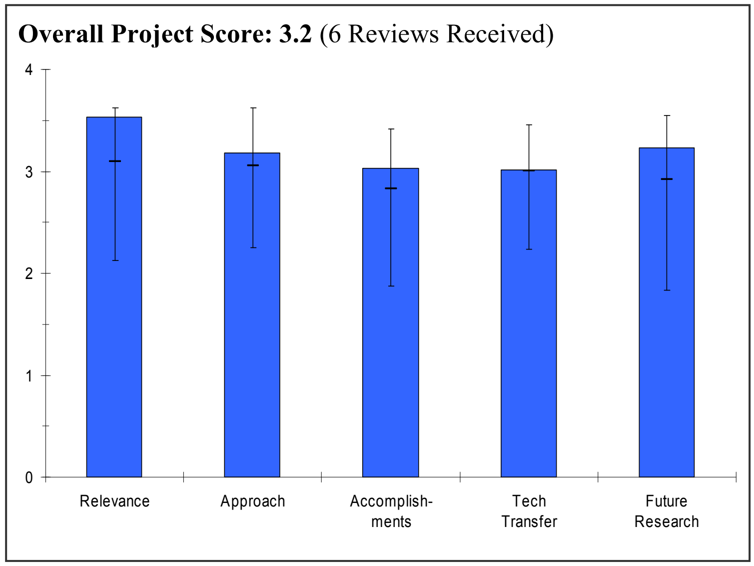
Weaknesses
- Presentation should be clear about value added by this activity (what was done by this vs. other organizations). Presenter should include some discussion of feedback received from the pilot testing.
- The project is in its beginning draft stages, so it's hard to make judgments . I look forward to hearing more about how the technology transfer with other organizations is planned to occur. There are many outlets for sharing this type of information. The PI should mention and take advantage of them.

Specific recommendations and additions or deletions to the work scope

- Don't let the web review process bog this down.
- Should interact with other international activities currently looking at safety issues (DOE, EU, JHFC, etc.).
- There are other projects relevant to this at University of Montana, CaFCP (their training materials) and Hydrogen Safety does some of this as well. If not already coordinated with them, it might be beneficial.

Project # ED-04: Increasing "H_2IQ": A Public Information Program

Henry Gentenaar; The Media Network



Brief Summary of Project

The objectives of this project are to:
Develop and disseminate resonant messaging that communicates to the general public basic facts about –
1. Hydrogen as a fuel/form of energy
2. The coming hydrogen economy
3. The DOE Hydrogen Program and Hydrogen Fuel Initiative
4. Generate interest and increase public requests for more information
5. Give the Hydrogen Program a communications mechanism with a flexible framework for reasons of timing and budget
6. Support the DOE brand
7. Help position the Program and Initiative in the mind of the public
8. Build recognition of hydrogen and government efforts to build the hydrogen economy
9. Make the most of DOE resources and provide a gauge of success.

These objectives are planned to be accomplished using print, radio, television, satellite, and new forms of media. The project will coordinate closely with the Technology Validation Sub-Program and focus initially on Hydrogen Learning Demonstration project areas/locations. The primary target audience is the general public.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Education is important, but it is critical that accurate technical language is used. Critical that message is realistic as well as truthful and accurate.
- This project ties directly to the Hydrogen Education Baseline Study.
- If successful, should raise the H_2IQ of general public. Given the high level of misinformation currently being put into the media, this activity will be critical.
- Important topic to get H_2 education into media.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The sound-bite mentality has a high likelihood of inaccuracy and misunderstanding.
- Well thought, thorough approach to huge audience - the general public. Mature thinking of approach shines.
- The Communications Blueprint and The Program Model provide excellent long-term approach. Simple positive messaging approach is excellent.
- Impressive in scope, not clear that such a plan is possible without significant funding (beyond what is currently available). Need to be careful about over promising, maintain trust.
- Good concept, but slide hard to read.
- Very balanced and approach all media included.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Project just started. Technical experts with experience with general public education on hydrogen must be consulted on the accuracy and understandability of the messages.
- This is a new project. It is well crafted for this stage.
- Progress limited by recent start date but progress not totally apparent to reviewer based on Slide #9.
- Good start, but need more information on what are the top priority messages.
- More clarity should be given on technical accomplishments.
- In short time program has been running its seems to have accomplished several key objectives.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Just beginning to look for partners. Need to engage education experts, scientists and engineers, and social scientists.
- Collaboration will take a lot of work. This will need a future focus.
- Much is going on in industry and academia. Need to understand how these messages fit in with what is already out there.
- Outside organization will ensure that there no overlap with other activity out there.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- Has potential for success with appropriate participation.
- Looking forward to what is coming in 2006, 2007 and beyond.
- Budget is very limited and, therefore future efforts need to be very focused and based upon realistic expectations as to what budgets might be available for this work.
- Ambitious future plans. Future plans need to articulate priorities for message and medium.
- Project seems to be optimistic with strategies and the goals do not seem realistic considering the amount of funding.

Strengths and weaknesses

Strengths
- A very well thought out approach. Looking forward to its launch. A prediction - this will become a very strong tool for the Hydrogen, Fuel Cells, and Infrastructure Technologies Program.
- Enthusiastic speaker.
- Using modern technology (I-Pods) and conventional media means (TV).
- Reasonable statements within the "think future" scenario (good target).
- Strategy specific to hydrogen and fuel cells.

Weaknesses
- Metrics beyond web hits and calls to the Information Center will need to be developed.
- Amount of funding doesn't seem realistic based on plan.
- Need to articulate a plan that can be executed at different funding levels (prioritize).
- Message needs to be concise. Good concepts, but unclear from presentation how the subjects were presented.

Specific recommendations and additions or deletions to the work scope

- Additional funding recommended.
- Make sure to document “success stories” especially in situations where you have to respond to “misinformation.”
- Presentation slides are very complex. Work on communication.

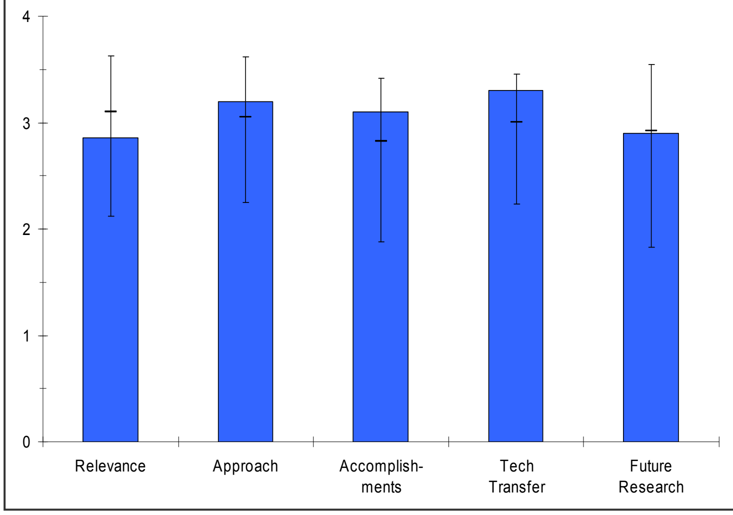
Project # EDP-01: Hydrogen/Alternative Energy Center

Ruth Borger; Lansing Comm. College

Brief Summary of Project

Lansing Community College (LCC) is focused on increasing the number of technicians available to service and maintain hydrogen equipment by providing an open and accessible lab in which students can learn about hydrogen fuel cells and alternative energy. LCC is also building links between businesses and industry and educational institutions. Its goal is to become a resource center for educators, innovators, and policy makers.

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- Relevance very focused on State of Michigan and specifically the Lansing Community College.
- Educational efforts are important. Flexibility of course materials will be critical, given the lack of commercial products.
- Generally aligned with the President's Initiative. It certainly is important that technicians be trained to work on and repair these emerging technologies.
- This project is a critical groundbreaker. Creating this curricula will not only address technical knowledge areas, but will provide positive outreach as well and start to build a base of knowledgeable technicians.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Scattered over too many (only slightly related) activities.
- The curriculum being developed to train technicians or as a bridge to a four year institution seems reasonable. It is not clear that the vehicle transporter and assortment of vehicles is a significant contributor to the education process.
- Area focused on (training technicians) is desperately needed. Very thorough method of approach.
- Logical, straight-forward approach.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- Curriculum development and progress in educating students is to be applauded.
- Progress in curriculum development is good. Outreach activities are varied.
- It is encouraging that most of the courses have been taught at least once and these courses are making a difference in students finding jobs.
- Program has met targeted goals.
- It appears that the project is moving along according to the execution plan.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Good set of universities.
- The collaborations with universities are good, but the partnerships need to be expanded to include more industry. This expansion should include fuel providers, fuel cell suppliers and other automotive component suppliers. They could be brought in as members of an advisory committee and hopefully would see value in donating or loaning equipment.
- Projects collaboration with other local universities with similar fuel cell degrees or vehicle programs bolsters strength and reach.
- Collaboration with the other institutions and companies is not only essential, but looks to be quite comprehensive. I would be very interested in knowing as follow-up, which schools built on this project and to what extent companies like GM were able to take advantage of students who graduated with this degree.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- The need/objective of using a hydrogen fueling station to support the education of technicians is a worthy one, but they should seek collaboration with existing/planned Michigan fueling facilities to accomplish this objective.
- Completion of the curriculum is important.
- Continuing course development is good, but plans to procure a hydrogen refueling station are not worth the high cost. This is another area where they should search out a partnership for industry to obtain a training platform.
- Program provides substantial room for growth and success. Many possible routes and partnerships available in location.
- The plan for finishing this project and looking ahead looks logical. I have a slight hesitation about whether a fully dedicated fueling station would need to be built for an extension of this project, however a slightly modified approach might be beneficial – find a locally convenient site where a fueling station is needed for other reasons (fleet fueling, auto company, etc.) and construct one with the appropriate partners so that students have access to it for training. Expanding the technician training to include fueling stations is logical. I wonder if the industrial gas companies and energy companies see the same need for trained personnel as the auto companies do.

Strengths and weaknesses

Strengths
- Courses are being offered and students are being employed!
- Linkages with universities are very good.
- Working with local universities and organizations (NextEnergy) provides great rewards.
- Basic, straightforward approach to a known issue. Good progress so far.

Weaknesses
- Limited to Michigan.
- Focus on high-visibility items, such as the vehicle transporter and hydrogen refueling station, may detract from the educational process.
- None identified.

Specific recommendations and additions or deletions to the work scope

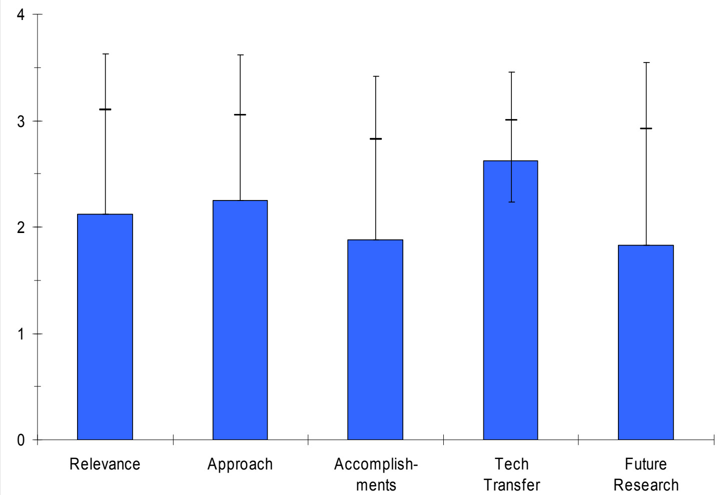
- Concentrate remaining financial resources on building partnerships with industry. Show the value of educating technicians that will work for them or service their equipment. Build training partnerships, possibly even performing some of the class/lab work in an industrial setting of the partner. EDP-01 and EDP-03 should form a collaborative and jointly expand their curriculum.

Project # EDP-02: Shared Technology Transfer Project

John Griffin; Nicholls State Un.

Brief Summary of Project

Overall Project Score: 2.1 (4 Reviews Received)



The overarching goal of the Nicholls State University project is to establish a collaborative process with domestic industries for the purpose of sharing Navy-developed technology. The purpose is to educate private sector businesses and increase their awareness of available technologies, with an initial focus on technology applications that are related to the DOE Hydrogen Program. NAVSEA-Carderock and the Houston Advanced Research Center are teaming with Nicholls State University to catalog NAVSEA-Carderock unclassified technologies, rate the level of technology readiness, and establish a web-based catalog of the technologies. The catalog contains technology descriptions, including testing summaries and overviews of related presentations, and an evaluation of the technology readiness.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.1** for its relevance to DOE objectives.

- This project does not directly support the President's hydrogen initiative. This is a technology transfer project geared towards identifying patents that could be relevant to industry. It is not geared towards advancing hydrogen technologies, nor educating the public and other stakeholders as to the opportunities for hydrogen.
- Very focused on Navy technology, but hydrogen appears to be an after thought.
- This is a problematic project. It has not aligned itself to the HFCITP Program. The presenter did not show up at conference.
- The arguments given for this project appear to be well-aligned with the goals the President's Initiative and the HFCIT Program

Question 2: Approach to performing the research and development

This project was rated **2.3** on its approach.

- Although this project could yield some useful information, it is unclear if the team has the technical expertise to draw the relationships between the various patents and their potential relevance to hydrogen technologies. The example shown in the presentation as a relevant technology is concerning, particularly the claim relative a hydrogen cost of <$2 /kg. No basis is provided for this claim.
- Rather than an education effort, this appears to be a consulting activity to match technical needs of the Navy with local industry. The Integrated Technology Manufacturing Initiative has performed a similar activity for DOD with far less funding and developed a database which is much larger.
- The approach to bringing out U.S. Navy technologies is a good one. It needs to target hydrogen and fuel cell technologies better.
- Some parts of the approach seem well thought out and straightforward like the cataloguing and the creation of the website. The relationship between the students and the outreach to the private sector was confusing from the slides/poster.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **1.9** based on accomplishments.

- The cost of this project is very high relative to the scope. The database architecture, including the catalog categories and technology maturation codes, should make the database useful. Seems to have completed objective of cataloging 600 patents.
- A database has been developed, but the accomplishments do not match the level of funding.
- How many hydrogen technologies have been identified? How many fuel cell technologies have been identified? These are unknowns from the poster.
- It's hard to measure progress unless you can quantify how many industry professionals felt there was value in being educated about the different technologies. The website is extensive, but does not communicate well. As someone who is familiar with underwater technologies like the ones discussed, I often did not understand what the technology did or the relevancy to the hydrogen program. Perhaps those in the private sector would have this same issue.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- Good effort is being made to ensure that the database will be visible and to alert the technical community of its availability.
- Technology matching may be good, but without the PI to answer questions, I could not verify.
- There is a need for saltwater technologies to be identified for use in saltwater wind to hydrogen applications. None were shown.
- Looks like a good team. Improving the ability of the website to communicate would greatly increase the opportunity for this database of information to be useful to other audiences. I think one of the main points of this project is to help little-known technologies become better-known to companies that can use them. Therefore, the scope of the outreach and the ability of the website to communicate would greatly increase the technology transfer.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.8** for proposed future work.

- Although the planned efforts for tracking the relevancy of the database and for measuring inquiries is good, it is unclear whether the technical expertise exists to proactively identify opportunities for applying the technologies.
- Future work consists mostly of more meetings.
- This project is in the wrap-up stage. The opportunity for hydrogen/fuel cell identified technologies appears to have been missed.

Strengths and weaknesses

Strengths
- The strongest point of this effort is providing the linkage between the Navy and local industry.
- Great database of untapped information that could prove to be very useful if discovered by the right persons/ companies. Innovative concept.
- As far as database development and understanding the naval patents in their intended application, the team seems well qualified.
- A good approach is used to rating the market readiness of the identified technologies.

Weaknesses

- Based on poster language, technical expertise in hydrogen production is questionable. Website does not communicate clearly.
- The project provides little, if any benefit to advancing hydrogen technologies and supporting the President's Hydrogen Initiative. There is no benchmarking to assess the real potential of technologies and there appears to be limited technical expertise being applied to the valuation of technologies.
- The accomplishments and future work do not justify nearly $1M in funding.
- This project is not recommended for further HFCIT funding.

Specific recommendations and additions or deletions to the work scope

- Project representative should be available to address questions.

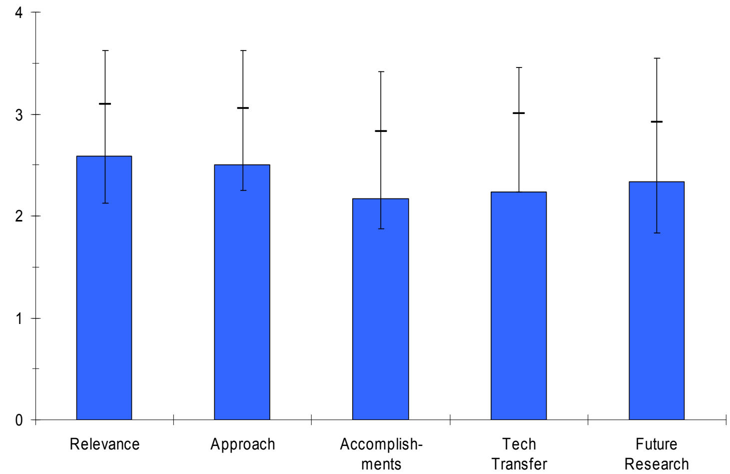
Project # EDP-03: Montana Hydrogen Futures Project

Paul Williamson; U of Montana

Brief Summary of Project

The Montana Hydrogen Futures Project includes: a new web-based curriculum for Alternative Energy Technicians' one-year certificates and two year associate degrees leading to an Energy Engineer Degree; a hydrogen education website (www.H2education.com) for students and teachers of all ages; an Alternative Energy and Hydrogen Safety Training Site to provide safety training and certification; and planning for the Hydrogen Futures Park to house a sustainable college campus.

Overall Project Score: 2.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- While a pilot education program in Montana setting up a college curriculum for hydrogen education may be a good thing and a web site will reach more than Montana residents, this work does not appear to be critical, and in many cases it is redundant.
- This project appears limited to Montana and it is not very clear how it will help foster Montana's hydrogen economy.
- Generally aligned with the President's Initiative. It is important that technicians be trained to work on and repair these emerging technologies.
- Although educating the next generation work force on hydrogen technologies will be important, this program seems more focused on infrastructure upgrades at the University. It should focus on curriculum development that can be replicated nationally. Although safety training is extremely important, it is not clear that the team is qualified to create a safety training center.
- Great relevance to the hydrogen program; this project addresses key areas that need more activity. The cross-cutting issues that this project tackles between education and safety, codes and standards are fantastic

Question 2: Approach to performing the research and development

This project was rated **2.5** on its approach.

- I believe that more needed to be done to ensure that the work was not going to be redundant. For instance, is a hydrogen safety training center at the University of Montana necessary if there is going to be one at HAMMER? The question that must be asked is: "What is being proposed/performed here that is value-added and just not another website or education program?"
- The approach is worthy but not very clear as to how this project will "support the creation of a Montana hydrogen economy and stimulate national progression…."
- The approach has been refined, but it is still searching for a niche that needs to be filled.
- Would benefit from greater emphasis on benchmarking of technology status, related programs at other institutions and need. The current hydrogen industry members are the leaders in hydrogen safety and hydrogen safety training, yet there appears to be no effort to involve these experts in the design and implementation of the hydrogen safety center and its curriculum.
- Straightforward approach. Could be better documented in the presentation but overall, the path is well defined and set-up for success.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.2** based on accomplishments.

- It cannot be expected that much other than planning be done when there are four topics and only about 750K available, so it seems that they are making some progress. I looked at the website, however, and there does not seem to be much there that can't be found elsewhere. Much of the material is redundant with information from DOE, NHA and many others. There are a lot of useful links here, but I don't see any uniqueness. One disturbing thing is that there is a plethora of "Hydrogen News" referenced over the time period June–October 2005 and then nothing after that. A website that looks like it is not being kept up to date gives one the feeling of being defunct, and is perhaps worse than no website at all.
- H_2 Education Website is a worthy accomplishment.
- Progress has been slow, but this has benefited the approach. It appears that this project lacks internal support and may not be sustainable.
- Although the project was scheduled for completion in April 2006, the work is not yet complete. It appears as though the project tried to take on too much. It would have benefited from a narrower focus.
- Good progress made in developing the materials and coursework. Website looks great and is easy to navigate. Looking forward to seeing the curriculum. Is the project 80% complete if the curriculum is not completed and the Safety Program is not yet completed? May need more funding to finish.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.2** for technology transfer and collaboration.

- They have contacted the right people, but it seems more an act of collecting data than of a real collaboration.
- Having interaction with noted partners is fine but if project is "designed to foster the infrastructure needed in Montana's Hydrogen Economy" the reviewer questions whether the project has the right set of partners.
- The project is working with others to improve its value.
- This project would have benefited from stronger ties to other curriculum development efforts. Should have leveraged more external expertise. Unclear how much of the curriculum developed has been vetted by the technical community.
- Plans for collaboration could be better articulated. I question the claim of 50 presentations made in Montana. Over two years, that's a presentation every-other week (and to an average audience of 60 people)? If it's true, that's great but supporting information would be helpful.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- The future plan, assuming there is money (this present contract is finished,) calls for completion of what has been started. This is always a logical approach.
- Creation of a Hydrogen Safety Center is worthy but unfocused.
- At the current level of internal support, it is not clear that many, if any, future plans will be carried out.
- It is unclear how curriculum will be maintained and revised as technologies mature. Again, the lack of involvement by industrial hydrogen safety experts in the hydrogen safety program development is a concern.
- See technical accomplishments/progress. Otherwise, clear and straightforward approach to finishing this project.

Strengths and weaknesses

Strengths

- Teaching more people - the public, energy technicians, school children or fire marshals - about is a good thing.
- Serving Montana's interests.
- Linkages with other educational institutions.
- Project addresses well-known needs in a straightforward manner. Scope is broad but achievable.
- Good efforts related to outreach in the Montana community.

Weaknesses

- Redundancy with other projects. While redundancy is good on a technical project where different approaches may lead to a more efficient or less expensive product, this is not necessarily true in the education arena. Quality must far outweigh quantity. There is not a lot of value added here. A website that features old news is worse than no website at all.
- A number of aspects of this project seem to have "the cart before the horse" when it comes to the development and support of a Montana hydrogen infrastructure and plan toward a hydrogen economy.
- Lack of internal support.
- Need to wait for DOE National efforts. Coordinating with DOE and DOT does not mean creating own curriculum without their input.
- Presentation documents could include more detailed information on progress and achievements.
- The project appeared to take on too broad of a scope and would have benefited from a focused effort in one or two areas.
- Appears to have been limited involvement only from external experts.

Specific recommendations and additions or deletions to the work scope

- If there is to be further federal funding here, it would be good to focus on one item and get it done rather than partially develop several. Care should be taken to avoid redundancies.
- Recommend utilizing DOE Emergency Response materials and not trying to create another effort.
- Plans for continuation of this effort should be aligned with the school's mission and goals to assure full support from the school. EDP-01 and EDP-03 should form a collaborative and jointly expand their curriculum. Also, identifying a niche and tailoring the curriculum for that niche would be beneficial.

Systems Analysis

Summary of Annual Merit Review Systems Analysis Subprogram

Summary of Reviewer Comments on Systems Analysis Subprogram:

The reviewers considered the Systems Analysis Subprogram essential component to the Hydrogen Program mission and critical to the President's Hydrogen Fuel Initiative. The projects are considered appropriately diverse and focused on addressing technical barriers and meeting targets.

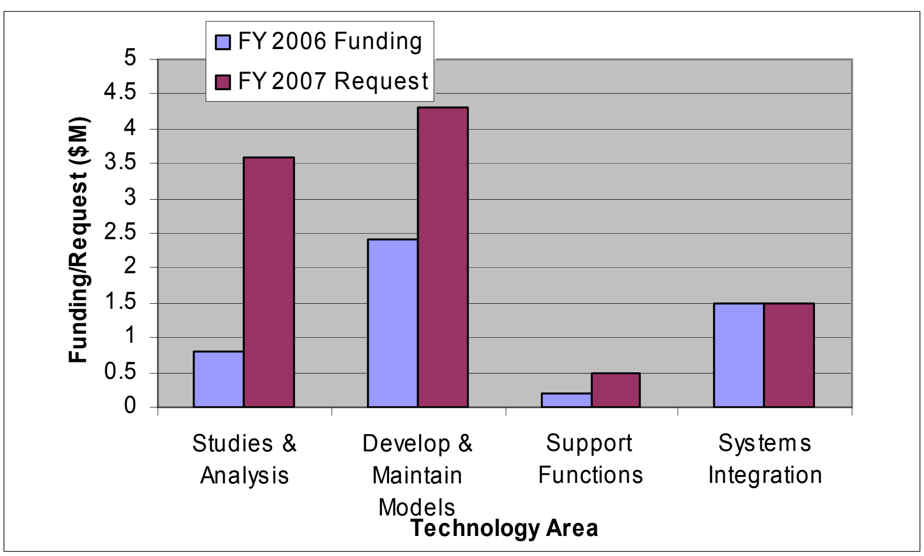
In general, the reviewers noted that Systems Analysis is a complex subprogram but is receiving the appropriate management attention. Some reviewers commented that the subprogram is well managed and has adopted an organized approach for analytical support of the Hydrogen Program, which is consistent with addressing the comprehensive list of identified barriers.

The major concerns identified by the reviewers for Systems Analysis were: 1) coordination and cooperation with the other DOE offices, e.g., Biomass, Solar and Wind, is required; 2) the plan for how the analytical parts fit together should be defined; 3) roles of the various models should be described; and 4) a summary of the common set of inputs and assumptions used for the modeling should be provided. The Systems Analysis subprogram addresses these issues in the Systems Analysis Plan which is soon to be issued.

Finally, the reviewers commented on the need to understand the international drivers and factors impacting a hydrogen economy. The models and systems should evolve to include the analysis of the impact of the world economy on the U.S. fuel systems.

Systems Analysis Funding:

The funding portfolio for Systems Analysis primarily addresses the model development and required analysis to support the Technology Readiness Goal. The requested 2007 funding profile, subject to Congressional appropriation, addresses the National Academies' Report recommendations and provides greater emphasis on the transition analysis.



Majority of Reviewer Comments and Recommendations:

In general, the maximum, minimum and average scores of the reviewers of the Systems Analysis projects were 3.7, 2.7 and 3.1, respectively. The Systems Analysis project portfolio includes a mix to address the "analysis and modeling gaps" of the subprogram and the transition requirements. The major recommendations for the Systems Analysis projects are summarized below. DOE will act on the reviewer recommendations for the overall Systems Analysis effort.

- **Hydrogen Production Infrastructure Options Analysis Project:** Consider adding actual supply and demand data from gasoline station performance to answer questions of committing overcapacity/supply without policy incentives. Emphasis should be directed to analyzing the advantages/disadvantages of different pathways that minimize capital risk early in the transition.

- **Impact of Hydrogen Production on U.S. Energy Markets Project:** Focus on the linkage and integration with other models to insure consistent inputs, outputs and assumptions are being utilized. Introduce plug-in hybrid technology in the analysis of alternative pathways for energy security.

- **Analysis of the Hydrogen Production and Delivery Infrastructure as a Complex Adaptive Model Project:** Considering the complexity of the modeling approach, incorporate a broader advisory group to enhance the model applicability and adaptability with other models. Ensure the agents representing industry include risk profiles, spending practices and business goals. Introduce methodology to analyze policy implications.

- **WinDS-H_2 Model and Analysis Project:** Ensure this model is incorporated in the Macro-System model architecture. Focus on adding demand forecast information and model output in the model as a next step. Consider the addition of gasoline hybrid and plug-in hybrid technology in the modeling structure.

- **Macro-System Model Project:** Ensure common and consistent assumptions and inputs are utilized in the linked models. Emphasis on a coherent summary report product from the model is required.

- **Hydrogen Transition Modeling and Analysis:** HyTrans v.1.2: Ensure the model analytical capabilities include plug-in hybrid and other alternate fuel vehicles.

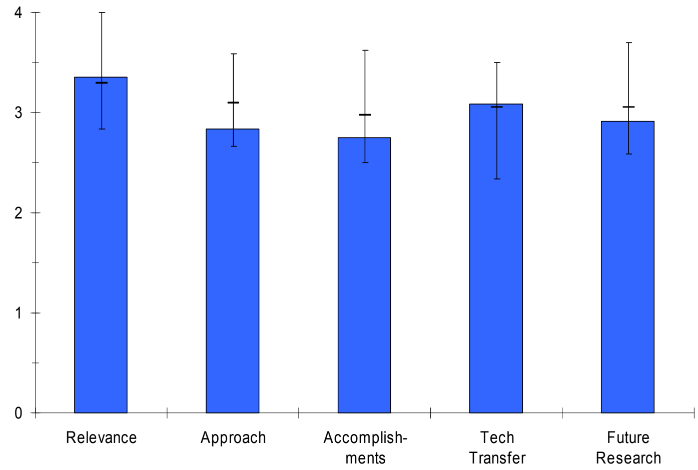
- **Hydrogen Analysis Resource Center (HyARC) Project:** Focus on continued maintenance and updates to the resource center. Introduce international data and information as a resource. Consider adding safety and education information/data to the resource center.

Project # AN-01: Hydrogen Production Infrastructure Options Analysis

Brian D. James; Directed Techs.

Brief Summary of Project

Overall Project Score: 2.9 (7 Reviews Received)



The objectives of this project are to: 1) Create an analytical tool robust enough to assess the impact of different assumptions on hydrogen infrastructure development; 2) Exercise the tool using various assumptions to understand the infrastructure's sensitivity to different scenarios; and 3) Suggest to DOE areas of further research based on the most influential parameters in the infrastructure development. The unique features of this model include: its ability to evaluate infrastructures with varying utilization over lifetime; its ease of use; an interface aimed for use by a wide audience; a structure that can be incorporated into the Macro Model; the allowance for investor demand foresight; incorporation of stranded asset logic; user input of yearly varying hydrogen demand, unit efficiencies, and capital costs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Analysis of the infrastructure options is important to planning the H_2 transition
- Should be a good tool for examining affects of various parameters in making choices between various approaches: thus could be useful in making Go/No-Go decisions regarding options.
- The model will be useful for evaluation of H_2 supply options, with the proper input data.
- Project relevant since infrastructure development is essential to reach goals and objectives.
- The optimization of the development of infrastructure is a key issue for the overall program.
- It is important to understand H_2 transition costs and the impact on technology options.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- Objective function approach is sound. Assumption of a fixed demand growth rate is simplistic. Assumption of homogeneous demand per geographic area is very simplistic; other investigators have considered regional-specific demands for some time. The cost-based decision of technology for build-out is a good approach. Matlab is appropriate tool, reading Excel sheets is convenient to H2A users
- The production modeling appears robust. It is not clear that one can isolate it from the distribution side to developing cost/ infrastructure development models. Future value would be improved if the effort was better connected to distribution versus making generic assumptions.
- Stranded asset logic is a strong point. Objective function cost approach is a good one. Fairly complete inclusion of various production options. Basic premises of model are straightforward. While it allows different scenarios and lots of parametric variations on inputs, it doesn't clearly state how these inputs will be defined: user must be careful to avoid "Garbage In-Garbage Out" syndrome. Not clear how they are treating "externalities", such as policy and technology issues
- The model's purpose is to evaluate between hydrogen supply options, which it can do. Some additions, such as including variability in demand, would improve the model.
- Consider using much higher overcapacity scenarios. Not sure if using H2A Delivery Scenario Model is going to give the right delivery distance/cost.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Good partnerships established in advisory board. These 1st year results are obviously in hind-sight; local SMR is known to be the most cost-effective. The revised result showing coal gasification contributes at higher demand indicates that the approach can be used to select production technologies; however, for the build-out to include more than one technology option, regional-specific demand must be used in the future. It appears that the progress this year was primarily in building the model.
- The analysis is not at a point that there is confidence in the results such that forward planning can occur.
- They appear to be moving along fairly well. Might be premature selection of SMR forecourt option, since one might expect significant rise in natural gas costs in near future Demand curve may well be dominant, and it is not at all clear how good the one being used is.
- The presentation could have focused more on the assumptions and input data, instead of narrowing in on a conclusion at this stage. Conclusions are premature until the input information is validated. It is important to understand the sensitivity of the variables and how intangible considerations are handled.
- The model simulates an optimal infrastructure build.
- Concerned that Forecourt Production is always the best case given the close involvement of H2Gen.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- A strong set of collaborative partners.
- Appear to have participation of meaningful groups, namely representation of the oil industry and of commercial gas producers.
- The project has several advisors from industry. Additional input on the business assumptions and to gather the most accurate input data would be helpful.
- Collaboration with industry is evident.
- Consider sharing model with other analysis teams, e.g. H_2 Delivery Analysis Team, and more collaboration with other technology developers.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- The future plans should include removing the assumption of homogeneous demand to consider some kind of regional distribution.
- The proposed next steps are appropriate and are on target.
- Task 4 among the listed future tasks is probably the key one.
- As identified, looking at build out capital will be a good next step. An additional consideration would be to analyze the advantages/disadvantages of different approaches that minimize capital risk (and encourage investment) early in the transition. It does not appear as if enough effort will be made to improve data input quality. Without that, the model will not be effective.
- Sensitivity analysis proposed is important in order to test the robustness of conclusions to variations in costs.
- Consider looking into additional demand and build-out scenarios that are regionally dependant.

Strengths and weaknesses

Strengths
- Modeling infrastructure choices with objective function is a good technique. Analyzing the infrastructure development with build-out charts is instructive.
- Objectives clear and well-defined Stranded asset logic inclusion is important Appears to include all likely production scenarios: no wasted effort on less likely approaches such as thermochemical production of hydrogen.

- The model appears to be flexible and consider a substantial number of variables, including an attempt to consider real world business conditions. As identified by the presenter, the challenge will be to assure accurate data to the analysis. The program manager is knowledgeable on the material and has adequate experience to complete the project successfully.
- Designed to interface with H2A and Macro System Model. Real world asset utilization levels & stranding of assets is represented. Inclusion of existing hydrogen supply provides realistic initial option.
- Very good use of existing H2A models.

Weaknesses
- Assumptions on demand oversimplify the problem and make the results less interesting, especially for homogeneous regional demand
- Objective Function Cost not well defined. Not clear how they are handling "externalities" Possible premature selection of forecourt Steam Methane Reformer given likely sharp increases in price of natural gas in near future. Strong dependence on Demand curve needs to be addressed/examined.
- The analysis does not take into account varying demand at stations during normal course of business. This might significantly affect results. It also appears to assume that a single station size and/or single station type would meet all needs.
- The demand assumption does not respond to infrastructure build. The basis for investment in the infrastructure does not appear to be market based - a central planning approach appears to be modeled. Overcapacity will drive investment decisions towards lower capital cost options. Other constraints such as GHG are not influencing outcomes. The feedstock cost to production facilities appears to be independent of location whereas it will vary in reality. Cost is determined offline – what does this mean?
- Appears to be relatively little coordination with other analysis teams to date.

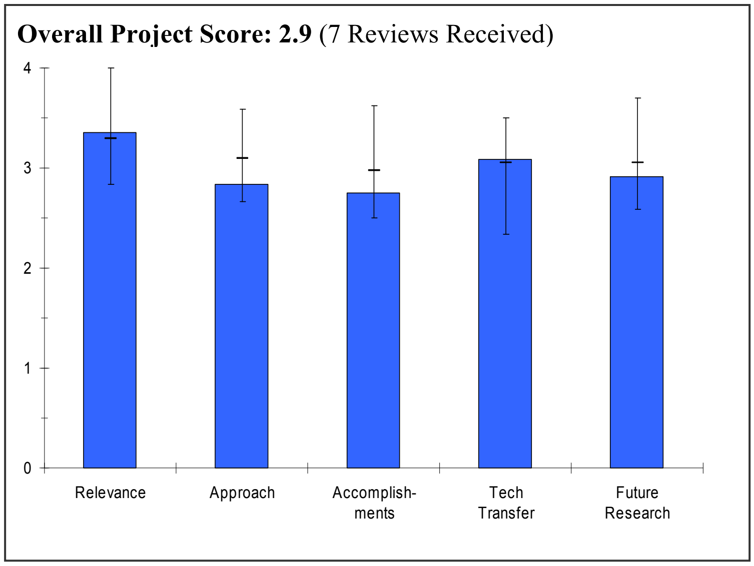
Specific recommendations and additions or deletions to the work scope

- Consider relaxing the homogeneous demand assumption with a regional model Start planning for detailed regional analysis based on GIS information.
- None.
- Consider adding real data from gasoline station usage.
- Need to include external costs, e.g. GHG emissions. Market reality of investment decisions should be considered – will overcapacity be committed without policy incentives? An interface is needed to a demand model.

Project # AN-01: Hydrogen Production Infrastructure Options Analysis

Brian D. James; Directed Techs.

Brief Summary of Project



The objectives of this project are to: 1) Create an analytical tool robust enough to assess the impact of different assumptions on hydrogen infrastructure development; 2) Exercise the tool using various assumptions to understand the infrastructure's sensitivity to different scenarios; and 3) Suggest to DOE areas of further research based on the most influential parameters in the infrastructure development. The unique features of this model include: its ability to evaluate infrastructures with varying utilization over lifetime; its ease of use; an interface aimed for use by a wide audience; a structure that can be incorporated into the Macro Model; the allowance for investor demand foresight; incorporation of stranded asset logic; user input of yearly varying hydrogen demand, unit efficiencies, and capital costs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Analysis of the infrastructure options is important to planning the H_2 transition
- Should be a good tool for examining affects of various parameters in making choices between various approaches: thus could be useful in making Go/No-Go decisions regarding options.
- The model will be useful for evaluation of H_2 supply options, with the proper input data.
- Project relevant since infrastructure development is essential to reach goals and objectives.
- The optimization of the development of infrastructure is a key issue for the overall program.
- It is important to understand H_2 transition costs and the impact on technology options.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- Objective function approach is sound. Assumption of a fixed demand growth rate is simplistic. Assumption of homogeneous demand per geographic area is very simplistic; other investigators have considered regional-specific demands for some time. The cost-based decision of technology for build-out is a good approach. Matlab is appropriate tool, reading Excel sheets is convenient to H2A users
- The production modeling appears robust. It is not clear that one can isolate it from the distribution side to developing cost/ infrastructure development models. Future value would be improved if the effort was better connected to distribution versus making generic assumptions.
- Stranded asset logic is a strong point. Objective function cost approach is a good one. Fairly complete inclusion of various production options. Basic premises of model are straightforward. While it allows different scenarios and lots of parametric variations on inputs, it doesn't clearly state how these inputs will be defined: user must be careful to avoid "Garbage In-Garbage Out" syndrome. Not clear how they are treating "externalities", such as policy and technology issues
- The model's purpose is to evaluate between hydrogen supply options, which it can do. Some additions, such as including variability in demand, would improve the model.
- Consider using much higher overcapacity scenarios. Not sure if using H2A Delivery Scenario Model is going to give the right delivery distance/cost.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Good partnerships established in advisory board. These 1st year results are obviously in hind-sight; local SMR is known to be the most cost-effective. The revised result showing coal gasification contributes at higher demand indicates that the approach can be used to select production technologies; however, for the build-out to include more than one technology option, regional-specific demand must be used in the future. It appears that the progress this year was primarily in building the model.
- The analysis is not at a point that there is confidence in the results such that forward planning can occur.
- They appear to be moving along fairly well. Might be premature selection of SMR forecourt option, since one might expect significant rise in natural gas costs in near future Demand curve may well be dominant, and it is not at all clear how good the one being used is.
- The presentation could have focused more on the assumptions and input data, instead of narrowing in on a conclusion at this stage. Conclusions are premature until the input information is validated. It is important to understand the sensitivity of the variables and how intangible considerations are handled.
- The model simulates an optimal infrastructure build.
- Concerned that Forecourt Production is always the best case given the close involvement of H2Gen.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- A strong set of collaborative partners.
- Appear to have participation of meaningful groups, namely representation of the oil industry and of commercial gas producers.
- The project has several advisors from industry. Additional input on the business assumptions and to gather the most accurate input data would be helpful.
- Collaboration with industry is evident.
- Consider sharing model with other analysis teams, e.g. H_2 Delivery Analysis Team, and more collaboration with other technology developers.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- The future plans should include removing the assumption of homogeneous demand to consider some kind of regional distribution.
- The proposed next steps are appropriate and are on target.
- Task 4 among the listed future tasks is probably the key one.
- As identified, looking at build out capital will be a good next step. An additional consideration would be to analyze the advantages/disadvantages of different approaches that minimize capital risk (and encourage investment) early in the transition. It does not appear as if enough effort will be made to improve data input quality. Without that, the model will not be effective.
- Sensitivity analysis proposed is important in order to test the robustness of conclusions to variations in costs.
- Consider looking into additional demand and build-out scenarios that are regionally dependant.

Strengths and weaknesses

Strengths

- Modeling infrastructure choices with objective function is a good technique. Analyzing the infrastructure development with build-out charts is instructive.
- Objectives clear and well-defined Stranded asset logic inclusion is important Appears to include all likely production scenarios: no wasted effort on less likely approaches such as thermochemical production of hydrogen.

- The model appears to be flexible and consider a substantial number of variables, including an attempt to consider real world business conditions. As identified by the presenter, the challenge will be to assure accurate data to the analysis. The program manager is knowledgeable on the material and has adequate experience to complete the project successfully.
- Designed to interface with H2A and Macro System Model. Real world asset utilization levels & stranding of assets is represented. Inclusion of existing hydrogen supply provides realistic initial option.
- Very good use of existing H2A models.

Weaknesses
- Assumptions on demand oversimplify the problem and make the results less interesting, especially for homogeneous regional demand
- Objective Function Cost not well defined. Not clear how they are handling "externalities" Possible premature selection of forecourt Steam Methane Reformer given likely sharp increases in price of natural gas in near future. Strong dependence on Demand curve needs to be addressed/examined.
- The analysis does not take into account varying demand at stations during normal course of business. This might significantly affect results. It also appears to assume that a single station size and/or single station type would meet all needs.
- The demand assumption does not respond to infrastructure build. The basis for investment in the infrastructure does not appear to be market based - a central planning approach appears to be modeled. Overcapacity will drive investment decisions towards lower capital cost options. Other constraints such as GHG are not influencing outcomes. The feedstock cost to production facilities appears to be independent of location whereas it will vary in reality. Cost is determined offline – what does this mean?
- Appears to be relatively little coordination with other analysis teams to date.

Specific recommendations and additions or deletions to the work scope

- Consider relaxing the homogeneous demand assumption with a regional model Start planning for detailed regional analysis based on GIS information.
- None.
- Consider adding real data from gasoline station usage.
- Need to include external costs, e.g. GHG emissions. Market reality of investment decisions should be considered – will overcapacity be committed without policy incentives? An interface is needed to a demand model.

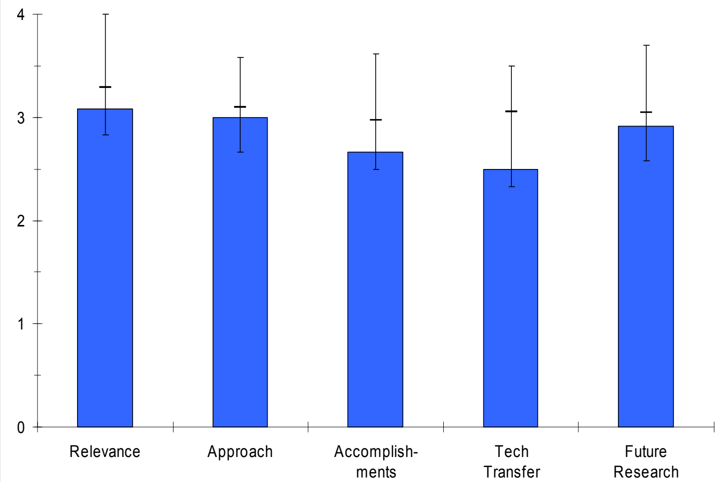
Project # AN-02: Impact of Hydrogen Production on U.S. Energy Markets

Harry Vidas; EEA

Brief Summary of Project

The objectives of this project are to: 1) Develop a consistent, integrated framework for evaluation of impacts of hydrogen production within U.S. energy markets using a regionalized version of the MARKAL model; 2) Evaluate costs and timeliness of various scenarios of a developing hydrogen supply infrastructure; 3) Evaluate impacts on U.S. energy markets including price and consumption changes for coal, natural gas, renewables and electricity; and 4) Identify most economic routes and financial risks of hydrogen production.

Overall Project Score: 2.8 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Not very important to the Hydrogen Vision: definitely not a maker or breaker. Not at all clear how they are going to get to realistic numbers. Examination of regional differences is important aspect of this study. Would be good if they looked at hydrogen versus other approaches to lessening fuel shortages, but in doing so they would need to include the objective function of minimizing carbon dioxide production.
- H_2 influence on energy markets is important.
- Project objectives support the Hydrogen Initiative and provide the MARKAL model with the ability to analyze regional infrastructure impacts.
- The integrated consideration of the transport and stationary energy markets is very important as they draw on similar resources. Market impacts need to be fed back into hydrogen market models.
- Most objectives are in-line with DOE goals, but it maybe too early for this kind of detailed analysis.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- At least some aspects of this study don't really seem to directly address the main problem and not well-focused on leading to Go/No-Go decisions. Including impact (feedback) on price/consumption changes for various scenarios is a major plus. Need to look at sensitivity of results to assumed relative efficiencies of hydrogen fuel cell vehicles (FCV) and gasoline internal combustion engine (ICE). Regionalization is a good point. Appears to address question of CO_2 sequestration: this is a plus.
- Combination of models leverages previous work. Modifications to regions for MARKAL is sound approach. Hydrogen modifications were not described in enough detail.
- The approach is well thought out for the fossil input. It lags in the renewables (biomass and renewables).
- The use of existing models is a strength for this project. It avoids waste of effort and questions about validity.
- Not clear that this modeling exercise will translate into understanding of the real world. In reality, there will be too many other consumer drivers that will impact vehicle demand.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Last year's accomplishments are not very clear. How much of this is really new versus how much is plowing old ground. Fairly methodical. The current use of old databases in the project should be reconsidered. Hydrogen vehicles versus year charts appear to be very unrealistically high.
- Progress focused on researching cost estimates for coal, biomass, & sequestration. Project did not show enough progress on the code modifications. Assumption of 3X fuel economy for FCV is not realistic, which will make model predictions overly optimistic.
- There is a concern that progress is slower than desirable, seems to have been a lot of work in 2005/6 but perhaps < 50% of scope addressed at this point in the program.
- Portfolio of models used address very well hydrocarbons, gas, and coal (a 3+ score). Approaches to biomass are not consistent with the DOE Biomass Program and the current Presidential initiative on Biofuels (a score of 2). Approach took data from one publication of the '90s which addressed biomass availability for 2010 and not the current studies that project availability by 2020+. The area already has incentives to accelerate development.
- The next year will be important for this project to produce outcomes.
- Most work was focused on coal. Not clear how this will be used to generate useful results for hydrogen.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- Not strong in that they don't appear to be involving commercial gas producers or oil and natural gas companies.
- It is not clear how this project will interface with other models being developed in other projects, e.g., demand and feedstock prices into DTI model. Infrastructure design could benefit from consideration of work done at UC Davis/others - appearance is that this is being done in isolation of other potential sources. Could consider natural gas storage as proxy for hydrogen storage rather than storage of liquid transport fuels?
- Collaborations are very appropriate for the fossil energy components. Need more explicit collaboration in the biomass and renewable portions.
- Collaboration in use of existing models is fundamental to the project.
- Closer work with other analysis teams would prevent overlap and duplication of effort. Review with Tech Teams may help drive the analysis towards generating more useful results.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- Mostly more of the same, but that's not bad in this case.
- Plans for the next stage are clear and consistent with the remit.
- Apply to the fossil energy inputs not the biomass or other renewables.
- The next year will be important for this project to produce outcomes.
- Interesting modeling effort, but it is not clear that the analysis will yield any results that can be applied to the real world. Therefore, it is not clear how this project will provide a better understanding of how we can develop a H_2 infra.

Strengths and weaknesses

Strengths
- Examination of Inter-regional differences is important. Including impact (feedback) on price/consumption changes for various primary sources is very important. Including treatment of CO_2 sequestration is important plus.
- Project is designed to evaluate H_2 production in the context of other energy markets.
- Thorough and comprehensive methodology, use of MARKAL, development of regional breakdown.
- MARKAL knowledge; models for fossil energy resources are all very good.

- Adds electricity and gas market modeling to the modeling suite – these are critical linkages for hydrogen modeling. Provides the missing feedback loop on market price impacts. Uses a mature energy market models (MARKAL etc).

Weaknesses
- Demand curves on hydrogen vehicles versus time appear badly out of kilter with reality, absent government incentives/policies.
- Progress on model development is not shown. Results after the level of spending to date seem less than expectations.
- I note that crude oil to hydrogen is not an option in the modeling system - this could be gasified just like biomass/coal.
- Lack of explicit knowledge of the biomass field.
- Not clear how the end results will be represented. Needs to be complemented by demand modeling.
- Presenter did not make it clear what the results of the analysis will be and how they will advance DOE goals/understanding. There appears to be little coordination with other analysis teams to date resulting in possible duplication of effort.

Specific recommendations and additions or deletions to the work scope

- Need to look at hydrogen vehicles against other fuel-saving options such as optimized plug-in gas/electric hybrids. Needs sensitivity analyses leading to error bars.
- The project would benefit from more integration with other modeling efforts under DOE sponsorship, particularly sharing of inputs and outputs. There appears some duplication, e.g., demand estimates, but I suspect this is not really the case as the estimates are derived from different directions, hence the previous comment.
- Significantly revise the scope/approach in the biomass area. Could collaboration with the DOE Biomass Program be started? The project approach cannot take one data set from the literature of the '90s and project it into the future when there are current potentials for biomass availability that DOE and USDA have published recently for 2020.
- Some technologies are modeled that have uncertain cost - e.g. Geosequestration. The results could be biased by not accounting for uncertainty in technology. Is gasification modeled – bypassing the electricity grid? Storage capacity is an important issue – security of supply. How is optimization achieved? Inclusion of plug in hybrids (ICE & FC) should be considered? Need to ensure assumptions are consistent with other models. Measurement of cost, security and GHG emissions would assist evaluation of scenarios.

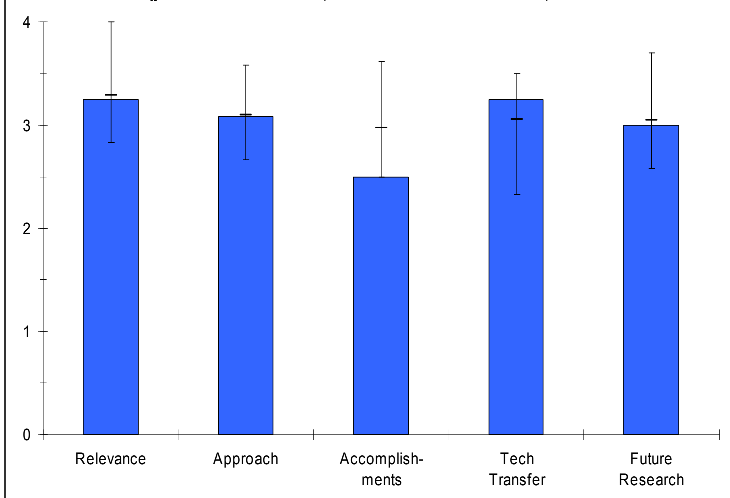
Project # AN-03: Analysis of the Hydrogen Production and Delivery Infrastructure as a Complex Adaptive System

George Tolley; RCF, Inc.

Brief Summary of Project

The objectives of this project are to: 1) Use agent-based modeling (ABM) to provide insights into likely infrastructure investment patterns; 2) Deal with chicken-or-egg aspect of early transition; and 3) Provide answer to the question, "Will the private sector invest in hydrogen infrastructure?" These objectives will be met by focusing on investments as business decisions, developing a basis for preliminary assessment of profitability, and preparing an ABM for detailed simulations.

Overall Project Score: 2.9 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Infrastructure transition analysis is important to planning the path to H_2 use.
- Addresses a rather key question of "Will the Private Sector Invest in H_2 Infrastructure."
- Interesting and different approach to analysis of production and delivery.
- Agent Based Model is likely to provide good insights to government programs to understand how to decrease investment barriers in new technologies and their infrastructure.
- Consideration of diverse decision making approaches is an important addition to the hydrogen modeling suite.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- Agent based model approach is unique and appropriate for making decisions regarding both government and private sector parties.
- Agent-based modeling approach with their premises is excellent. Business Decision Framework is a significant analytical tool for this task. Addresses all-important "chicken-and-egg" question. Seems to be some off-target wandering around rather than homing in on the central issues. Not clear how useful of a tool for others is going to come out of this.
- The project would benefit from a better/more complete understanding of the business goals, particularly their risk profiles and spending practices on companies in this business. Downstream spending by major oil companies is a minimal part of their capital spending budgets.
- Important elements are in the model that has been nearly completed and begins to be exercised.
- The agent based decision making process is an excellent feature to be brought to hydrogen modeling.
- Not enough information on how the model works (i.e. What are the assumptions?) to understand the approach or how results were generated.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- The project is just getting started, only 15% of funding. The preliminary results show the approach is viable.

- Seems a little thin on results for the big bucks involved. Not clear that the mapping studies are particularly useful. Reviewing earlier technology penetration rates does not appear particularly useful.
- Difficult to assess overall value while only considering distributed infrastructure. Results and methodology difficult to understand in a short presentation, therefore this leads to doubts about value of results?
- 1) Good example shown of business decision model – case of Posture Plan (government proposal) versus Agent Based Model-BDM driven process with a delay of 5+ years. 2) Somewhat naive lessons from introductions of previous technological innovations. There is a wealth of literature on more germane technological change examples and learning curves for implementation in the energy scenario itself (combustion to turbines). 3) Very good balance of inputs of soft and hard data – social and technical.
- Provides a framework for evaluation of the transition in a manner that takes GIS factors into account.
- Results were shown, but it was not clear how they were generated.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Good team of effective collaborators with experience in H_2 system analysis.
- Right types of people included (oil company, gas producer).
- Good list of collaborators but concern over value of their contribution since some assertions don't seem to be valid, e.g., investment levels by major energy companies. Good survey of prior innovation lessons, but is this the same? Later examples like mobile phones might be more valid? Does hydrogen infrastructure have to mirror existing gasoline infrastructure?
- Starting project, coordination among partners can improve. Sharing of basic knowledge like calculation platform should be common knowledge.
- The collaboration in this project is a strength. Clearly the collaborators have made a significant contribution in an area that has been lacking in other models.
- Not clear if this model will be able to be used in/by other projects.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- PI appears to be uninvolved in software design. Project should be more closely coupled to Macro-System Model activities to make sure there can be software compatibility.
- The pathforward should be clearly identified which was not obtained from the project presentation.
- How difficult would it be to expand the approach to address central/city gate plants, not just distributed?
- Plan addresses key areas for exercising the models so that policies, decision drivers for industry, consumer behavior can be modeled.
- The next phase of work is important in order for outcomes of the work to be usable.
- Lots of work to do.

Strengths and weaknesses

Strengths

- The agent based modeling approach is a good means of simulating the interests of varying parties involved in the transition to hydrogen.
- Agent-based modeling approach. Good question and premises. Chicken and egg aspect important.
- Interesting approach, allows for uncertainty and learning.
- Agent Based Modeling is a good addition to the tools of the Analysis program. The ability to input "behaviors" of agents and analyze their potential outcomes is a powerful guide to the program.
- Agent based decision process adds a market reality to infrastructure decisions. Decisions will be based on convenience and vehicle costs. Model will include both in and out of town trips. Simulation model link to other models.

Weaknesses

- A clear description of the software issues needs to be presented.
- Looks thin on useful output and on actually answering the well-posed questions.
- No clear path as to how to integrate this work into other analysis work, i.e., captures its value into other work.
- A suggestion to avoid a potential weakness of analyses of such complex multivariable data sets is to use of a situation for which there are data from policy actions on new fuels (e.g., ethanol, COG, etc) and their penetration. One example (ethanol) had a successful policy to achieve a higher penetration and the second not being sufficient. Analyze the role of the alignment of state and local governments to the federal and how much this amplified the federal actions. While retrospective, the ability of the model to predict these outcomes, which resulted in significant industrial investment, would reinforce the value of the model.
- Options not as strong a focus. Not clear how this will interface with other modeling. Not clear what the assumptions or structure of the model are.
- Modal approach and assumptions were not conveyed in the presentation. This made it hard to rate this project. The presentation approach should be evaluated to give a clear understanding of the model.

Specific recommendations and additions or deletions to the work scope

- Couple this project more closely to the Macro-System Model project, at least for planning and coordination purposes.
- None.
- The concept of an advisory group, larger than the project participants, might help guide reasonableness in assumptions. Explicit interactions with other analysis work would help, e.g., demand simulation into DTI work, cost data from other work.
- See above.
- A description of how agent behavior adapts is necessary? Defines the outcome of competition but not necessarily the optimal solution for the economy – central planning vs. competition. Will private sector make these decisions – need to account for policy stimulus – close gap with posture plan by introducing incentives The idea of Government agent was raised as a way to address government objectives. This modeling and collaborators could be used to develop a demand model which is a critical element to the overall modeling suite.

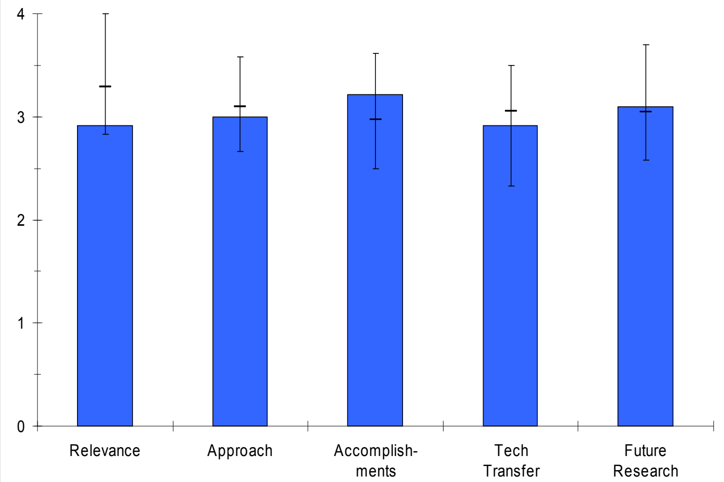
Project # AN-04: WinDS-H2 Model and Analysis

Keith Parks; NREL

Brief Summary of Project

This project is a GIS-based, supply-side hydrogen economy transition analysis. The analysis determines optimum hydrogen production and delivery pathways for cities within a region and calculates delivered hydrogen cost. It will also determine the infrastructure layout for different production/delivery choices and consider electricity sector impacts and contributions to the hydrogen economy.

Overall Project Score: 3.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- Minor contribution at best: not going to be a maker or breaker. Not particularly useful in Go/No-Go decisions. Not really contributing to progress toward a hydrogen transport sector economy.
- Regional analysis is important to understanding the trade-off between H_2 production and delivery options.
- Project relevant since Energy Policy Act of 2005 explicitly requested the understanding of renewables and hydrogen link which has been partially done. For instance, Wind/H_2 was considered hybridized with the grid and not with another compatible renewable.
- Has unique features – regional rather than city based.
- This model addresses the development of hydrogen infrastructure.
- In-line with project funding.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Objectives not unreasonable but scenario is too simple and non-integrated. Overly simplistic. One plus is that it does cost out pathways for different regions.
- Combination of models is a good approach. Correlations from H2A scenarios model simulate delivery options. Model is static, so transition analysis is outside realm of study.
- The model provides visual outcomes that illustrate well infrastructure options, weaknesses, and strengths. Strength is the analyses of distributed systems compared with central production and siting in rural or urban regions and their interface.
- Use of other models and contributors is good. Representation of economies of scale is good. Geographic representation/build out good, especially recognition of distributed technologies occurring in rural areas for longer.
- Integration with GIS capability, H2A and NEMS is a strength.
- Good integration with existing models.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Maps not particularly informative. Pathway optimization is straightforward and easy to exercise. Presentation charts tended to be confusing.

- Results show realistic options are coal gasification or distributed Steam Methane Reforming (SMR), with SMR necessary at some distance from plant. Michigan study should be compared with some other region of county.
- The model was exercised to provide the spatial link and explore the urban/rural interface. It confirms that for isolated regions distributed hydrogen options will be necessary, that are more costly amongst the technical choices currently in H2A that were used.
- Progress vs. Timetable appears good. Where does the pricing of other feedstocks get affected, e.g., coal?
- The modeling appears to be producing logical results.
- In-line with project funding.

<u>Question 4: Technology transfer/collaborations with industry, universities and other laboratories</u>

This project was rated **2.9** for technology transfer and collaboration.

- Do not appear to be involving appropriate outside people (oil and gas companies, potential users.
- Collaborated with others who developed the models that were linked in this analysis.
- Project completed and integrated the overall program models.
- Good use of other models/collaborators.
- Very strong modeling links.
- These results should be compared to the DTI project and other preliminary analysis results.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated **3.1** for proposed future work.

- Doesn't appear to be worth pursuing further. Simple EXCEL product which simply reads tabular input files seems to be an appropriate endpoint.
- Project is nearly completed.
- Scoring future work not relevant since the project is completed (NA).
- Project almost complete. Would want to ensure that results can be available to other modeling projects.
- The sensitivity analysis is important to ensure that robust results are considered.

<u>Strengths and weaknesses</u>

<u>Strengths</u>

- Essentially none.
- GIS based regional analysis that compares various production options.
- Model developed to illustrate visually infrastructure options for centralized and distributed production. As more distributed options are developed in the program, the model should be updated.
- Good build onto the winds project. I especially like the GIS interface and apparent ease of use.
- Strong linkage to H2A (production and delivery). Integrated GIS, Optimization approach. Supply curve development is powerful. Feedback onto natural gas price – based on NEMS forecast – could link to MARKAL Model. Includes existing hydrogen infrastructure.

<u>Weaknesses</u>

- No clear conclusions. Very little value added.
- Results and conclusions are limited by the analysis of only one region.
- Static and set as competition of three technologies at a time.
- Need to put in demand forecast. Not a criticism per se but it would be good to have a demand forecast project that can be used across the various analysis projects.
- Lacks demand evaluation. Green House Gas (GHG) emissions are not factored in.

Specific recommendations and additions or deletions to the work scope

- The project should end.
- Consider what aspects of this modeling effort are applicable for future incorporation into the Macro-System Model.
- I would suggest collaboration with the Energy and Environmental Analysis project (and potentially others) to see how their data/outputs could be integrated with this model.
- Integration with a demand model would make this tool powerful. Modeling of plug in hybrids (ICE or Fuel Cell) could be considered. The objective should include cost, energy security and Green House Gas (GHG) emissions.

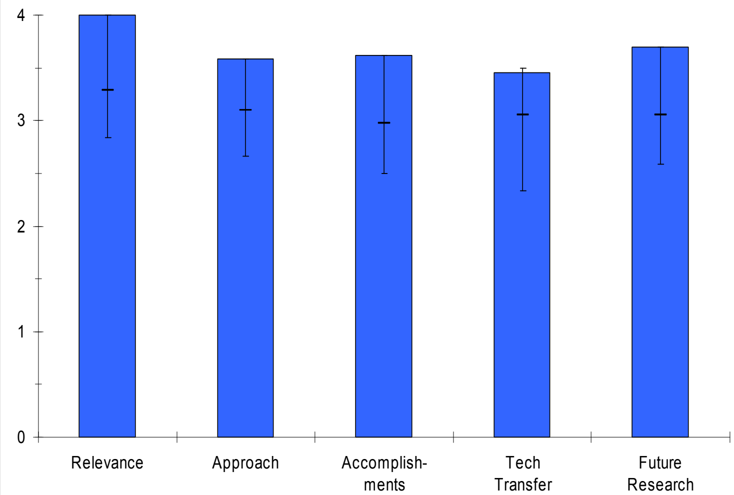
Project # AN-05: Macro-System Model

Mark Ruth; NREL

Brief Summary of Project

The overall objective of this project is to develop a macro-system model (MSM) aimed at performing rapid cross-cutting analysis within the Hydrogen Program. It will use high-level architecture to link models being developed or used by the Program. The MSM will generate results that support decisions regarding programmatic investments and focus of future funding. Currently, the H2A, HDSAM, and GREET models have been linked within the MSM framework.

Overall Project Score: 3.7 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- Excellent approach to quantitatively defining costs in terms of both costs and resource utilization. Should be important in terms of determining whether or not to continue down various paths and in narrowing down options.
- This is crucial work to an overall understanding of the Hydrogen Program.
- Outputs from this model will directly support decisions and direction of the President's Hydrogen Fuel Imitative.
- The integration of all models enabling cross cutting analysis, tradeoff analysis, and support of programmatic decisions.
- Pulling the modeling resources together is critical to the success of the program.
- Has potential to address many technical barriers and questions, but will depend on accomplishments.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- Addressing right questions and developing a good systems approach to evaluating and comparing options. Includes policy and incentives and environmental considerations, a strong plus. Analysis of raw material needs also a plus. 2005-2006 objectives and overall objectives very pertinent. Have delineated the important questions very well. Well-to-wheels analysis is excellent feature. Top-notch approach to developing a comprehensive over-arching model of various pathways from well to wheel. Will produce useful product for future users.
- Very analytical approach. I am concerned about how the value of this work can be effectively communicated to the wider stakeholder community. A "layman's" summary could be valuable at regular intervals.
- Selecting the Federated Object Model architecture appears to be the best compromise between a fully integrated model and one that can be developed and used in a reasonable amount of time.
- Very well designed taking input from National Academy of Science (NAS), analysts, the community, and users. Well poised to answer questions on high priority issues for the transition into a hydrogen economy. The approach chosen is well suited for the component analyses that exist and that could emerge with time.
- The linking process has been achieved, with a functional demonstration.

- Very difficult task to integrate these disperse models. The approach appeared to be sound, but it wasn't completely clear how the details will be worked out… presenter should have been given more time to explain this complex project.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- Excellent progress. Interaction with other models is an important facet of this work---integrating existing submodels where possible. Federated Object Model (FOM) architecture is an important feature of this work.
- Progress has been good; expectations similarly high.
- Accomplishments to date, including deciding on a reasonable approach and initiating model development are very good.
- First working prototype was demonstrated and made excellent progress integrating the existing analyses models.
- Progress is consistent with the plan.
- Good progress on a very difficult project. Results need to be compared to other analysis results.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- Could use more interaction with energy companies and chemical gas producers than is currently apparent.
- Collaboration is key and is being handled well, e.g., analyst workshop, use of other models. Further wider debate could be valuable to the Macro-System Model (MSM) builders, users and audience.
- Collaborations with the other model developers are reasonably good. Bringing in Sandia National Laboratory (SNL) to support Federated Object Model development is an excellent move. Relying on other model developers more could move this rating to "outstanding".
- The collaboration between National Renewable Energy Laboratory (NREL) and SNL has been excellent in enabling the federated model to be implemented. Program participants that developed models (H2A Production, HDSAM, and GREET), have provided input models that now can be exercised by one organization instead of sequentially by several organizations.
- This model is very strongly dependent on other models and is likely to become central to most modeling.
- Now that a framework has been developed, I hope the Tech Team involvement and collaboration will increase.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.7** for proposed future work.

- Very well laid out future plans.
- Early stage work so far. Future plan is thorough and laid out very clearly.
- Priorities for building the model appear very reasonable. Results will provide benefit within two years and benefit growth will follow model expansion after that point.
- Additional models for transportation will be incorporated with time. Spatial, temporal, and consumer models will be incorporated.
- The plan is ambitious but there appear to be many potential options for this models development.

Strengths and weaknesses

Strengths

- Very systematic approach with excellent definition of important issues. Analysis issues categories and prioritization well-defined. Have delineated and posed important questions very well/thoroughly. Well-to-wheels pathway analyses are a major plus.
- Unique modeling effort. Federated Object Model (FOM) approach could allow use of multiple models with common assumptions and philosophies.

- Very good architecture developed and implemented for 3 of the programs. More models will be added.
- Access to the FOM model architecture. Models can be used when linked to the Macro-System Model (MSM) or independently.
- Adopting the FOM architecture and including SNL's expertise to build the model.

Weaknesses
- Could use a little more discussion on how model validation will be performed.
- Complexity of the effort makes QA/(appropriate use) a concern.
- Robustness of the models for multiple users.
- The Macro-System Model (MSM) lacks optimization and decision making structure. User currently specifies the one year situation. Does not represent the transition. There will be a limitation on which models can be used. There does not appear to be a clear consideration of the options for MSM development.
- Not clear that other model developers are being brought in and utilized as well as SNL is.

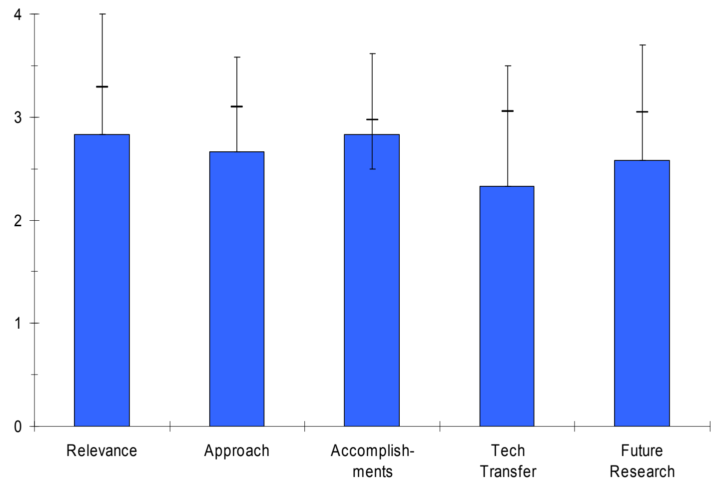
Specific recommendations and additions or deletions to the work scope

- I would like to see a dedicated project or addition to this one that synthesizes the various analyses and summarizes results in a layman fashion on a regular basis. Many of the individual projects are difficult to understand and have confidence in the outputs. If one group/project were responsible for collating and summarizing, and ensuring common assumptions and philosophies, a coherent summary report series could be produced. I think this could help to inform the wider community and dispel several misunderstandings! Minor quibble - process of liquefying is called liquefaction.
- The Macro-System Model (MSM) could have a decision modeling layer added on top of current model and include an optimization layer above current model The objective function of the MSM could include cost, energy security measure and Green House Gas (GHG) emissions. Future development will necessarily result in more complex structure.
- This is almost certainly the best of the analysis projects.
- Recommended that some way of assigning positive points to reducing global warming emissions be included in comparison with non-hydrogen approaches to alleviating fuel shortages be built in eventually.
- Recommend that NREL concentrate on integration role and depend more on other developers/partners for model and interface development.

Project # AN-06: Geographically Based Hydrogen Demand & Infrastructure Analysis

Margo Melendez; NREL

Overall Project Score: 2.7 (6 Reviews Received)



Brief Summary of Project

This National Renewable Energy Laboratory hydrogen infrastructure development and demand analysis project seeks to quantify hydrogen demand in the U.S. and estimate costs to support infrastructure to meet emerging hydrogen demand. The work focuses on a combination of spatial and temporal assessments to identify the most economic pathways for successfully meeting emerging hydrogen demands. Hydrogen infrastructure transition analysis identifies, describes, and quantifies options for hydrogen refueling during the transition to hydrogen as a transportation fuel.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- This type of analysis appears very mushy, not leading to any means of down selecting approaches. Concept of demand for hydrogen as opposed to demand for fuel in general is not realistic.
- Geographic infrastructure analysis is relevant to the hydrogen initiative.
- Seems to be some overlap with the HyDS project? Both have GIS base, infrastructure rollout. Is demand forecast the key new feature?
- Project is supportive of the Hydrogen Initiative. It adds geographic dimensions to the transitional analyses of the Hydrogen Program and enables visualization of major components of the transition. Demographics evolution, industrial/economic activity, government and private sector ownership, current infrastructure, etc.
- This element of analysis will be very helpful as an input to other models that currently use less sophisticated demand assumptions.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- Doesn't really address the very important cost issues (or even feasibility issues). Approach is very fuzzy (unfocused). Their "demographic attributes" maps add very little value. The study seems to take a sociological analysis approach rather than an analysis of cold hard facts (such as costs, infrastructure, requirements, environmental impacts, etc,etc,etc).
- Using GIS to study demographics of likely hydrogen demand is sound approach. Demand seems a bit dependent on assumptions of consumer likelihood to adopt hydrogen vehicles; these assumptions seem reasonable, but are not backed up by any consumer survey information. Weighting the demand factors is somewhat arbitrary.
- Demographic based approach is a useful addition to the analysis suite. Fleet to consumer strategy interesting…but private fleets not like public ones?
- The diagram of page 6 is a very good illustration of what the role of the GIS and resource analysis plays. The degree of integration with other analyses did not come across as well.
- The assumptions don't appear to be market tested.
- This approach is a much better way to estimate hydrogen demand than other approaches.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Not clear what is learned from these maps, which are simply reproductions of easily attainable statistics.
- Results achieved the objective of quantifying hydrogen demand. National maps of demand show varying results with adjustments to the arbitrary weightings. Objectives states cost of infrastructure would be considered, but results to date did not mention.
- Results to date are impressive. Need to be translated and made available for input into other models.
- The project illustrated various factors and provided maps of information that highly depended on the audience knowing the various government programs that exist. It is essential explain the data without excessive jargon.
- The technical framework appears to be strong.
- Good work. Would like to see more integration into other projects.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- No interactions with those who are going to influence the use of a hydrogen economy are apparent.
- Author is collaborating with experts at Davis and elsewhere. The connection to the Macro-System Model (MSM) is not clear.
- Most of the collaborations include universities and laboratories. Input from industry could be explored further.
- There is a lack of real market input.
- More collaboration with other analysis groups and make model or at least results available to the public.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- It actually appears that this project is coming to an end, thus, future plans are not appropriate.
- Future research will consider costs of infrastructure.
- Project almost complete. Would want to ensure that results can be available to other modeling projects.
- Focus on specific examples of policy-alternative fuels geographic deployment (e.g., alternative fuels or ethanol) to show the geographic/technology interactions. So far, data are to be interpreted by the readers and the case has to be done in the analysis to show the links e.g. distributed versus centralizes is a good example.
- Looks good. I am looking forward to seeing final results and using them in other analyses. Results should be prepared in a way that they can be critiqued and used by other analysis projects, e.g. Statistical approach.

Strengths and weaknesses

Strengths
- None.
- Using GIS data for national and regional study of the hydrogen infrastructure is a good approach.
- GIS representation very easy to comprehend, ability to scale down to more detailed areas good. Approach using customer characterization is useful. Effect of policies surprisingly good although not explained in any detail!
- Very good GIS capability and coupled databases
- Focus market behavior. Sensitivity analysis on take up is quite helpful.

Weaknesses

- The GIS information is used to apply a demand expectation that will be logical. It has some arbitrary weighting factors built in. It would help if these factors could be justified by any kind of survey information.
- Linkages to technologies and policies need to be made to show value of the analyses to general audiences (not all audiences understand renewables disperse nature).
- Behaviors appear to be assumed but could be strengthened with market research or analogous market behavior (ethanol or Hybrid take up behavior). Refueling locations are assumed to be near homes. The project AN3 approach may be more valid. Need to take account of route utilization and how demand spreads based on routes traveled.

Specific recommendations and additions or deletions to the work scope

- This GIS-based project should be directed to start thinking about how to coordinate with the Macro-System Model (MSM) efforts.
- Collaboration with HyDS project going forward would be valuable to both projects - a combined project extension might make sense.
- The model would be strengthened by integration with the approach used in project AN3 (Agent Based Model project). There needs to be some market testing to confirm assumptions - perhaps by collaboration with project AN3 (Agent Based Model project) parties. Fleet data could be better accessed though automotive industry collaboration. Description of the weighting of factors attributes used in assessing demand would be helpful? UC Davis work on infrastructure development may be useful in developing fuel station infrastructure requirements resulting from the demand.

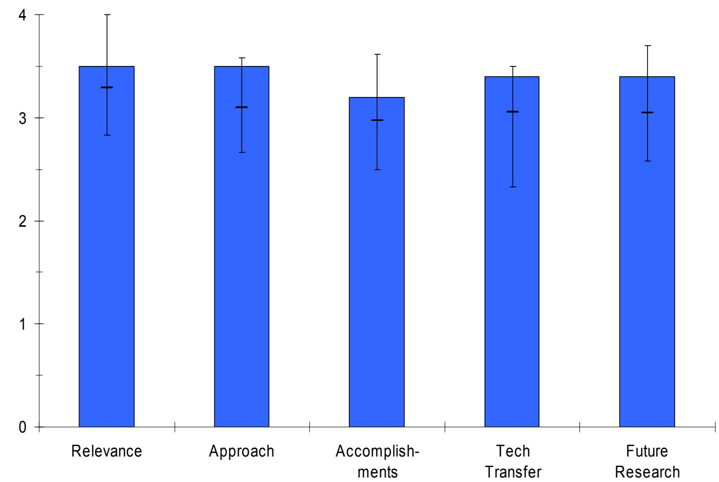
Project # AN-07: Hydrogen Transition Modeling and Analysis: HYTRANS v. 1.0

Paul Leiby; ORNL

Brief Summary of Project

Oak Ridge National Laboratory has created a working version of an integrated model of the market's transition to hydrogen as a transportation fuel using non-linear optimization methods. The model includes representation of 1) hydrogen production and delivery; 2) vehicle production, including technological progress, scale economies and learning-by-doing; and 3) demand for vehicles and fuels, including the effects of fuel availability and diversity of vehicle choice. Future development will focus on continuous improvement of model components, producing a regional model, generating plausible scenarios of the transition to hydrogen based on DOE Multi-Year Program Plan (MYPP) goals and other technology forecasts, and publishing model documentation and scenario analyses.

Overall Project Score: 3.4 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Clear alignment to DOE plan goals.
- Looks like one of the two best approaches to reaching logical conclusions about the future of hydrogen usage in the transportation sector and to the narrowing down of optional approaches. Market transition analysis is an important aspect of this study.
- Project integrates major components of the transition to a hydrogen economy. It is being exercised to provide data on the early transition to guide policy development.
- Appear to have several facets that are analogous to the Macro System Model but has optimization over a transition period also.
- Duplication of effort.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- Integration of consumer choice and effect on hydrogen demand and costs, etc., is very good.
- Very good approach to analyzing various scenarios and homing in on optimal approaches to hydrogen utilization. Inclusion of policy/incentive scenarios is a strong plus. Adding finer granularity to the early transition years is important, also. Addressing the question of availability of various starting energy sources by region is a plus. Maximization of total consumption benefit minus production, distribution, and other costs is a very important concept.
- Well designed and flexible to address evolving program needs.
- Modeling techniques address need to assess transition.
- Didn't hear anything about the consumer choice model, but from what i've seen in the past, it does not look like it would yield interesting results.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

Chris Aardahl
Pacific Northwest National Laboratory
1529 W. Sequim Bay Rd
Sequim, WA 98382
360-582-2529
christopher.aardahl@pnl.gov

Timothy Aaron
Sr. Engineer
Praxair, Inc.
175 East Park Drive
Tonawanda, NY 14150
716-879-2615
tim_aaron@praxair.com

Tarek Abdel-baset
Hydrogen Storage Engineer
DaimlerChrysler
800 Chrysler Dr S2W14 W3002
Auburn Hills, MI 48326
248-576-7634
tab51@dcx.com

Mohamed Abdou
Technology Manager
DuPont
4417 Lancaster Avenue, CRP 701
Wilmington, DE 19880-0701
302-999-2380
Mohamed.Abdou@usa.dupont.com

Judi Abraham
President
Conference Management Associates, Inc.
1401 Spring Lake Drive
Haymarket, VA 20169
703-754-0066
Confserv@aol.com

Salvador Aceves
Group Leader-Energy Conversion Storage
Lawrence Livermore National Laboratory
7000 East Ave L-644
Livermore, CA 94550
925-488-0864
saceves1@llnl.gov

Mark Ackiewicz
Technology & Management Services, Inc.
955 L'Enfant Plaza North, SW, Suite 1500
Washington, DC 20024
202-554-4602
mackiewicz@tms-hq.com

Haruhiko Adachi
General Manager
The Japan Institute of Energy
6 Floor, 6-5-4 Sotokanda, Chiyoda-ku
Tokyo, 101-0021
3-3834-6456
adachi@jie.or.jp

Jesse Adams
Project Officer
U.S. Department of Energy
1617 Cole Blvd.
Golden, CO 80401
303-275-4954
jesse.adams@go.doe.gov

Thad Adams
Savannah River National Laboratory
Bldg. 773-41A/151
Aiken, SC 29808
803-725-5510
thad.adams@srnl.doe.gov

Kev Adjemian
Manager
Nissan Motor Company
1, Natsushima-cho, Yokosuka-shi
Kanagawa, 237-8523
81-46-867-5378
k-adjemian@mail.nissan.co.jp

Radoslav Adzic
Brookhaven National Laboratory
Bldg 555
Upton, NY 11973-5000
631 344-4522
adzic@bnl.gov

Rachel Aga
Postdoctoral Researcher
Oak Ridge National Laboratory
One Bethel Valley Rd, P.O. Box 2008, MS-6115
Oak Ridge, TN 37831-6115
865-771-4105
agars@ornl.gov

Rajesh Ahluwalia
Senior Engineer
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439
630-252-5979
walia@anl.gov

Shabbir Ahmed
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439-4837
630-252-4553
ahmed@cmt.anl.gov

Channing Ahn
Caltech
1200 E. California Blvd, MS 138-78
Pasadena, CA 91125
626-395-2174
cca@caltech.edu

HIDEAKI AKAMATSU
General Manager, Planning Div.1
Fuel Cell Commercialization Conference of Japan
3-6 Kioi-cho, Chiyoda-ku
Tokyo, 102-8556
33512 2855
akamatsu@mx10.ttcn.ne.jp

Mohammad Alam
Professor/Chair
Researcher
1968 Bradbury Drive West
Mobile, AL 36695
251-460-6117
tshoemo@usouthal.edu

Sudhakar Alapati
PhD Student
Carnegie Mellon University
Carnegie Mellon University, 5000 Forbes Av.,Chemic
Pittsburgh, PA 15213
412-268-2245
asudhaka@andrew.cmu.edu

James Alkire
Project Officer
U.S. Department of Energy
1617 Cole Blvd.
Golden, CO 80401
303-275-4795
james.alkire@go.doe.gov

Andrea Ambrosini
Postdoctoral appointee
Sandia National Laboratories
PO Box 5800, MS 0734
Albuquerque, NM 87185-0734
505-284-1340
aambros@sandia.gov

Kazuki Amemiya
Toyota Motor Corporation
1200,Mishuku
Susono,Shizuoka, 410-1193
55-997-7844
kazuki@amemiya.tec.toyota.co.jp

APPENDIX A: ATTENDEE LIST

Arlene Anderson
Technology Development Mgr
U.S. Department of Energy
1000 Independence Ave SW
Washington, DC 20585
202-586-3818
arlene.anderson@ee.doe.gov

Everett Anderson
Director of Technology
Proton Energy Systems
10 Technology Drive
Wallingford, CT 6492
203-678-2105
eanderson@protonenergy.com

Sashe Annete
Policy strategist
U.S. Green Building Council
202 Huy 34 N.
Colts Necknj, NJ 7722
732-492-688
sashe@att.net

Donald Anton
Consulting Technical Fellow
Savannah River National Laboratory
Bldg. 999W
Aiken, SC 29803
803-507-8551
donald.anton@srnl.doe.gov

Laurent Antoni
Laboratory Manager
CEA
17, rue des martyrs
Grenoble, 38000
3343-878-6025
laurent.antoni@cea.fr

Sheral Arbuckle
Supervisor
Ford Motor Company
1201 Village Road
Dearborn, MI 48121
313-845-0008
sarbuckl@ford.com

Douglas Archer
Program Manager
U.S. Department of Energy
FE-24, GTN, 1000 Independence
Ave., S.W.
Washington, DC 20585-1290
301-903-9443
douglas.archer@hq.doe.gov

David Arcury
VP, Sales
Advanced Fluid Tech. dba Dynalene
Heat Transfer Fl
5250 West Coplay Road
Whitehall, PA 18052
610-262-9686
davida@dynalene.com

Anthony Arduengo
Professor
University of Alabama
338 Shelby Hall; 500 Campus Drive
Tuscaloosa, AL 25487
205 348-3556
ajarduengo@att.net

Muhammad Arif
NIST/U.S. Dept. of Commerce
100 Bureau Drive
Gaithersburg, MD 20899
301-662-4637
arif@nist.gov

Timothy Armstrong
Program Manager
Oak Ridge National Laboratory
One Bethel Valley Road
Oak Ridge, TN 37831
865-574-7996
armstrongt@ornl.gov

James Arps
Southwest Research Institute
6220 Culebra Rd
San Antonio, TX 78238
210-522-6588
jarps@swri.org

Radoslav Atanasoski
3M
3M Center 210-2S-05
St. Paul, MN 55144-1000
651 733-9441
rtatanasoski@mmm.com

Paolina Atanassova
Manager of R&D, Fuel Cells
Cabot Corporation
5401 Venice NE
Albuqueruqe, NM 87113
505-342-1492
paolina_atanassova@cabot-corp.com

David Austgen
General Manager - Technology
Shell Hydrogen
Penzoil Place, NT 700 Milam St.
Houston, TX 77002
713-546-8322
dave.austgen@shell.com

Tom Autrey
Pacific Northwest National
Laboratory
PO Box 999
Richland, WA 99352
509-375-3792
Donna.Sturdevant@pnl.gov

Jorge Ayala
Director Technology Programs
Columbian Chemicals
1800 West Oak Commons Court
Marietta, GA 30062
770-792-9485
jayala@columbianchemicals.com

Aprilgrace Aytona
Energetics, Incorporated
7164 Columbia Gateway Drive
Columbia, MD 21046
410-953-6211
aaytona@energetics.com

Amy Babcock
President
Government Contractor
1255 University Ave, Suite 110
Rochester, NY 14607
585-473-3220
ababcock@stg2.com

Rob Bacyinski
Director; Hydrogen
DTE Energy
3200 Hobson St. Noble TR
Detroit, MI 48201
313-577-7224
bacyinskir@dteenergy.com

R. Tom Baker
Research Chemist
Oak Ridge National Laboratory
Chemistry Division, MS J582
Los Alamos, NM 87545
505-667-9274
bakertom@lanl.gov

Paul Bakke
Project Manager
DOE Golden Field Office
1617 Cole Blvd
Golden, CO 80401-3393
303-275-4916
paul.bakke@go.doe.gov

U. (Balu) Balachandran
Manager, Ceramics Section
Argonne National Laboratory
9700 S. Cass Avenue
Argonne, IL 60439
630-252-4250
balu@anl.gov

Thomas Barton
Lead Engineer
Western Research Institute
365 North 9th Street
Laramie, WY 82072
307-721-2472
tbarton@uwyo.edu

Suresh Baskaran
Product Line Manager
Pacific Northwest National Laboratory
902 Battelle Blvd. K2-44
Richland, WA 99352
509-375-6483
suresh.baskaran@pnl.gov

Olga Baturina
Naval Research Laboratory/Nova Research, Inc
6008 Mardale Ln
Burke, VA 22015
703-644-0541
olga.baturina@nrl.navy.mil

Theodore Baumann
Lawrence Livermore National Laboratory
P.O. Box 808, L-092
Livermore, CA 94551
925-423-5543
baumann2@llnl.gov

Farshad Bavarian
Sr. Engineer
Chevron Technology Ventures LLC
3901 Briarpark
houston, TX 77042
713-954-6975
bavarf@chevron.com

Coda Beatrice
European Commission
Rue de Champs de Mars 21
Bruxelles, 1050
0032 2 29 87484
beatrice.coda@cec.eu.int

Rich Bechtold
Sr. Project Manager
Alliance Technical Services
5400 Thunder Hill Rd.
Columbia, MD 21045
443-812-3921
rlbechtold@alliance-technicalservices.com

Michael Bednarz
Project Manager
US DOE - Midwest Regional Office
141 W. Jackson Blvd., Rm 1710A
Chicago, IL 60604
312-886-8585
michael.bednarz@ee.doe.gov

Bud Beebe
Regulatory Affairs Coordinator
SMUD, The Sacramento Municipal Utility District
6201 S Street M.S. B257
Sacramento, CA 95628
916-732-5254
bbeebe@smud.org

Juergen Behnisch
VP & Technical Director
Degussa Corporation
379 Interpace Parkway
Parsippany, NJ 7054
973-541-8717
juergen.behnisch@degussa.com

Tim Bekkedahl
BekkTech LLC
PO Box 2876
Loveland, CO 80539
970-461-3998
rhonda@bekktech.com

Brian Benicewicz
Professor
Rensselaer Poly Inst.
Department of Chemistry, Cogswell lab
Troy, NY 12180
518-276-2534
benice@rpi.edu

Thomas Benjamin
Argonne National Lab
9700 S. cass Ave
Argonne, IL 60439
630-252-1632
benjamin@cmt.anl.gov

Jeffrey Bentley
CEO
CellTech Power LLC
20 Landmark Road
Westford, MA 1886
617 293-8703
bentley@fuelcells.com

Gene Berry
Engineer
Lawrence Livermore National Laboratory
7000 East Ave (L-644)
Livermore, CA 94550
925-424-3621
berry6@llnl.gov

China Raju Bhimapaka
Post Doctoral Research Associate
Purdue University
560 Oval Dr
West Lafayette, IN 47907
765-494-5317
chinaraju@purdue.edu

Brian Bischoff
Oak Ridge National Laboratory
PO Box 2008
Oak Ridge, TN 37831-6044
865-241-3172
bischoffbl@ornl.gov

Larry Blair
Consultant
1500 Bridger Road
Rio Rancho, NM 87144
505-896-6686
larry.blair@ee.doe.gov

Judy Blanchard
Chevron

James Blencoe
Research Geochemist
Oak Ridge National Laboratory
P.O. Box 2008, Building 4500-S
Oak Ridge, TN 37831-6110
865-574-7041
blencoejg@ornl.gov

David Block
Hydrogen Program Director
Florida Solar Energy Center
1679 Clearlake Road
Cocoa, FL 32922-5703
321-638-1001
block@fsec.ucf.edu

Ira Bloom
Manager
Argonne National Laboratory
9700 S Cass Avenue
Argonne, IL 60439
630-252-4516
ira.bloom@anl.ogv

APPENDIX A: ATTENDEE LIST

Robert Boehm
UNLV/UNLV Research Foundation
4505 Maryland Parkway, MS-4027
Las Vegas, NV 89154
702-895-4160
boehm@me.unlv.edu

James Boncella
Los Alamos National Laboratory
Chemistry Division, P.O. Box 1663, MS J514
Los Alamos, NM 87544
505-661-0347
boncella@lanl.gov

Klaus Bonhoff
Sr. Manager, Fuel Cell Systems
DaimlerChrysler AG
Bldg. 1, Rm 01415 Neue Str. 95
Kircheim unter Teck, 73230
011-49-7021-89-3474
klaus.bonhoff@daimlerchrysler.com

Brian Bonner
Global Product Manager
Air Products and Chemicals, Inc.
7201 Hamilton Boulevard
Allentown, PA 18240
610-481-5222
bonnerbb@airproducts.com

Christopher Bordeaux
President
Bordeaux International Energy Consulting, LLC
8727 Braddock Avenue
Alexandria, VA 22309
703-360-4366
h2bordeaux@aol.com

Oleg Borodin
Research Assistant Professor
University of Utah
122 S. Central Campus Dr. Rm 304
Salt Lake City, UT 84112
801-581-8991
Oleg.Borodin@utah.edu

William Borthwick
European Commission
rue de la Loi 200 (CDMA 05/207)
Brussels, 1049
+32 2 2965025
william.borthwick@ec.europa.eu

Michael Bortner
Conductive Composites Group Leader
NanoSonic, Inc.
1485 S Main St
Blacksburg, VA 24060
540-953-1785
mbortner@nanosonic.com

Rod Borup
Team Leader
Los Alamos National Laboratory
P.O. Box 1663, MS J579
Los Alamos, NM 87544
505 667 2823
Borup@lanl.gov

Arun Bose
DOE-NETL
626 Cochrans Mill Road
Pittsburgh, PA 15236-0940
412-386-4467
arun.bose@netl.doe.gov

Richard Bourgeois
GE Global Research Center
ES606 1 Research Circle
Niskayuna, NY 12309
518-387-4550
bourgeoi@crd.ge.com

Nico Bouwkamp
Technology Analyst
California Fuel Cell Partnership
3300 Industrial Blvd., Suite 1000
West Sacramento, CA 95691
916-375-8050
nbouwkamp@cafcp.org

Mark Bowden
Industrial Research
PO Box 31-310
Lower Hutt, 6009
4 9313 178
m.bowden@irl.cri.nz

Robert Bowman
JPL Task Manager MHCoE
Jet Propulsion Laboratory
Mail Stop 79-24 4800 Oak Grove Dr
Pasadena, CA 91109-8099
818-354-7941
robert.c.bowman-jr@jpl.nasa.gov

Wayne Britton
Chief Technology Officer
FuelSell Technologies, Inc.
1300 Valley House Drive
Rohnert Park, CA 94112
707-793-9313
wayne@fstenergy.com

Craig Brown
NIST
100 Bureau Dr
Gaithersburg, MD 20899
301-975-5134
craig.brown@nist.gov

Gilbert Brown
Oak Ridge National Laboratory
PO Box 2008
Oak Ridge, TN 37922
865-576-2756
browngm1@ornl.gov

Kenneth Brown
Vice President
Safe Hydrogen, LLC
99 Sanborn Lane
Reading, MA 1867
781-944-1133
kbrown@safehydrogen.com

Lloyd Brown
Senior Technical Advisor
General Atomics
Box 85608
San Diego, CA 92186
858-455-3078
lloydbrown@gat.com

Richard Budzich
Budget Analyst
U.S. DOE- EE- Business Admin
1000 Independence Ave, SW
Washington, DC 20858
202-586-5546
Richard.Budzich@ee.doe.gov

John Bullis
President
Xonics Corporation
4500 N. Hiatus Rd. # 206
Sunrise, FL 33351
954-536-4637
jbullis@xonics.com

Israel Cabasso
Pofessor
State University fo New York - Syracuse
SuNY-ESF
Syracuse, NY 13210
315-470-6857
icabasso@syr.edu

Gordon Calundann
CTO
E-TEK Division, PEMEAS USA
39 Veronica Ave
Somerset, NJ 8873
732-545-5100 ext 174
g.calundann@etek-inc.com

Donald Camaioni
Pacific Northwest National Laboratory
PO Box 999
Richland, WA 99352
509-375-2739
Donna.Sturdevant@pnl.gov

Stephen Campbell
Ballard Power Systems
9000 Glenlyon Parkway
Burnaby, V5J 5J8
604-412-3151
stephen.campbell@ballard.com

Yan CAO
Lab Manager
Institute for Combustion Science and Environmental
2413 Nashville Road
Bowling Green, KY 42101
270-7790202
yan.cao@wku.edu

Robert Carling
Director
Sandia National Labs
7011 East Avenue MS9405
Livermore, CA 94550
925-294-2206
rwcarli@sandia.gov

Eric Carlson
Principal
TIAX LLC
20 Acorn Park
Cambridge, MA 2140
617-498-5903
carlson.e@tiaxllc.com

Todd Carlson
Senior Principal Project Engineer
Air Products and Chemicals, Inc.
7201 Hamilton Blvd - Mail Code A5315
Allentown, PA 18195-1501
610-481-4217
carlsote@apci.com

Joseph Carpenter
Technology Development Mgr.
U.S. Department of Energy
1000 Independence Ave. SW
Washington, DC 20585
202-586-1022
joseph.carpenter@ee.doe.gov

Daniel Casey
Chevron
3901 Briarpark Drive
Houston, TX 77042
713-954-6867
Dcasey@chevron.com

Richard Chahine
Director
Hydrogen Research Institute
C.P. 500
Trois-Riviýres, QC,, G9A 5H7
819-376-5011 x3588
richard.chahine@uqtr.ca

Steven Chalk
DOE Hydrogen Program Manager
U.S. Department of Energy
1000 Independence Ave SW EE2H
Washington, DC 20585
202 586 3388
steven.chalk@ee.doe.gov

Dhanesh Chandra
Professor
University of Nevada, Reno
MS 388, Metallurgical and Materials Engr.
Reno, NV 89557
775-784-49060
dchandra@unr.edu

Cheng Chen
Georgia Tech
School of Chemical & Biomolecular Engineering, 311
Atlanta, GA 30332
404-894-2834
cheng.chen@chbe.gatech.edu

Gang Chen
Professor
Massachusetts Institute of Technology
77 Massachusetts Avenue, 3-260
Cambridge, MA 2139
617-253-0006
gchen@mit.edu

Long-Qing Chen
Professor
Penn State University
102 Steidle Building
University Park, PA 16802
814-863-8101
lqc3@psu.edu

Stanley Chen
general engineer
DOE-FE
19901 Germantown road
germantown, MD 20874
301-903-2827
stanley.chen@hq.doe.gov

Yok Chen
Program Manager
U.S. Department of Energy
Basic Energy Sciences
Germantown, MD 20874-1290
301-903-4174
yok.chen@science.doe.gov

Hansong Cheng
Lead Chemist
Air Products & Chemicals, Inc.
7201 Hamilton Blvd
Allentown, PA 18195
610-481-8502
chengh@apci.com

William Chernicoff
Hydrogen Engineer / Hydrogen Program manager
US DOT - RITA
400 7th St. SW
Washington, DC 20001
202-366-4999
william.chernicoff@dot.gov

Fred Chesley
Engineer
Startech Environmental Corp
190 Century Drive
Bristol, CT 6010
860-582-6190
fchesley@startech.net

Francis Chiaramonte
Program Exec
NASA
300 E. St. SW
Washington, DC 20540
202-358-0693
francis.p.chiaramonte@nasa.gov

Arthur Chin
Rohm and Haas Company
3100 State Road
Croydon, PA 19021
215-785-7249
achin@rohmhaas.com

Kwontae Cho
Engineering Manager
Hyundai America Tech Center
12610 East End Ave.
Chino, CA 91710
909-993-5440
motolo@hyundai-motor.com

Mei-Yin Chou
Chair and Professor/ School of Physics
Georgia Institute of Technology
837 State Street
Atlanta, GA 30332-0430
404-894-4688
meiyin.chou@physics.gatech.edu

Biswajit Choudhury
Sr. Research Scientist
E.I. Dupont Company
Dupont Fuel Cells, Experimental Station E262/435
Wilmington, DE 19880
302-695-6801
biswajit.choudhury@can.dupont.com

Joel Christian
Manager, Advanced Research
OSRAM SYLVANIA
Hawes Street
Towanda, PA 18848
570-268-5345
joel.christian@sylvania.com

Deryn Chu
U. S. Army Research Laboratory
2800 Powder Mill Rd.
Adelphi, MD 20783-1197
301-394-0308
dchu@arl.army.mil

Steven Chuang
Professor
The University of Akron
201 E Buchtel Common
Akron, OH 44325-3906
330-972-6993
schuang@uakron.edu

Helena Chum
Group Manager, Sr. Advisor
National Renewable Energy Laboratory
1617 Cole Blvd., MS-3311
Golden, CO 80401
303-384-7711
helena_chum@nrel.gov

T. C. Mike Chung
Professor
Penn State University
325 Steidle Building
University Park, PA 16802
814-8631394
chung@ems.psu.edu

Daniel Cicero
H2 & Syngas Technology Manager
National Energy Technology Laboratory
3610 Collins Ferry Road
Morgantown, WV 26507-0880
304-285-4826
daniel.cicero@netl.doe.gov

Richard Ciora
Media and Process Technology Inc.
1155 William Pitt Way
Pittsburgh, PA 15238
412-826-3716
rciora@mediaandprocess.com

Bill Cleary
Argonne National Laboratory
10505 Woodleigh Court
Beltsville, MD 20705
202-586-1570
bill.cleary@ee.doe.gov

Simon Cleghorn
Technical leader
W.L. Gore and Associates
201 Airport Road
Elkton, MD 21922
410-506-7634
scleghorn@wlgore.com

Alex Clem
Physical Chemist/Env. Scientist
Private (formerly with EPA)
108 Walnut St. NW
Washington, DC 20012-2155
202-726-2812
alex01022003@aol.com

Bruce Clemens
Professor
Stanford University
416 Escondido Mall, Building 550
Stanford, CA 94305-2205
650-725-7455
bmc@stanford.edu

Michael Clifford
Process Engineer
HyRadix, Inc.
175 W. Oakton
Des Plaines, IL 60018
847-391-1254
michael.clifford@hyradix.com

Beatrice Coda
European Commission official
European Commission
Rue de la Loi 200
Brussels, 1000
0032 2 29 78484
beatrice.coda@cec.eu.int

Philippe Coignet
Research Engineer
Air Liquide
305 Water Street
Newport, DE 19804
302 2252180
philippe.coignet@airliquide.com

Whitney Colella
Engineering Research Associate
Stanford University
P.O. Box 19546
Stanford, CA 94309-9546
650-283-2701
wcolella@alumni.princeton.edu

William Collins
Staff Engineer
UTC Power/Fuel Cells
195 Governors highway
South Windsor, CT 6074
860-727-2559
william.collins@UTCpower.COM

Charliee Collins II
DOE
6113 Hellen Lee Drive
Clinton, MD 20735
202-441-0639
ccolinsii@hotmail.com

Mark Conradi
Professor
Washington University
Dept of Physics-1105
Saint Louis, MO 63130-4899
314-935-6418
msc@wuphys.wustl.edu

Mario Conte
Dr. /Program-Project Manager
Italian National Agency - ENEA
Via Anguillarese, 301
S. Maria di Galeria (Roma), 66 or -6292
conte@casaccia.enea.it

Cristian Contescu
Senior Research Staff Scientist
Oak Ridge National Laboratory
1 Bethel Valley Road, PO Box 2006, MS 6087
Oak Ridge, TN 37831-6087
(39063) 048-4829
contescuci@ornl.gov

Alan Cooper
Sr. Principal Research Chemist
Air Products and Chemicals, Inc.
7201 Hamilton Blvd.
Allentown, PA 18195
865-241-3318
cooperac@airproducts.com

Christy Cooper
U.S. DOE Hydrogen Program
1000 Independence Ave, SW EE-2H
Washington, DC 20585
610-481-2607
christy.cooper@ee.doe.gov

Chris Cornelius
Technical Staff
Sandia National Laboratories
PO Box 5800 MS 0734
Albuquerque, NM 87185
202-586-1885
cjcorne@sandia.gov

Dennis Corrigan
President/COO
Ovonic Fuel Cell Company
2983 Waterview Drive
Rochester Hills, MI 48309
505-844-6192
dcorrigan@ovonic.com

Randy Cortright
CTO IEVP
Virent Energy Systems
3571 Anderson street
Madison, WI 53704
248-293-7000
randycortriht@virent.com

Stephen Creager
Professor
Clemson University
Hunter Laboratory, Clemson University
Clemson, SC 29634
608-663-0228
screage@clemson.edu

Vincent Crespi
Penn State University
104 Davey Lab MB163
University Park, PA 16802
864-656-4995
doeh2.20.ipserc@spamgourmet.com

Cecelia Cropley
Director/Systems Engineering
Giner Inc.
89 Rumford Ave
Newton, MA 2466
814 863-0163
ccropley@ginerinc.com

James Cross
Vice President
Nuvera
20 Acron Park Road
Cambridge, MA 2140
781-529-0506
jcross@nuvera.com

John Crossen
Associate/Environmental Attorney
Booz Allen Hamilton
1550 Crystal Drive, Suite 1100
Arlington, VA 22202-4158
617-245-7568
crossen_john@bah.com

Maria Curry-Nkansah
Dr./Business Development Manager
BP
150 West Warrenville Rd.
Naperville, IL 60563-8460
703-412-7808
curry-me@bp.com

Dennis Curtin
Technology Associate
DuPont Fuel Cells
22828 NC Hwy 87 W
Fayetteville, NC 28306
630-420-5832
dennis.e.curtin@usa.dupont.com

David Dacosta
President
HERA USA Inc.
373 Margaret Fing Ave
Ringwood, NJ 7456
910-678-1224
dacosta@ergenics.com

Bing Dai
University of Pittsburgh
1249 Benedum Hall, University of Pittsburgh
Pittsburgh, PA 15261
973-728-8815
bdai@puccini.che.pitt.edu

Akimasa Daimaru
Chief Engineer
Honda R&D Co.,Ltd
4630 Shimotakanezawa, Haga-machi
Haga-gun, Tochigi, 321-3393
412-708-9927
akimasa_daimaru@n.t.rd.honda.co.jp

Ashok Damle
RTI International
P.O. Box 12194
Research Triangle Park, NC 27709
28-677-6971
adamle@rti.org

Shilpa Damle-Mogri
Research Engineer
Shell Global Solutions
3333 Highway 6 South
Houston, TX 77082-3101
919-541-6146
shilpa.damle-mogri@shell.com

Subodh Das
President/CEO
Secat, Inc.
1505 Bull Lea Rd
Lexington, KY 40511
281-544-8242
skdas@secat.net

Patrick Davis
Hydrogen Production Team Leader
U.S. Department of Energy
1000 Independenc Ave SW
Washington, DC 20585
859-514-4989
patrick.davis@ee.doe.gov

Bob Dax
Manager, project development
Concurrent Technologies
425 Sixth Ave, 28th Floor
Pittsburgh, PA 15219
202-586-8061
sax@ctc.com

Lutgard De Jonghe
Professor
Lawrence Berkeley Nat'l Lab
MSD-Bldg 62; LBNL
Berkeley, CA 94720
412-992-5353
dejonghe@lbl.gov

Jesse Dean
Mechanical Engineer
Pacific Northwest Nat. Lab
Battloe blrd. P.O. Box 999 MSK6-10
WA 99352
510-486-6138
jesse.dean@pnl.gov

Satyen Deb
Center Director
National Renewable Energy Laboratory
1617 Cole Boulevard
Golden, CO 80401-3393
509-375-6348
satyen_deb@nrel.gov

Mark Debe
Sr. Staff Scientist and Technical Manager
3M Company
201-2N-19, 3M Center
St. Paul, MN 55144
303-384-6405
mkdebe1@mmm.com

Gerald DeCuollo
President/CEO
TreadStone Technologies, Inc.
201 Washington Road
Princeton, NJ 08543-5300
651-736-9563
gdecuollo@treadstone-technologies.com

Xunming Deng
Professor
University of Toledo
2801 W. Bancroft Street, MS111
Toledo, OH 43606
609-734-2368
dengx@physics.utoledo.edu

Robert Derby
Principal
RWD Associates
74 Drumwood Road
Martinsburg, WV 25401
419-530-4782
rwderby@rwdassociates.com

Darryl DesMarteau
Professor
Clemsom University
359 Hunter Chemistry Lab.
Clemson, SC 296340973
508-254-9668
fluorin@clemson.edu

Pete Devlin
U.S. Department of Energy
1000 Independence Ave
Washington, DC 20585
864-656-4705
peter.devlin@ee.doe.gov

Pankaj Dhingra
President and CEO
Nanostellar, Inc.
3696 Haven Avenue
Redwood City, CA 94063
656-1251
pdhingra@nanostellar.com

Anita DiAndreth
Global product and Planning Manager
DuPont Fuel Cells
PO Box 80701
Wilmington, DE 19880-0701
202-586-4905
anita.diandreth-1@usa.dupont.com

Howard Dillman
Chair, Transportation Technologies Department
Lansing Community College
4100W-Transportation Technologies, P.O. Box 40010
Lansing, MI 48901-7210
650-381-7152
dillmanh@lcc.edu

David Dixon
Robert Ramsay Chair
The University of Alabama
Chemistry Dept.,, Shelby Hall, Box 870336
Tuscaloosa, AL 35487-0336
302-999-3557
dadixon@bama.ua.edu

Tabbetha Dobbins
Assistant Professor
Louisiana Tech University
911 Hergot Avenue
Ruston, LA 71272
517-483-1533
tdobbins@latech.edu

Richard Doctor
Section Leader H2 and Greenhouse Gas
Argonne National Laboratory
9700 S. Cass Ave.
Argonne, IL 60439
205-348-8441
rdoctor@ anl.gov

Junhang Dong
Associate Professor
New Mexico Tech
801 Lexoy Place
Socorro, NM 87801
318-257-5234
jhdong@nmt.edu

Nyla Dookeran
Technical Manager
DuPont Fuel Cells
Chestnut Run Plaza, Centre Boulevard, Building 701
Wilmington, DE 19805
630-252-5913
nyla.n.dookeran@usa.dupont.com

Shain Doong
Gas Technology Institute
1700 South Mount Prospect Road
Des Plaines, IL 60018
505-835-5293
shain.doong@gastechnology.org

Mildred Dresselhaus
Institute Professor
MIT
Room 13-3005
Cambridge, MA 2139
302-999-4642
millie@mgm.mit.edu

Wally Dubno
Sr Product Engineer
Quantum Technologies
17872 Cartwright Rd
Irvine, CA 92614
847-7680884
wdubno@qtww.com

Michael Duffy
Systems Integration Office - NREL
1617 Cole Blvd
Golden, CO 80401
617-253-6864
michael_duffy@nrel.gov

James Dunn
CEO/CFO
CTC, Inc.
134 Flanders Road, # 375
Westborough, MA 1581
949-399-4653
jdunn@ctc.org

Vincent Durante
Catalysis Technology Leader
W.L. Gore and Associates
201 Airport Road, PO Box 1488
Elkton, MD 21922-1488
303-275-3022
vdurante@wlgore.com

Ali Ebadian
Consultant
GTI
2516 Monteray Ct.
Weston, FL 33327
508-870-0042
mebadian@yahoo.com

Juergen Eckert
University of California
Materials Research Laboratory
Santa Barbara, CA 93106
410 506 7524
juergen@mrl.ucsb.edu

Mohamed Eddaoudi
Assistant Professor
University of South Florida
4202 E. Fowler Avenue CHE203
Tampa, FL 33647
786-325-4577
eddaoudi@cas.usf.edu

Anthony Eggert
Assoc. Director
Institute of Trans. Studies
One Shields Avenue
Davis, CA 95616
805-893-8247
areggert@ucdavis.edu

Glenn Eisman
Professor
Rensselaer Polytechnic Institute
110 8th Street - 142 MRC
Troy, NY 12180
813-974-9622
eisman@rpi.edu

Peter Eklund
Professor
Penn State University
104 Davey Lab
University Park, PA 16802
530-902-3201
pce3@psu.edu

Carolyn Elam
U.S. Department of Energy
1617 Cole Blvd.
Golden, CO 80401
518-276-3878
carolyn.elam@go.doe.gov

S. (Elango) Elangovan
SOFC Projects Manager
Ceramatec, Inc.
2425 South 900 West
Salt Lake City, UT 84119-1517
814-865-5233
elango@ceramatec.com

Anter El-Azab
Assoc. Professor
Florida State University
2525 Potts Jamer Street
Tallahassee, FL 32310
303-275-4953
anter@eng.fsu.edu

John Elter
CTO
Plug Power Inc.
968 Albany Shaker Rd.
Latham, NY 12110
801-978-2162
john_elter@plugpower.com

Sean Emerson
United Technologies Research Center
411 Silver Lane
East Hartford, CT 6108
850-410-6655
emersosc@utrc.utc.com

Mohammad Enayetullah
VP, Advanced Technology
Protonex Technology
153 Northboro Rd
Southborough, MA 1772
518-738-0997
mae@protonex.com

Kathi Epping
Fuel Cell Technology Development
Manager
U.S. Department of Energy
1000 Independence Ave, SW
Washington, DC 20585
860-610-7524
Kathi.Epping@ee.doe.gov

William Ernst
Chief Scientist
Plug Power Inc.
968 Albany-Shaker Road
Latham, NY 12110
508-490-9960
william_ernst@plugpower.com;donn
a_ranalli@plugpowe

Gonzalo Escobedo
Division Engineer
DuPont Fuel Cells
701 Chestnut Run Plaza
Wilmington, DE 19805
202-586-7425
gonzalo.escobedo@usa.dupont.com

Linda Eslin
Senior Process Engineer
Concurrent Technologies Corporation
100 CTC Drive
Johnstown, PA 15904
518-738-0126
eslinl@ctc.com

Leslie Eudy
Sr. Project Leader
National Renewable Energy
Laboratory
1617 Cole Blvd.
Golden, CO 80401
302-999-2762
leslie_eudy@nrel.gov

Robert Evans
National Renewable Energy
Laboratory
1617 ColeBlvd
Golden, CO 80401
814-269-2841
Bob_Evans@nrel.gov

Samuel Evans
Research Analyst
NEDO
1220 19th St. NW Suite 500
Washington, DC 20036
303-275-4412
sevans@numarkassoc.com

Matthew Fairlie
Director Special Projects
Canadian Hydrogen Association
Fairfield Farm
RR4 Shelburne, Ontario, L0N 1S8
303-275-3708
fairlie@h2.ca

Chinbay Fan
R&D Manager
Gas Technology Institute
1700 S. Mt. Prospect Rd.
Des Plaines, IL 60018
202-466-2700
fan@gastechnology.org

Zhigang Fang
Professor
University of Utah
135 S. 1460 E. Room 412
Salt Lake City, UT 84112
416-806-8486
zfang@mines.utah.edu

David Farese
Engineering Manager
Air Products
7201 Hamilton Blvd
Allentown, PA 18195
847-768-0812
faresedj@airproducts.com

Robert Faron
Energy practice leader
Project Performance Corporation
1760 Old Meadow Road
McLean, VA 22102
801-581-8128
rfaron@ppc.com

Robert Farrauto
Research Fellow
Engelhard
101 Wood Avenue
Iselin, NJ 8830
610-181-4869
bob.farrauto@engelhard.com

Doug Faulkner
Principal Deputy Assistant Secretary
U.S. Department of Energy
703-748-7028
doug.faulkner@ee.doe.gov

Erika Felix
Research Analyst
IPHE
1000 Independence Ave SW
Washington, DC 20585
732-205-5306
efelix@sentech.org

James Fenton
Director
Florida Solar Energy Center
1679 Clearlake Road
Cocoa, FL 32922-5703
202-586-9220
jfenton@fsec.ucf.edu

L. Ferguson
President & CEO
The Enterprise Center
1250 Market Street, Suite 3020
Chattanooga, TN 37402
202-586-2790
ferguson_j@theenterprisectr.org

William Fernald
Senior Program Manager
U.S. DOE/FE-24 GTN
1000 Independence Ave., SW
Washington, DC 20585-1290
321-638-1002
william.fernald@hq.doe.gov

Karl Fiegenschuh
Manager, Freedomcar Triad
Ford Motor Company
2101 Village Road; Mail Drop 2247
Dearborn, MI 48121
423-425-3770
kfiegens@ford.com

Constantina Filiou
Scientific Officer
European Commission - Dg Jrc
PO Box 2
Petten, NL-1755 ZG
301-903-9448
constantina.filiou@jrc.nl

Florian Finsterwalder
Dr., Manager MEA Development
DaimlerChrysler
Wilhelm-Runge-Strasse 11
Ulm, 89081
313-337-3125
florian.finsterwalder@daimlerchrysle
r.com

John Fischer
Professor
University of Pennsylvania
MSE Dept 3231 Walnut St.
Philadelphia, PA 19104-6272
224- 56-5171
fischer@seas.upenn.edu

Tim Fitzsimmons
U.S. Department of Energy
Division of Materials Sciences and
Engineering, SC
Washington, DC 20585-1290
+49 731 505 2995
tim.fitzsimmons@science.doe.gov

Greg Fleming
Research Director
Air Liquide
305 Water Street
Newport, DE 19806
215-898-6924
greg.fleming@airliquide.com

Paula Flenory
Project Manager
US DOE/NETL
P. O. Box 10940-MS 922-273C
Pittsburgh, PA 15236
301-390-9830
flenory@netl.doe.gov

Robert Fletcher
Associate Professor of Mechanical
Engineering
Lawrence Technological University
21000 West Ten Mile Road
Southfield, MI 48075-1058
302-225-2114
rfletcher@LTU.edu

Chris Forbes
Siemens
1310 Beulah Rd
Pittsburgh, PA 15235
412-386-4781
chrisforbes@siemens.con

Richard Fosmoe
Project Manager
DaimlerChrysler
3300 Industrial Blvd
West Sacramento, CA 95691
248-204-2525
richard.fosmoe@daimlerchrysler.com

Donald Foster
Consultant, Ind. Partnerships
LBNL
1 Cyclotron Road, MS 90R3027D
Berkeley, CA 94720-8131
412-256-2022
dgfoster@lbl.gov

Michel Foure
Director R&D
Arkema Inc.
900 First Avenue
King of Prussia, PA 19406
916-205-1406
michel.foure@arkemagroup.com

Henry Fowler
Project Analyst - Hydrogen Fuel
Cells and Infrastructure
Navarro Research & Engineering
1617 Cole Blvd
Golden, CO 80401
510-486-6175
henry.fowler@go.doe.gov

David Frank
Director of Technology
Hydrogenics Corp
5985 McLaughlin Ave
Mississauga, L5R 1B8
610-878-6790
dfrank@hydrogenics.com

Scott Freeman
Manager - Business Policy &
Strategy
DaimlerChrysler Corporation
800 Chrysler Drive
Auburn Hills, MI 48326-2757
303-275-4903
sjf@dcx.com

Greg Frenette
Chief Engineer - Fuel Cell Programs
Ford Motor Company
39263 Lorien Drive
Sterling Heights, MI 48313
905-361-3660
gfrenett@ford.com

Robert Friedland
Senior VP Hydrogen Technology Group
Proton Energy Systems, Inc.
10 Technology Drive
Wallingford, CT, CT 6492
248-576-8220
rob.friedland@protonenergy.com

Paul Friley
BNL
901 D Street, SE Suite 900
Washington, DC 20024
313-390-2505
pfriley@bnl.gov

Katrina Fritz Intwala
Director of Government Relations
Plug Power
968 Albany Shaker Road
Latham, NY 12144
203-678-2144
katrina_fritzintwala@plugpower.com

George Froudakis
Assistant Professor of Computational Chemistry
University of Crete
PO.BOX 1470
Crete, 714 09
202-646-5206
frudakis@chemistry.uoc.gr

Cy Fujimoto
Sandia National Laboratories
P.O. Box 5800
Albuquerque, NM 87185
518-738-0369
chfujim@sandia.gov

Thomas Fuller
Professor
Georgia Tech
311 Ferst Drive
Atlanta, GA 30332-0100
-2269
tom.fuller@gtri.gatech.edu

Richard Fullerton
NASA
300 E Street SE
Washington, DC 20564
505-844-6432
rfullert@nasa.gov

Stuart Funk
Research Fellow
LMI
2000 Corporate Ridge
McLean, VA 22102-7805
404-894-2898
sfunk@lmi.org

Kazuyoshi Fushinobu
Associate Professor
Tokyo Institute of Technology
2-12-1-I1-6, O-okayama
Meguro-ku, Tokyo, 152-8552
202-358-1453
fushinok@mech.titech.ac.jp

Scott Gaboury
Manager, Coatings & Films
Arkema Inc.
900 First Ave.
King of Prussia, PA 19406
703-917-7277
scott.gaboury@arkemagroup.com

Alexei Gabrielov
Shell
3333 Highway 6 South
Houston, TX 77082
3-5734-2500
alexei.gabrielov@shell.com

Sarang Gadre
Air Liquide
305 Water Street
Newport, DE 19809
610-878-6766
sarang.gadre@airliquide.com

Nidia Gallego
Oak Ridge National Laboratory
1 Bethel Valley Rd. - M.S. 6087
Oak Ridge, TN 37831-6087
281-544-8101
gallegonc@ornl.gov

Barbara Galuszka-Muga
TOFTEC, Inc.
PO Box 15358
Gainesville, FL 32604
302-225-2182
muga@chem.ufl.edu

Jennifer Gangi
Program Director
Fuel Cells 2000
1100 H Street, NW Suite 800
Washington, DC 20005
865-241-9459
jennifer@fuelcells.org

Jason Ganley
Asst. Professor
Howard University
2300 6th Street NW
Washington, DC 20059
352-392-1441
jganley@howard.edu

John Garbak
Technology Development Manager
U.S. Department of Energy
1000 Independence Ave
Washington, DC 20585
202-785-4222
john.garbak@ee.doe.gov

Monterey Gardiner
Fuel Cell Engineer
Hyundai Kia America Tech. Center Inc.
3300 Industrial Blvd. Suite 300
West Sacramento, CA 95691
202-806-4796
mgardiner@hatci.com

Dale Gardner
Director, Systems Integration
National Renewable Energy Laboratory
1617 Cole Blvd, MS 1732
Golden, CO 80403
202-586-1723
dale_gardner@nrel.gov

Wayne Gardner
Franklin Fuel Cells
83 Great Valley Parkway
Malvern, PA 19355
916-374-1978
wgardner@franklinfuelcells.com

Nancy Garland
U.S. Department of Energy
1000 Independence Avenue SW
Washington, DC 20585
303-275-3020
Nancy.garland@ee.doe.gov

Roxanne Garland
Technology Development Manager
U.S. Department of Energy
1000 Independence Ave, SW
Washington, DC 20585
610-640-7547
roxanne.garland@ee.doe.gov

David Garman
U.S. Department of Energy
202-586-5673

Fernando Garzon
Team Leader
Los Alamos National Laboratory
MS D429
Los Alamos, NM 87506
202-586-7260
garzon@lanl.gov

Jack Gatzuras
Program Manager- DOE Fleet
UTC Power
195 Governor's Highway
South Windsor, CT 6074
jack.gatzuras@utcpower.com

Qingfeng Ge
Assistant Professor
Southern Illinois University Carbondale
1245 Lincoln Dr.
Carbondale, IL 62901
505-667-6643
qge@chem.siu.edu

Mark Gee
Honeywell
2525 West 190th Street
Torrance, CA 90504
860-727-2875
mark.gee@honeywell.com

Henry Gentenaar
The Media Network
8720 Georgia Ave. Suite 606,
Silver Spring, MD 20910
618-453-6406
hgentenaar@aol.com

David Geohegan
Research Staff
Oak Ridge National Laboratory
1 Bethel Valley Road, MS-6056
Oak Ridge, TN 37831-6056
310-512-3606
odg@ornl.gov

Dominic Gervasio
Assoc. Prof. Research
Arizona State University
1001 S. McAllister Ave
Tempe, AZ 85287-5001
301-565-0770
don.gervasio@asu.edu

Lawrence Gestaut
Vice-President
Giner Electrochemical Systems, LLC
89 Rumford Avenue
Newton, MA 02466-1311
865-576-5097
lgestaut@ginerinc.com

Maria Ghirardi
Sr. Scientist
National Renewable Energy Laboratory
1617 Cole Blvd.
Golden, CO 80401
480-727-8169
maria_ghirardi@nrel.gov

Joseph Giove
Sr. Program Manager
Department of Energy
1000 Independence Avenue SW
Washington, DC 20585-1290
781-529-0533
joseph.giove@hq.doe.gov

Robert Glass
Lawrence Livermore National Laboratory
P. O. Box 808, L-644
Livermore, CA 94550
303-384-6312
glass3@llnl.gov

Bruno Gnoerich
Research Engineer (Alternative Drivetrains)
RWTH Aachen University, Aachen/Germany
Steinbachstr. 7
Aachen, 52064
301-903-4130
gnoerich@ika.rwth-aachen.de

Yury Gogotsi
Professor and Associate Dean
College of Engineering
Materials Science and Engineering
3141 Chesnut Sr.
Philadelphia, PA 19104
925-423-7140
gogotsi@drexel.edu

James Goldbach
Research Scientist
Arkema Inc.
900 First Ave.
King of Prussia, PA 19406
+49(0)2418025617
james.goldbach@arkemagroup.com

Marcos Gomez
Innovation Manager
BASF Venture Capital America
1114 State Street , suite 239
Santa Barbara, CA 93101
215-895-6446
marcos.gomez@basf.com

Jay Gore
Associate Dean/Director Energy Center
Purdue University
400 Centennial Mall Drive
West Lafayette, IN 47907
610-878-6661
gore@purdue.edu

Dharendra Goswami
Co-Director
U of South Florida's Clean Energy Research
4202 East Fowler Avenue, ENB 118
Tampa, FL 33620-5250
805-966-1644
goswami@eng.usf.edu

Andrew Goudy
Professor of Chemistry
Delaware State University
1200 N. Dupont Highway
Dover, DE 19901
765-494-2122
agoudy@desu.edu

Jason Graetz
Scientist
Brookhaven National Laboratory
Bldg. 815 Brookhaven National Lab
Upton, NY 11973
813-974-7322
graetz@bnl.gov

Leo Grassilli
Alternate Fuels Consultant
Office of Naval Research
13179 Oak Farm Drive
Woodbridge, VA 22192
302-857-6534
Grassilli@FuelCells.com

Dan Graves
Acumentrics Corporation
20 Southwest Park
Westwood, MA 2090
631-344-3242
dgraves@acumentrics.com

Paul Grimmer
President
Eltron Research Inc.
4600 Nautilus Court South
Boulder, CO 80301
703-494-1770
pgrimmer@eltronresearch.com

Sigmund Gronich
Technology Validation Manager
U.S. Department of Energy
203 Yoakum Pkwy
Alexandria, VA 22304
781-461-8251
sigmund.gronich@ee.doe.gov

Celine Gross
DaimlerChrysler
800 Chrysler Drive, CIMS 482-01-20
Auburn Hills, MI 48326-2757
303-530-0263
cvg2@dcx.com

Karl Gross
CEO
Hy-Energy, LLC
8440 Central Ave
Newark, CA 94560
202-586-1623
karljgross@hy-energy.com

Thomas Gross
IF,LLC and LMI
4121 Evergreen Drive
Fairfax, VA 22032
248-944-1113
tgenergy@cox.net

Thomas Grosskopf
Program Manager
Modine Manufacturing Company
1500 DeKoven Avenue
Racine, WI 5340
510-793-3345
t.grosskopf@na.modine.com

Stephen Grot
Ion-Power
720 Governor Lea Road
NewCastle, DE 19720
703-273-0631
S.Grot@ion-power.com

Jill Gruber
DOE GFO
1617 Cole Blvd
Golden, CO 80401
262-636-1638
jill.gruber@go.doe.gov

Jie Guan
GE Global Research
19310 Pacific Gateway Drive
Torrance, CA 90503
302-832-9550
jie.guan@ge.com

Mallika Gummalla
United Technologies Research Center
411 Silver Lane
East Hartford, CT 6108
303-275-4961
gummalm@utrc.utc.com

Raghubir Gupta
Senior Director
RTI International
P.O. Box 12194
RTP, NC 27709
310-5387254
gupta@rti.org

David Guro
Product Manager
Air Products and Chemicals, Inc.
7201 Hamilton Blvd
Allentown, PA 18195
860-610-7383
gurode@airproducts.com

Maciej Gutowski
Pacific Northwest National Laboratory
PO Box 999, K1-83
Richland, WA 99352
919-541-8023
Donna.Sturdevant@pnl.gov

Christopher Guzy
Chief Technology Officer
Ballard Power Systems
4343 North Fraser Way
Vancouver, BC, V5J 5J9
610-481-4625
chris.guzy@ballard.com

David Haberman
IF, LLC
POB 812291
Boca Raton, FL 33481
509-375-4387
ifdhllc@aol.com

Jerry Hallmark
Dir. Energy Technologies
Motorola Labs
2100 E. Elliot Road, EL309
Tempe, AZ 85284
604-762-7280
Jerry.Hallmark@motorola.com

Barry Halper
Manager, Contract Administration
Air Products and Chemicals, Inc.
7201 Hamilton Boulevard
Allentown, PA 18195
561-498-9646
halperbj@airproducts.com

Steven Hamrock
3M
3M Center Building 201-1W-28
St. Paul, MN 55082
480-413-3237
sjhamrock@mmm.com

Chul-Hee Han
IPHE visiting analyst
Mr
12563 Summit Manor Dr 613
Fairfax, VA 22033
610-481-7685
chul-hee.han@ee.doe.gov

Kiyoshi Handa
HONDA R&D Americas, Inc.
1900 Harpers way
Torrance, CA 90501
651-733-4254
khanda@hra.com

Thomas Hannum
VP Business Development
Hyoercino Engineering
1080 North Main St.
Brigham City, UT 84302
202-586-4507
thomas@hypercompegn.com

Donald Hardesty
Senior Manager, Combustion & Industrial Technology
Sandia National Laboratories
7011 East Avenue, MS 9054
Livermore, CA 94550
310-781-5538
drharde@sandia.gov

Angela Hardin
Associate Editor
Platts, Inside Energy
1200 G St., NW
Washington, DC 20005
801-510-9317
angela_hardin@platts.com

Jonathan Hardis
Scientific Advisor to Director, PL
NIST
100 Bureau Dr. Stop 8400
Gaithersburg, MD 20899-8400
925-294-2321
jhardis@nist.gov

William Haris
U.S. Army
1701 N. Beauregadr Street
Alexandria, VA 22311
202-383-2241
william.haris@us.army.mil

APPENDIX A: ATTENDEE LIST

Marianne Harmon
GE Global Research
1 Research Circle, CEB 455
Niskayuna, NY 12309
301-975-2373
harmon@research.ge.com

Mary Harrell
Staff
Staff
703-575-2859

Kevin Harrison
National Renewable Energy Lab
1617 Cole blvd.
Golden, CO 80401
518-387-4462
Kevin-harrison@nrel.gov

Ryan Harty
Engineer
Honda R&D Americas, Inc
1900 Harpers Way
Torrance, CA 90501
rharty@hra.com

Kazuhito Hato
Chief Officer
NEDO
20F Muza Kawasaki Building, 1310, Omiya-cho, Saiwa
Kawasaki City, Kanagawa Prefecture, 212-8554
303-384-7091
hatohkzh@nedo.go.jp

Robert Hauge
Technology Director
Rice University
6100 Main Street - MS100
Houston, TX 77005
310-781-6075
hauge@rice.edu

Bob Hawkins
Research Scientist
Eprida
1151 E Whitehall Rd. Bldg 2
Athens, GA 30605
81-44-520-5261
bob.hawkins@eprida.com

Marion Hawthorne
Co-director international Institute of Nano and Mo
University of Missouri-Columbia
301 Business Loop 70 West,DC 117.00
Columbia, MO 65211
713-348-6384
Hawthornem@health.missouri.edu or mfh@chem.ucla.ed

Ting He
Principal Scientist
Honda Research Institute USA
1381 Kinnear Rd.
Columbus, OH 43212
706-316-1765
the@honda-ri.com

Xiaoming He
Principal Technologist
Hydrogen Solar
350 Sunpac Ct.
Henderson, NV 89015
573-884-5021
xiaoming.he@hydrogensolar.com

Michael Heben
National Renewable Energy Laboratory
1617 Cole Blvd
Golden, CO 80401
614-327-4767
michael_heben@nrel.gov

Anthony Hechanova
Associate Research Professor
University of Nevada, Las Vegas
4505 Maryland Parkway, Box 454009
Las Vegas, NV 89154-4009
702-579-7600
hechanova@unlv.nevada.edu

Michael Heinekey
Professor
University of Washington
Dept of Chemistry
Seattle, WA 98195-1700
303-384-6641
heinekey@chem.washington.edu

Barbara Hennessey
Physical Scientist
USDOT/NHTSA
400 Seventh Street SW
Washington, DC 20590
702-895-1457
Barbara.Hennessey@nhtsa.dot.gov

Jim Henry
Professor
UT-Chattanooga
615 McCallie Av Dept 2503
Chattanooga, TN 37377
206-543-7522
jim-henry@utc.edu

Julie Herberg
Staff Physicist
LLNL
7000 East Avenue
Livermore, CA 94611
202-366-4714
herberg1@llnl.gov

Andy Herring
Associate Professor
CSM
Colorado School of Mines
Golden, CO 80466
423-425-4398
aherring@mines.edu

Steve Herring
Idaho National Laboratory
P.O. Box 1625, MS 3860
Idaho Falls, ID 43415-3860
925-422-5900
j.herring@inl.gov

Clemens Heske
Associate Professor
Department of Chemistry, University of Nevada, LV
4505 Maryland Parkway
Las Vegas, NV 89154-4003
303-384-2082
heske@unlv.nevada.edu

Edward Heydorn
Engineering Associate
Air Products and Chemicals, Inc.
7201 Hamilton Blvd.
Allentown, PA 18185
208-526-9497
heydorec@airproducts.com

Mike Hicks
Specialist
3M Company
3M Center, Bldg 0201-02-N-19
St. Paul, MN 55144-1000
702-895-2694
mike-hicks@mmm.com

David Hill
Inergy Automotive
2710 Bellingham
Troy, MI 48083
610-481-7099
david.hill@inergyautomotive.com

Shinichi Hirano
Manager/Fuel Cell Research
Ford Motor Company
1201 Village Rd., ASC Room 2A310
Dearborn, MI 48121
651-736-8433
shirano7@ford.com

Katsuhiko Hirose
Project General Manager
1200,Mishuku
Susono,Shizuoka, 410-1193
248-743-5747
hirose@katsuhiko.tec.toyota.co.jp

Kiyoshi Hiyama
Chief Officer
NEDO
20F Muza Kawasaki
Building,1310,Omiya-cho,Saiwai-k
Kawasaki City Kanagawa-ken, 212-8554
313-323-0783
hiyamakys@nedo.go.jp

Donna Ho
Technology Development Manager
U.S. Department of Energy
1000 Independence Ave, SW, EE-2H
Washington, DC, DC 20585
9772-997-55□
donna.ho@ee.doe.gov

Raymond Hobbs
Sr. Consulting Engineer
Arizona Public Service
502 S 2nd Ave
Phoenix, AZ 85003
81-44-520-5261
raymond.hobbs@aps.com

Ryan Hoesly
Engineer
SENTECH
7475 Wisconsin Avenue, Suite 900
Bethesda, MD 20814
202-586-8000
rhoesly@sentech.org

Peter Hoffmann
Editor & Publisher
The Hydrogen & Fuel Cell Letter
69 Grinnell St.; PO Box 14
Rhinecliff, NY 12574-0014
602-250-1510
info@hfcletter.com

Doug Hooker
Team Lead
DOE Golden Field Office
1617 Cole Blvd.
Golden, CO 80401
240-233-5500
Doug.Hooker@go.doe.gov

Scott Hopkins
Pall Corp
3669 St Rt 281
Cortland, NY 13045
845-876-5988
Scott_D._Hopkins@Pall.com

Michael Hsu
President
Ztek Corporation
300 West Cummings Park
Woburn, MA 1801
303-275-4780
mhsu@ztekcorporation.com

Hui Hu
Oak Ridge National Laboratory
Bethel Valley Road
OAK RIDGE, TN 37831
607-753-6041
huihu@ornl.gov

Gerald Huffman
Professor/Director
University of Kentucky/CFFS
Suite 107, 533 S. Limestone St.
Lexington, KY 40506-0043
781-933-8339
huffman@engr.uky.edu

Joyce Hung
GE Global Research
1 Research Circle, CEB-1593
Niskayuna, NY 12309
865-241-2559
hungj@research.ge.com

Rick Hurt
UNLV/UNLV Research Foundation
4505 Maryland Parkway, MS-4027
Las Vegas, NV 89154
859-257-4027
rikhurt@earthlink.net

Daniel Hussey
National Institute of Standards and Technology
100 Bureau Dr., MailStop 8461
Gaithersburg, MD 20899-8461
518-387-4046
daniel.hussey@nist.gov

Robert Hwang
Sandia National Labs
MS-1415
Albuquerque, NM 87185
702-895-1125
rqhwang@sandia.gov

Samir Ibrahim
Teledyne Energy Systems
10707 Gilroy Road
Hunt Valley, MD 21030
301-975-6465
samir.ibrahim@teledynees.com

Akihiro Iiyama
Expert Leader
Nissan Motor Co., Ltd. Fuel Cell Laboratory
1, Natsushima-cho
Yokosuka, 237-8523
505-844-5852
a-iiyama@mail.nissan.co.jp

Shinichi Ikeda
Senior Specialist
Toshiba Fuel Cell Power Systems Corporation
1-1, Shibaura 1-Chome, MInato-Ku
Tokyo, 105-8001
410-891-2222
shinichi2.ikeda@toshiba.co.jp

William Ingler Jr
Research Associate
University of Toledo
2801 W. Bancroft St., MS 111
Toledo, OH 43606
81-468-67-5174
bingler@physics.utoledo.edu

Donald Itzkoff
Foley & Lardner LLP
3000 K Street, NW
Washington,, DC 20007
03-3457-3624
ditzkoff@foley.com

Hiroyuki Iwabuchi
Chief Researcher
The Insitute of Applied Energy
Shinbashi SY BLDG. 14-2
Nishishinbashi 1-Chome, Mi
Tokyo, 105-0003
419-530-2651
iwabuchi@iae.or.jp

Takakuni IWASE
Manager
Nissan Motor Co.,ltd
1-Natsushima-cho
Yokosuka, 237-8523
202-295-4064
iwa-taka@mail.nissan.co.jp

Mahesh Iyer
The Ohio State University
140 W. 19th Ave, 125A Koffolt Labs
Columbus, OH 43210
81-3-3508-8894
iyer.25@osu.edu

Natraj Iyer
Director , Materials Science and Technology
Savannah River National Laboratory
Bldg 773-41A
Aiken, SC 29808
81-468675376
natraj.iyer@srnl.doe.gov

Jeffrey Jacobs
Vice President, Hydrogen Strategy, Planning & Business
Chevron Technology Ventures
6001 Bollinger Canyon Road
San Ramon, CA 94583
614-688-4912
jeffrey.jacobs@chevron.com

David Jacobson
Physicist
National Institute of Standards and Technology
100 Bureau Drive Stop 8461
Gaithersburg, MD 20899
803-725-2695
david.jacobson@nist.gov

Todd Jaffe
Vice President
American Wind power & Hydrogen Company
3420 Keyser Road
Baltimore, MD 21208
925-842-2797
tjaffe@windpowerandhydrogen.com

Satish Jalisatgi
Staff Researcher
UCLA
Department of Chemistry and Biochemistry, 607 Char
Los Angeles, CA 90095
301-975-6207
satish@chem.ucla.edu

Brian James
Research Director
Directed Technologies Inc.
3601 Wilson Blvd, Suite 650
Arlington, VA 22201
410-602-2589
Brian_James@DirectedTechnologies.com

Mark Janney
Senior Research Engineer
Porvair Advanced Materials
700 Shepherd St
Hendersonville, NC 28792
310-206-8641
mjanney@pamus.com

Thomas Jarvi
Mgr., Cell Stack Technology
UTC Power
195 Governor's Highway
South Windsor, CT 6074
703-778-7114
Tom.Jarvi@UTCPower.com

Puru Jena
Distinguished Professor
Virginia Commonwealth Univ
Physics Dept, Box 84-2000
Richmond, VA 23284-2000
828-694-3453
pjena@vcu.edu

Charles Jennings
BCS, Incorporated
5550 Sterrett Place, Suite 306
Columbia, MD 21044
860-727-7265
charles.jennings@ee.doe.gov

Craig Jensen
Professor
University of Hawaii
2545 McCarthy Mall
Honolulu, HI 96822
804-828-8991
jensen@gold.chem.hawaii.edu

Lisa Jerram
Technology Director
Breakthrough Technologies Inst
1100 H St NW, Suite 800
Washington, DC 20005
202-586-8196
lisa@fuelcells.org

Vijay John
Professor
Tulane University
Chemical and Biomolecular Engineering Department
New Orleans, LA 70118
808-956-2769
vijay.johhn@tulane.edu

Karl Johnson
Professor
University of Pittsburgh
1249 Benedum Hall, Dept. Chem. Eng.
Pittsburgh, PA 15261
202-785-4222 x16
karlj@puccini.che.pitt.edu

William Johnson
Associate
W. L. Gore & Associates
201 Airport Road, P. O. Box 1488
Elkton, MD 21922-1488
504-865-5883
wjohnson@wlgore.com

Christiina Johnston
Postdoctoral
LANL
MSD429, P.O.Box 1663
Los Alamos, NM 87545
412-624-5644
dbl@lanl.gov

Donald Jones
Vice President
RCF Economic & Financial Consulting, Inc.
333 N. Michigan Ave., Ste 804
Chicago, IL 60601
410-506-7567
djones@rcfecon.com

Karl Jonietz
Project Leader
Los Alamos National Laboratory
MS C331
Los Alamos, NM 87544
505-667-6832
kjonietz@lanl.gov

Scott Jorgensen
Manager energy storage systems
GM
30500 Mound rd. 480-106-160
Warren, MI 48090
312-431-1540
scott.w.jorgensen@gm.com

Fred Joseck
Technology Analyst
DOE Hydrogen Program
1000 Independence Ave SW
Washington D.C., DC 20585
505-667-1311
fred.joseck@ee.doe.gov

Tomohiro Kaburagi
Nissan Motor Co.,LTD
1,Natsushima-cho
Kanagawa Yokosuka-shi, 237-8523
586-986-1915
t-kaburagi@mail.nissan.co.jp

Jeff Kalinoski
Subcontractor
3601 Wilson Blvd. Suite 650
Arlington, VA 22201
202-586-7932
jkalinoski@directedtechnologies.com

Erik Kallio
US Army TARDEC
6501 E. 11 Mile, ATTN AMSRD-TAR-N 272
Warren, MI 48397-5000
81-46-867-5171
erik.kallio@us.army.mil

Izuru KANOYA
Honda R&D Co., Ltd.
1-4-1 Chuo
Wako, 351-0193
703-778-7117
izuru_kanoya@n.f.rd.honda.co.jp

Kal Kapur
Diversified Energy
P O Box 3
Tempe, AZ 85280
810-423-1746
highiq3000@yahoo.com

Andy Karsner
Assistant Secretary, Energy Efficiency & Renewable Energy
U.S. Department of Energy
48-461-2511
a.karsner@ee.doe.gov

Takehiko KATO
President
Interlink Corporation
3-4-11, Osaki, Shinagawa-ku
Tokyo, 141-0032
480-225-0005
katotsr@aol.com

Ursula Kattner
Physical Scientist
NIST
100 Bureau Dr. Stop 8555
Gaithersburg, MD 20899
202-586-9220
ursula.kattner@nist.gov

Joe Kaufman
Manager Fuel & Vehicle Trends
ConocoPhillips
818 Phillips Bldg
Bartlesville, OK 74004
81-3-5496-0856
joseph.w.kaufman@conocophillips.com

Susan Kauzlarich
Professor
UC Davis
Department of Chemistry
Davis, CA 95616
301-975-6044
smkauzlarich@ucdavis.edu

Kazuo Kawahara
Senior Executive Engineer
TTC-USA,INC
2350 Green Road
Ann Arbor, MI 48105
918-661-4084
kazuo.kawahara@tema.toyota.com

Maurice Kaya
Chief Technology Officer
State of Hawaii, DBEDT, SID
P.O. Box 2359
Honolulu, HI 96804
530-752-4756
mkaya@dbedt.hawaii.gov

Gregory Keenan
Air Products and Chemicals
7201 Hamilton Blvd
Allentown, PA 18195
734-995-0075
keenangr@airproducts.com

Steven Keinath
Research Scientist
Michigan Molecular Institute
1910 West St. Andrews Road
Midland, MI 48640-2696
808-587-3807
keinath@mmi.org

Jay Keller
Manager
Sandia National Laboratories
7011 East Ave. MS 9052
Livermore, CA 94551
610-481-2077
jokelle@sandia.gov

Richard Kelley
Program Manager BES/DOE
U.S. Department of Energy
1000 Independence Avenue
Washington, DC 20854
989-832-5555 x. 588
richard.kelley@science.doe.gov

Bruce Kelly
Senior Consultant
Nexant, Inc.
101 Second Street, 10th Floor
San Francisco, CA 94105-3672
925-294-3316
BDKelly@Nexant.com

Michael Kelly
Sr. Lead Scientist
Millennium Cell Inc.
One Industrial Way West
Eatontown, NJ 7724
301-903-6051
kelly@millenniumcell.com

Stephen Kelly
Graduate Student
Stanford University
1815 Latham St. Apt 15
Mountain View, CA 94041
415-369-1069
stkelly@stanford.edu

Fred Kemp
Consultant
RFA
10 Walloridge Rd
West Hartford, CT 6119
732-542-4000
kemp_fred_s@socglobal.net

Ray Kenard
President
American Wind Power & Hydrogen
445 park Avenue
New York, NY 10022
650-723-3463
rkenard@windpowerandhydrogen.com

Keith Kepler
President
Farasis Energy, Inc.
23575 Cabot Blvd, Ste 205
Hayward, CA 94002
800-523-8262
kkepler@farasis.com

APPENDIX A: ATTENDEE LIST

John Kerr
Program Manager
Lawrence Berkeley National Laboratory
MS 62R0203, Cyclotron Road
Berkeley, CA 94720
212-836-4705
jbkerr@lbl.gov

George Kervitsky
SENTECH
7475 Wisconsin Avenue, Suite 900
Bethesda, MD 20814
510-732-6600 x103
gkervitsky@aol.com

Edward Kiczek
Air Products,Inc
7201 Hamilton Blvd.
Allentown, PA 18195
510-486-6279
kiczekef@airproducts.com

Soowhan Kim
Senior Research Engineer
Hyundai Motor Company
104, Mabuk-Ri, Guseong-Eup
Yongin-Si, 449-912
240-223-5522
soowhan@hyundai-motor.com

Soonho Kim
Samsung Advanced Institute of Technology
P.O.Box 111
Suwon, 440-600
610 481-4705
kimsoonho@samsung.com

Hyoung-Juhn Kim
Korea Institute of Science and Technology
39-1 Hawolgok, Seongbuk
Seoul, 136-791
82-31-899-3203
hjkim25@kist.re.kr

Yu Seung Kim
Los Alamos National Laboratory
MST-11
Los Alamos, NM 87545
82-31-280-8155
yskim@lanl.gov

Merrill King
Pogram Executive
NASA Headquarters
300 E Street SW
Washington, DC 22181
82-2-958-5299
merrill.king@nasa.gov

Kim Kinoshita
K. Kinoshita Consulting
20644 Nancy Court
Cupertino, CA 95014
505-667-5782
linkim@att.net

Bruce Kinzey
Senior Research Engineer
Pacific Northwest National Laboratory
901 D St., S.W. Suite 900
Washington, DC 20024-2115
202-358-0817
Bruce.Kinzey@pnl.gov

Ella Kisilis
Manager, Strategy and Business Devel.
UTRC
411 Silver Lane,
East Hartford, CT 6108
408-390-8656
kisilied@utrc.utc.com

Carter Kittrell
CNL Lab Manager
Rice University
6100 Main Street - MS100
Houston, TX 77005
202-646-5231
kittrell@rice.edu

Leo Klawiter
Rohm and Haas Company
3100 State Road
Croydon, PA 19021
860-610-7816
iklawiter@rohmhaas.com

Lennie Klebanoff
Sandia National Laboratories
PO Box 969, MS 9409
Livermore, CA 94551
713-348-5108
lekleba@sandia.gov

Alfred Kleinhammes
UNC Chapel Hill
Department of Physics & Astronomy
Chapel Hill, NC 27599-3205
215-785-7424
kleinham@physics.unc.edu

Richard Klippstein
Contracts Manager
Air Products and Chemicals, Inc.
7201 Hamilton Blvd.
Allentown, PA 18195-1501
925-294-3471
klippsra@airproducts.com

Fumiaki Kobayashi
Toyota Motor North America
19001 S Western Ave.
Torrance, CA 90501
919-843-2407
fumiaki_kobayashi@tma.toyota.com

Shyam Kocha
Manager
Nissan Motor CO., Ltd.
1, Natsushima-cho
Yokosuka, 237-8523
610-481-8011
s-kocha@mail.nissan.co.jp

Dale Koelling
Program Manager
DOE - OBES
19901 Germantown Road
Germantown, MD 20874
310-468-6593
Dale.Koelling

Caroyln Koh
Colorado School of Mines
1600 Illinois St.
Golden, CO 80401
81-090-9971-3415
ckoh@mines.edu

John Kopasz
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439
301-903-2187
Kopasz@cmt.anl.gov

Ray Kopp
Manager, Production
FuelCell Energy, Inc.
3 Great Pasture Road
Danbury, CT 6813
303-273-3237
rkopp@fce.com

Refik Kortan
DOE-BES
SC-22/Germantown Building
1000 Independence Avenue, DC 20585-9513
630-252-7531
refik.kortan@science.doe.gov

Nikhil Kothurkar
Post Doc -Research Associate
U of Florida ~ Solar Energy and Energy Co
P.O. Box 116300
Gainesville, FL 32611
203-825-6019
nkoth@usl.edu

Ryan Kottenstette
Engineer
BMW Group Research and Technology
555 Hamilton Ave, Suite 300
Palo Alto, CA 94301
301 903 3308
ryan.kottenstette@bmw.de

Dennis Kountz
Senior Research Associate
DuPont Fuel Cells
DuPont Chestnut Run Plaza
Wilmington, DE 19880-0701
352-392-2328
dennis.j.kountz@usa.dupont.com

David Koyle
Professor
Wayne State University
5141 Provincial Drive
Bloomfield Hills, MI 48302
650-473-2020
dkoyle@aol.com

Robert Kramer
Director, Energy Efficiency and Reliability Center
Purdue University Calumet
2200 169th Street
Hammond, IN 46307
302-999-3354
kramerro@calumet.purdue.edu

Daniel Krassowski
Director of R&D - Natural Graphite
GrafTech International
12900 Snow Road
Parma, OH 44130
248-626-9012
dan.krassowski@graftech.com

Curt Krause
Chevron Technology Ventures
3901 Briarpark
Houston, TX 77042-5301
219-989-2147
CKrause@Chevron.com

Balaji Krishnamurthy
Chevron Technology Ventures
3901 Briarpark Dr
Houston, TX 77042
216-676-2657
balaji.k@chevron.com

Benjamin Kroposki
Sr. Engineer
National Renewable Energy Laboratory
1617 Cole Blvd.
Golden, CO 80401
713-954-6343
benjamin_kroposki@nrel.gov

Romesh Kumar
Head, Fuel Cell Department
Argonne National Laboratory
9700 S Cass Ave
Argonne, IL 60439-4837
713-954-6354
kumar@cmt.anl.gov

Harriet Kung
Division Director
DOE, Office of Basic Energy Sciences, Division of
19901 Germantown Rd.
Germantown, MD 20874
303-275-2979
harriet.kung@science.doe.gov

Nobuhiko Kuriyama
Group Leader of UBIQEN
AIST
1-8-31 Midorigaoka
Ikeda, 563-8577
630-252-4342
kuriyama-n@aist.go.jp

Quon Kwan
Program Manager
Federal Motor Carrier Safety Administration
400 Virginia AVe, SW., Suite 600
Washington, DC 20024
301-903-1330
quon.kwan@dot.gov

Jerri Laine
Technology Agency of Finland
3301 Massachusetts av
Washington, DC 20008
72-751-9651
jerri.laine@tekes.fi

Clint Lane
Research Professor
Northern Arizona University
Dept of Chemistry, PO Box 5698
Flagstaff, AZ 86011-5698
202 385 2389
Clint.Lane@nau.edu

Pete Langlois
Strategic Analyst
Ernst & Young
8484 Westpark Drive, Suite 200
McLean, VA 22102
202 413 3327
paxelpete@aol.com

John Larese
Professor
University of Tennessee/ORNL
Chemistry Dept./CSD
Knoxville, TN 37996
928-523-6296
jzl@utk.edu

Stephen Lasher
Principal
TIAX LLC
15 Acorn Park
Cambridge, MA 2140
703-747-1172
lasher.stephen@tiaxllc.com

Jay Laskin
President
Hyenergy Consulting, LLC
2210 Shefflin Ct.
Baltimore, MD 21209
865 9743429
Hyenergy@comcast.net

Othalene Lawrence
Environmental Scientist
Federal Government
1000 Independence Ave., SW
Washington, DC 20585
617-498-6108
Othalene.Lawrence@EE.doe.gov

Byung Gwon Lee
Director
Korea Insttitute of Science & Technology
P.O.Box 131, Cheongryang
Seoul, 130-650
443-854-0614
bglee@kist.re.kr

Paul Leiby
Sr. Scientist
Oak Ridge National Laboratory
P.O. Box 2008, MS 6036
Oak Ridge, TN 37831
202-586-8596
leibypn@ornl.gov

Paul Lemar Jr.
President
Resource Dynamics Corporation
8605 Westwood Center Drive, #410
Vienna, VA 22182
822-958-5857
pll@rdcnet.com

Johanna Levene
Chemical Process Engineer
National Renewable Energy Laboratory
1617 Cole Blvd, MS 1613
Golden, CO 80401
865-574-7720
johanna_levene@nrel.gov

Gregory Lewis
Research Associate
UOP LLC
50 E. Algonquin Rd
Des Plaines, IL 60017
703-356-1300 x204
gregory.lewis@uop.com

Michele Lewis
Chemist
Argonne National Laboratory
9700 S. Cass Ave.
Argonne, IL 60439
303-275-2922
lewism@cmt.anl.gov

Wen Li
Fuel Cell & Hydrogen Projects Leader
Toyota Technical Center, USA, Inc.
2350 Green Rd.
Ann Arbor, MI 48105
847-912-9237
wen.li@tema.toyota.com

Zhijiang (John) Li
Manager, Reformer and Membrane Development
Aspen Products Group
186 Cedar Hill Street
Marlborough, MA 1752
630-252-6603
zli@aspensystems.com

Cara Libby
EPRI Solutions, Inc.
1661 Page Mill Rd., Suite 175
Palo Alto, CA 94304
734-995-3165
clibby@eprisolutions.com

Robert Lieberman
President & CTO
Intellligent Optical Systems
2520 W. 237th Street
Torrance, CA 90505
508-481-5058
RLieberman@intopsys.com

Valri Lightner
U.S. Department of Energy
1000 Independence Ave., SW
Washington, DC 20585-0121
650-561-5319
valri.lightner@ee.doe.gov

Jerry Lin
Arizona State University
Dept. Chem. and Mater. Eng
Tempe, AZ 85260
310-530-7130
jerry.lin@asu.edu

Suzanne Linehan
Research Manager
Rohm and haas Company
60 Willow Street
North Andover, MA 1845
202-586-0937
slinehan@rohmhaas.com

Clovis Linkous
Senior Research Scientist
Florida Solar Energy Center
1679 Clearlake Road
Cocoa, FL 32922-5703
480-965-7769
calink@fsec.ucf.edu

Frank Liou
Professor/Director
University of Missouri-Rolla
307 ERL, 1870 Miner Circle
Rolla, MO 65409
978-689-1589
liou@umr.edu

Frank Lipiecki
Director Engineering Technical Center
Rohm and Haas Company
3100 State Road
Croydon, PA 19021
321-638-1447
flipiecki@rohmhaas.com

Ludwig Lipp
FuelCell Energy
3 Great Pasture Road
Danbury, CT 6813
573-341-4603
llipp@fce.com

William Liss
Executive Director
Gas Technology Institute
1700 S. Mount Prospect Road
Des Plaines, IL 60018
215-785-7040
william.liss@gastechnology.org

Morton Litt
Professor (Emeritus)
Case Western Reserve University
10900 Euclid Ave.
Cleveland, OH 44106-7202
203-205-2492
mhl2@case.edu

Simon (Zhong-Sheng) Liu
Group Leader- Modeling
National Research Council Canada
3250 East Mall
Vancouver, BC, V6T 1W5
847-768-0753
Simon.Liu@nrc.gc.ca

Ke Liu
Project Leader & Principal Investigator
GE Global Research Center
18A Mason
Irvine, CA 92618
216-368-4174
liuk@research.ge.com

Han Liu
Senior Project Scientist / Manager
Giner Electrochemical Systems
89 Rumford Ave.
Newton, MA 2466
604-221-3068
hliu@ginerinc.com

Jie Liu
Assocaite Professor
Duke University
Department of Chemistry
Durham, NC 27708
949-330-8977
j.liu@duke.edu

Paul Liu
Media and Process Technology Inc.
1155 William Pitt Way
Pittsburgh, PA 15068
781-529-0531
pliu@mediaandprocess.com

Yun Liu
National Institute of Standards and Technology
100 Bureau Drive, MS 8562
Gaithersburg, MD 20899
919-660-1549
yunliu@nist.gov

Jeffrey Long
University of California, Berkeley
Dept. of Chemistry
Berkeley, CA 94720-1460
412-826-3711
jrlong@berkeley.edu

Melissa Lott
Alliance Technical Services
1717 Pennsylvania Ave., NW, Ste. 650
Washington, DC 20006
301-975-6235
mjlott@alliance-technicalservices.com

Paul Lucchese
Hydrogen & fuel cell Director
CEA
Drt/Dpse Bat 446
Saclay, DC 91191
510-642-0860
paul.lucchese@cea.fr

William Lueckel
Consultant
Renewable Fuels Asociation
72 Crossroads Lane
Glastonbury, CT 6033
301-313-0429
lueckewc@alum.mit.edu

Weifang Luo
Sandia National Laboratories
7011 East Ave
Livermore, DC 94551
wluo@sandia.gov

Andy Lutz
Sandia National Laboratories
PO Box 969
Livermore, CA 94551-0969
860-659-1561
aelutz@sandia.gov

Rex Luzader
VP, Government Relations
Millennium Cell Inc.
One Industrial Way West
Eatontown, NJ 7724
925-294-3729
luzader@millenniumcell.com

Serguei Lvov
Professor
The Pennsylvania State University
207 Hosler
University Park, PA 16802
925-294-2761
lvov@psu.edu

Nicolas Lymberopoulos
Head, RES&H2 Technolies
Integration Section
19th km Marathonos Av.
Pikermi, GR-19009
732-542-4000
nlymber@cres.gr

David Lynch
Engineer
Startech Environmental Corp
190 Century Drive
Bristol, CT 6010
814-863-8377
dlynch@startech.net

Maxim Lyubovsky
H2Gen Innovations, Inc
4740 Eisenhower ave.
Alexandria, VA 22304
30-210-6603300
mlyubovs@h2gen.com

Alan MacDiarmid
Professor
University of Pennsylvania
231 South 34th Street
Philadelphia, PA 19104
860-582-6190
macdiarm@sas.upenn.edu

Digby Macdonald
Professor
Penn State
201 Steidle Bldg.
University Park, PA 16802
703-212-7444
ddm2@psu.edu

Arnulf Maeland
Visiting Senior Scientist
IFE (Inst. for Energy Technology)
Instituttveien
Kjeller, 2027
215-898-8307
arnulf@ife.no

Kathya Mahadevan
Research Scientist
Battelle
505 King Avenue
Columbus, OH 43201
814-863-7772
mahadevank@battelle.org

Eric Majzoub
Staff Scientist
Sandia National Laboratories
7011 East Ave
Livermore, CA 94550
561-790-5393
ehmajzo@sandia.gov

Thomas Maloney
Fueler Program Manager
Proton Energy Systems
10 Technology Drive
Wallingford, CT 6492
614-424-3197
tmaloney@protonenergy.com

Stanislav Malyshenko
Dr. Sci., head of laboratory
Institute for High Temperatures, RAS
Izhorskaya 13/19 IVTAN
Moscow, 125412
925-294-2498
litp@dataforce.net

Robert Mammarella
Fuji Photo Film, Inc.
921 Highway 246 S.
Greenwood, SC, SC 29649
203-678-2176
rmammarella@fujigreenwood.com

Pin-Ching Maness
Detailee
DOE Office of Basic Energy Sciences
19901 Germantown Road
Germantown, MD 20874
+7(495)3625311
pinching.maness@science.doe.gov

Amy Manheim
Technology Development Manager
HFCIT/EERE/DOE
8506 Beech Tree Court
Bethesda, MD 20817
864-223-2888 ext. 6146
amy.manheim@ee.doe.gov

Margaret Mann
National Renewable Energy Laboratory
1617 Cole Blvd
Golden, CO 80401
301-903-2153
margaret_mann@nrel.gov

Robert Mantz
Program Manager
Army Research Office
4300 South Miami Blve
Durham, NC 27704
202-586-1507
robert.a.mantz@us.army.mil

Samuel Mao
Lawrence Berkeley National Lab
1 cyclotron Rd
Berkeley, CA
303-275-2921
ssmao@lbl.gov

Jason Marcinkoski
U.S. Department of Energy
919-549-4309

Leonard Marianowski
Consultant
507 So Elmhurst Rd. Mt. Prospect, Il 60056
Mt. Prospect, IL 60056
510-486-7038
lmarian@comcast.net

Nenad Markovic
Senior scientist
Argonne National Laboratory
Materials Science Division
Argonne, IL 60439
nmmarkovic@anl.gov

Victor Maroni
Argonne National Laboratory
9700 South Cass Ave.
Argonne, IL 60439
847 338 0662
maroni@cmt.anl.gov

Andres Marquez
Post Doctoral Researcher
Ohio University
251 Stocker Center, Mech. Engr. Dpt.
Athens, OH 45701
630-252-5181
marquez@ohio.edu

David Martin
Membrane Reactor Technologies
1800-200 Granville Street
Vancouver, V6C 1S4
630-252-4547
dmartin@membranereactor.com

Michael Martin
Vice President Fuel Cell Technology
EMTEC
3155 Research Blvd., Suite 106
Dayton, OH 45420
740-593-4916
dely@emtec.org

Akiteru Maruta
Technova, Inc
1-1-1 Uchisaiwai-cho, Chiyoda-Ku
Tokyo, 100-0011
416.508.7247
maruta@technova.co.jp

David Masten
Staff Research Engineer
General Motors
10 Carriage St
Honeoye Falls, NY 14472-0603
937-259-1365
david.masten@gm.com

Shohei Matsuda
Senior Manager
Toshiba Fuel Cell SystemsCo.
4-1, Ukishima-Cho,Kawasaki-Ku
Kawasaki-Shi, Japan, 2100862
81-3-3508-2280
shohei.matsuda@toshiba.co.jp

Michael Matthews
Professor
University of South Carolina
Department of Chemical Engineering
Columbia, SC 29208
585-624-6606
matthews@engr.sc.edu

Joseph Mausar
Marketing and Sales Manager
Chemsultants
9349 Hamilton Drive
Mentor, OH 44060
44-288-8254
jmausar@chemsultants.com

Jimmy Mays
University of Tennessee
Department of Chemistry
Knoxville, TN 37996
803-777-0556
jimmymays@utk.edu

Robin Mazakas
Government Contractor
Air Products and Chemicals, Inc.
7201 Hamilton Blvd
Allentown, PA 18104
440-452-0218
mazakara@airproducts.com

Michael McCarthy
VP, Commercial Business Dev.
Protonex Technology Corporation
153 Northborough Road
Southborough, MA 01772-1034
865-974-0747
michael.mccarthy@protonex.com

Andrew McClaine
Chief Technology Officer
Safe Hydrogen, LLC
30 York St
Lexington, MA 02420-2009
610-481-3590
awmcclaine@rcn.com

Marc McConahy
Defense Logistics Agency
5157 Linette Lane
Annandale, VA 22003
508-490-9960 X226
mmcconahy@earthlink.net

Robert McConnell
Sr. Project leader
DOE (on assignment from NREL)
313Connecticut Ave. NV1
Washington, DC 20008
781 861-7016
bob.mconnell@ee.doe.gov

Jim McGetrick
Sr. Tech Ser Engineer
BP
150 W. Warrenville Road
Naperville, IL 60563
202-256-0552
mcgetrje@bp.com

James McGrath
University Distinguished Professor
Virginia Tech
2111 Hahn Hall, MC 0344
Blacksburg, VA 24061
303-589-2564
jmcgrath@vt.edu

Stewart McKenzie
Director Business Development
Columbian Chemicals
1800 West Oak commons Court
Marietta, GA 30062
630-420-4579
smckenzie@columbianchemicals.com

David McLean
Chief Operating officer
NextEnergy Center
461 Burroughs Ave.
Detroit, MI 48202
540-231-5976
davem@nextenergy.org

William Mcleod
Consultant
84 Settlers Farm Road
Monroe, CT 6468
770-792-9552
mmcleodwilliam@aol.com

Thomas McNicholas
Duke University
1123 Stacey Glen
Durham, NC 27705
313-833-0100 ext.110
tpm2@Duke.edu

Shawna McQueen
Energetics
7164 Gateway Drive
Columbia, MD 21046
203-268-2955
smcqueen@energetics.com

Jeffrey Mecham
Vice PResident of Research and Development
NanoSonic Inc.
1485 South Main Street
Blacksburg, VA 24060
610-246-0889
jmecham@nansonic.com

Stepeh Melancon
Senior Manager
Entergy Nuclear
1340 Echelon Parkway
Jackson, MS 39213
410-290-0370
smelanc@entergy.com

Margo Melendez
Sr. Project Leader
National Renewable Energy Laboratory
1617 Cole Blvd.
Golden, CO 80401
540-953-1785
margo_melendez@nrel.gov

Tasios Melis
Professor
Uc Berkeley
111 Koshland Hall
Berkeley, CA 94720-3102
601-368-5365
melis@nature.berkeley.edu

Jonathan Melman
Intematix Corporation
46410 Fremont Blvd.
Fremont, CA 94538
303-275-4479
jmelman@intematix.com

Michael Mercurio
President
Island Wind/AmericanWindHydrogen
39 W Indiana Ave
Beach Haven, NJ 8008
510-642-8166
island wind@aol.com

Juergen Mergel
Juleich Research Center
Wilhelm-Johnen-Straýe
Juelich, 52425
732-740-6426
j.mergel@fz-juelich.de

Jeremy Meyers
Manager, Adv Transportation Technology
UTC Fuel Cells
195 Governor's Hwy
South Windsor, CT 6074
732-740-6426
jeremy.meyers@UTCFuelCells.com

George Miley
Professor
Univ. of Illinois
103 S. Goodwin Ave.- 100 NEL
Urbana, IL 61821
49-2461-615996
ghmiley@uiuc.edu

Arnold Miller
President
Vehicle Projects LLC
621 17th Street, Suite 2131
Denver, CO 80293
860-727-2730
kristen.johnson@vehicleprojects.com

Lowell Miller
Director, Office of Sequestration, Hydrogen & Clea
U.S. Department of Energy
1000 Independence Avenue, S.W.
Washington, DC 20585-1290
217-3333772
lowell.miller@hq.doe.gov

Dan Miller
Board Member, Solazyme
918 Parker St.
Berkeley, CA 94710
303-296-4218
dan@rodagroup.com

Doyle Miller
Business Development
Intelligent Energy/MesoFuel
9019A Washington St. NE
Albuquerque, NM 87113
301-903-9451
doyle.miller@intelligent-energy.com

Eric Miller
University of Hawaii
1680 East-West Rd., POST 109
Honolulu, HI 96822
510-649-3505
ericm@hawaii.edu

James Miller
Argonne National Laboratory
9700 S. Cass Ave, Bldg 205
Argonne, IL 60439-4837
505-314-8145
millerj@cmt.anl.gov

Michael Miller
Staff Scientist
Southwest Research Institute
6220 Culebra Road
San Antonio, TX 78238-5166
808-956-5337
michael.miller@swri.org

Robert Miller
Sr. Contract Development Mgr.
Air Products and Chemicals
7201 Hamilton Blvd.
Allentown, PA 18195
630-252-6394
millerrn@airproducts.com

JoAnn Milliken
U.S. Department of Energy
210-522-2189

Michael Mills
Associate Director - IPHE Secretariat
U.S. DOE - IPHE
1000 Independence Ave., SW
Washington, DC 20585
610-481-4780
michael.mills@ee.doe.gov

Marianne Mintz
Transportation Energy Analyst
Argonne National Laboratory
9700 S Cass Ave
Argone, IL 60439
mmintz@anl.gov

Caetano Miranda
PhD / Post Doc Associate
DMSE - MIT
77 Mass Ave.
Cambridge, MA 2138
202-586-6653
cmiranda@mit.edu

Raul Miranda
PGM Ngr
U.S. Department of Energy
19901 Germantown rd
Germantown, MD 20841
630-252-5627

Cortney Mittelsteadt
Director, Energy
Giner Electrochemical Systems
89 Rumford Ave
Newton, MA 2466
617-324-2086
cmittelsteadt@ginerinc.com

Shimpei Miura
Manager
Toyota Motor Engineering and Manufacturing
1630 W.186th St
Gardena, CA 90248
shimpei.miura@tema.toyota.com

Kazutoshi Miwa
Senior researcher
Toyota Central R&D Labs., Inc.
Nagakute
Aichi, 480-1192
781-529-0529
miwa@cmp.tytlabs.co.jp

Chris Moen
Technical Manager
Sandia National Laboratories
7011 East Avenue
Livermore, CA 94550
310-787-5881
cmoen@sandia.gov

Nahid Mohajeri
Senior Research Engineer
Florida Solar Energy Center
1679 Clearlake Road
Cocoa, FL 32922-5703
561-63-4146
nmohajeri@fsec.ucf.edu

Satish Mohapatra
VP, Engineering
Advanced Fluid Tech. dba Dynalene Heat Transfer Fl
5250 West Coplay Road
Whitehall, PA 18052
925-294-3709
satishm@dynalene.com

Richard Mohring
Director - Technology Dev't
Millennium Cell
1 Industrial Way West
Eatontown, NJ 7724
321-638-1525
mohring@millenniumcell.com

Rana Mohtadi
Toyota Technical Center
2350 Green Rd
Ann Arbor, MI 48105
610-262-9686

Thomas Moore
VP - Liberty & Tech Affairs
DaimlerChrysler Retired
2401 Sullivan Road
Oxford, MI 48371
732-544-5706
thomassmoore40@aol.com

Graham Moore
Business Development Manager
Chevron Technology Ventures LLC
3901 Briarpark Dr
Houston, TX 77380
734-995-4012
graham.moore@chevron.com

Robert Moore
Professor - Principal Investigator
The University of Southern Mississippi
118 College Drive #10076
Hattiesburg, MS 39406
248-628-7628
rbmoore@usm.edu

Richard Moorer
Deputy Assistant Secretary for Tech. Development
DOE, Deputy Assistant Secretary Tech. Dev.
713-954-6323
richard.moorer@hq.doe.gov

Karren More
Oak Ridge National Laboratory
1 Bethel Valley Rd., Bldg. 4515
Oak Ridge, TN 37831-6064
601-266-4480
morekl1@ornl.gov

Gregory Moreland
Director of Government Relations
MTI MicroFuel Cells
431 New Karner Road
Albany, NY 12110
202-586-5350
gmoreland@mechtech.com

Marcina Moreno
Hydrogen Program Project Administrator / MHCoE
Sandia National Laboratories
P.O. Box 969, MS9052
Livermore, CA 94551-0969
865-574-7788
mamore@sandia.gov

Oscar Moreno
Senior Lead Scientist
Millennium Cell Inc.
One Industrial Way West
Eatontown, NJ 7724
518-533-2209
moreno@millenniumcell.com

Tetsuya Mori
Mgr/ Team Leader, Hydrogen Project
Tokyo Gas Co., Ltd.
1-5-20, Kaigan, Minato-ku,
Tokyo, 1900022
925-294-4733
moritetu@tokyo-gas.co.jp

Yu Morimoto
Toyota Central R&D Labs. Inc.
Nagakute
Aichi, 4801192
732-542-4000
morimoto@mosk.tytlabs.co.jp

Daniel Mosher
Project Leader
United Technologies Research Center
MS 129-21, 411 Silver Lane
East Hartford, CT 6108
81-354007597
mosherda@utrc.utc.com

Theodore Motyka
Savannah River National Laboratory
773-41A, 144
Aiken, SC 29808
81-561-63-4120
ted.motyka@srnl.doe.gov

David Mountz
Research Scientist
Arkema, Inc.
900 First Ave.
King of Prussia, PA 19406
860-610-7011
david.mountz@arkenagroup.com

Tim Mueller
Graduate Student
MIT
174 Morrison Ave. Apt. #2
Somerville, MA 2144
803-725-0772
tkm@mit.edu

Luis Muga
Professor
TOFTEC, Inc.
PO Box 15358
Gainesville, FL 32604
610-878-6418
muga@chem.ufl.edu

Rangachary Mukundan
Los Alamos National Laboratory
MS D429
Los Alamos, NM 87545
617-642-6476
mukundan@lanl.gov

Nazim Muradov
Principle Research Scientist
Florida Solar Energy Center
1679 Clearlake Road
Cocoa, FL 32922-5703
352-392-1441
Muradov@fsec.ucf.edu

Edward Mussi
Chief Program Engineer
Ricardo
40000 Ricardo Drive
Van Buren Twp, MI 48111
505-665-8523
ed.mussi@ricardo.com

Deborah Myers
Argonne National Laboratory
9700 S. Cass Avenue
Argonne, IL 60439
321-638-1448
myers@cmt.anl.gov

Balakrishnan Naduvalath
Associate Professor
Department of Chemistry, UNLV
4505 Maryland Parkway
Las Vegas, NV 89154
734-394-4163
naduvala@unlv.nevada.edu

Masatoshi Nagai
Professor
Tokyo University of Agriculture and Technology
Nakamachi 2-24-16
Koganei, Tokyo, 184-8588
630-252-4261
mnagai@cc.tuat.ac.jp

Kazuo Nagashima
Section Manager
Nissan Technical Center North America
18455 South Figueroa Street
Gardena, CA 90248
702-895-2907
kazuo.nagashima@nissan-usa.com

Barbara Nagle
SEPUP Director
Lawrence Hall of Science
University of California
Berkeley, CA 94720-5200
81-42-3887060
bnagle@berkeley.edu

Hiroshi Nakamura
Chief Engineer/Government and Industrial Affairs Of
Honda Motor Co., Ltd.
2-1-1 Minami-Aoyama, Minato-ku
Tokyo, 107-8556
310-771-3270
hiroshi_b_nakamura@hm.honda.co.jp

Gary Nakarado
Standards & Regulatory Policy
National Renewable Energy Laboratory
24657 Foothills Drive North
Golden, CO 80401
510-642-8718
Gary@Nakarado.Com

Sekharipuram Narayanan
Jet Propulsion Laboratory
4800 Oak Grove Drive, MS277-207
Pasadena, CA 91109
3-5412-1509
s.r.narayanan@jpl.nasa.gov

Robert Neilson
Department Manager
Idaho National Laboratory
P.O. Box 1625, MS 3830
Idaho Falls, ID 83415-3830
303-275-3719
Robert.Neilson@inl.gov

Tina Nenoff
Sandia National Labs
PO Box 5800, MS 0734
Albuquerque, NM 87185-0734
818-354 0013
tmnenof@sandia.gov

Dan Neumann
NIST Center for Neutron Research
100 Bureau Drive STOP 8562
Gaithersburg, MD 20899-8562
208-526-8274
dan@nist.gov

John Newman
Faculty Senior Scientist
Lawrence Berkeley National Laboratory
306 Gilman Hall, University of California,
Berkeley, CA 94720
505-844-0340
newman@newman.cchem.berkeley.edu

Dat Nguyen
VP Business Development
Hydra Fuel Cell Corporation
1800 169th Place Suite C700
Beaverton, OR 97006
301 975 5252
nguyenxdat@yahoo.com

Frank Novachek
Director, Corporate Planning
Xcel Energy
1099 18th Street, Suite 3000
Denver, CO 80201
510-642-4068
frank.novachek@xcelenergy.com

Joan Ogden
Professor
UC Davis
2305 Goldberry Lane
Davis, CA 95616
408-836-6861
jmogden@ucdavis.edu

Jim Ohi
National Renewable Energy Laboratory
1617 Cole Boulevard
Golden, CO 80401
303-308-6010
jim_ohi@nrel.gov

Michael O'Keefe
DOE/BES
19901 Germantown Road
Germantown, MD 20876
530-752-2768
michael.o'keefe@science.doe.gov

Sarah Olexsak
Member Services Specialist
USFCC
1100 H St NW, Suite 800
Washington, DC 20005
303-275-3706
sarah@usfcc.com

Gregory Olson
HRL Laboratories, LLC
3011 Malibu Canyon Road
Malibu, CA 90265
301-903-0340
olson@hrl.com

Susanne Opalka
Principal Scientist
United Technologies Research Center
411 Silver Lane
East Hartford, CT 6108
202-293-5500
opalkasm@utrc.utc.com

Grace Ordaz
U.S. Department of Energy
1000 Independence Ave
Washington dc, DC 20585
310-317-5457
grace.ordaz@ee.doe.gov

Jeffrey Ortega
Lead Scientist
Millennium Cell Inc.
One Industrial Way West
Eatontown, NJ 7724
860-610-7195
jortega@millenniumcell.com

KOJI OSHITA
Project Manager
Toyota Motor Engineering & Manufacturing N.A.
25 Atlantic Ave
Erlanger, KY 41018
202-586-8350
koji.oshita@tema.toyota.com

Ben Ovshinsky
ECD-Ovonics
589 Arlington Ave.
Berkeley, CA 94707
732-542-4000
benovshin@msn.com

Umit Ozkan
Professor
Ohio State University
Chemical Engineering, 140 W 19th Avenue
Columbus, OH 43210
859-372-2630
ozkan.1@osu.edu

Vidvuds Ozolins
Associate Professor
University of California, Los Angeles
Boelter Hall 6532F
Los Angeles, CA 90025-1595
510-525-8236
vidvuds@ucla.edu

Catherine Padro
Project Leader, Hydrogen Systems
Los Alamos National Laboratory
P.O. Box 1663
Los Alamos, NM 87545
614-292-6623
padro@lanl.gov

Dionissios Papadias
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439
310-267-5538
papadias@cmt.anl.gov

Urvi Parekh
Engineer
SENTECH, Inc.
7475 Wisconsin Ave, Suite 900
Bethesda, MD 20814
505-667-5539
uparekh@sentech.org

George Parks
Research Fellow
ConocoPhillips
344-A PL BTC
Bartlesville, OK 74004
630-252-3206
george.d.parks@conocophillips.com

Keith Parks
National Renewable Energy Laboratory
1617 Cole Blvd
Golden, CO 80401
202-586-4470
keith_parks@nrel.gov

Vencisclav Parvanov
Pacific Northwest National Laboratory
PO Box 999, K1-83
Richland, WA 99352
918-661-7780
Donna.Sturdevant@pnl.gov

Ugur Pasaogullari
Assistant Professor
University of Connecticut/CT Global Fuel Cell Cent
44 Weaver Rd Unit 5233
Storrs, CT 6269
303-384-7341
ugurpasa@engr.uconn.edu

Mark Paster
Technology development Manager
U.S. Department of Energy
1000 Independence Ave.
Washington, DC 20585
509-372-6151
mark.paster@ee,doe.gov

Pinakin Patel
Director, Sp Systems and Research
FuelCell Energy Inc
3 Great Pasture Rd
Danbury, CT 6813
860-486-9441
ppatel@fce.com

Nag Patibandla
Director, Center for Future Energy Systems
Rensselaer Polytechnic Institute
CII 8015 110 8th Street
Troy, NY 12180
292-586-2821
patibn@rpi.edu

Phil Patterson
U.S. Department of Energy
EE-3b, 1000 Independence Ave SW
Washington, DC 20585
203-825-6072
philip.patterson@ee.doe.gov

Dilo Paul
senior scientist
Science Applications Int'l Corp
626 Cochran Mill road
Pittsburgh, PA 15236
518-276-3784
paul@sa.netl.doe.gov

Vitalij Pecharsky
Professor
DOE Ames Laboratory and Iowa State U.
253 Spedding
Ames, IA 50011-3020
202-586-9121
vitkp@ameslab.gov

Julie Perez
Project Engineer
Directed Technologies, Inc.
3601 Wilson Blvd. Suite 650
Arlington, VA 22201
412-386-6110
julie_perez@directedtechnologies.com

Michael Pero
Director - Risk Management
Hydrogen Safety, LLC
66 Cedar Street, Suite 301
Newington, CT 06111-2646
515-294-8220
m.pero@hydrogensafety.com

Jerome Perrin
R&D Program Director
Air Liquide
CRCD, BP 126
Les Loges en Josas, 78354
703-778-7115
jerome-m.perrin@airliquide.com

Mike Perry
Product Manager
UTC Power
195 Governor's Highway
South Windsor, CT 6074
860-594-7183
michael.perry@utcpower.com

John Peters
Professor
Montana State University
108 Gaines Hall
Bozeman, MT 59717
(3313) 907-6429
john.peters@chemistry.montana.edu

David Peterson
DOE - Golden Office
1617 Cole Blvd.
Golden, CO 80401
860-727-2067
david.peterson@go.doe.gov

Mark Petri
Nuclear Hydrogen Program Director
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439
406-994
mcpetri@anl.gov

John Petrovic
Los Alamos National Laboratory
Fellow (retired)
Petrovic and Associates
8954 Puerto Del Rio Drive, #402
Cape Canaveral, FL 32920
303-275-4956
johnpetrovic@earthlink.net

Guido Pez
Chief Scientist
Air Products and Chemicals, Inc.
7201 Hamilton Blvd.
Allentown, PA 18195
630-252-3719
pezgp@airproducts.com

Peter Pfeifer
Professor
University of Missouri
Dept. of Physics
Columbia, MO 65211
321-392-3348
pfeiferp@missouri.edu

George Philippidis
Associate Director, Fiu
10555 W. Flagler Street
Miami, FL 33174
610-481-4271
George.Philippidis@arc.fiu.edu

Paul Pickard
Manager
Sandia National Laboratories
P. O. Box 5800
Albuquerque, NM 87105-1136
573-882-2335
pspicka@sandia.gov

Michael Pien
ElectroChem, Inc.
400 W Cummings park
Woburn, MA 1801
305-348-6628
mpien@fuelcell.com

Joseph Pierre
Manager, Government Programs
Siemens Power Generation,
Stationary Fuel Cells
1310 Beulah Road
Pittsburgh, PA 15235
505-845-3046
joseph.pierre@siemens.com

Peter Pintauro
Professor
Case Western Reserve University
Department of Chemical Engineering
Cleveland, OH 44106-7217
781-938-5300
pnp3@case.edu

William Pitts
Research Chemist
NIST
MS 8662
Gaithersburg, MD 20899-8662
412-256-5313
wpitts@nist.gov

Bryan Pivovar
Fuel Cell Team Leader
Los Alamos National Laboratory
PO Box 1663, MS D429
Los Alamos, NM 87545
216-368-4150
pivovar@lanl.gov

Marylynn Placet
Technical Group Manager
Pacific Northwest National
Laboratory
901 D St SW #900
Washington, DC 20024
301-975-6486
m.placet@pnl.gov

Sean Plasynski
Sequestration Technology Manager
DOE/NETL
P.O. Box 10940
Pittsburgh, PA 15236-0940
505-665-8918
sean.plasynski@netl.doe.gov

Walter Podolski
Argonne National Laboratory
9700 S. Cass Avenue
Argonne, IL 60439
202-646-5249
podolski@cmt.anl.gov

Joe Poindexter
Manager, Commercial Products
10707 Gilroy Road
Hunt Valley, MD 21030
412-386-4867
joe.poindexter@teledynees.com

Terence Poles
Director, Fuel Cells
Engelhard Corporation
101 Wood Avenue
Iselin, NJ 8830
630-252-7558
terry.poles@engelhard.com

Guillermo Pont
Sr. Project Engineer
Honeywell International
2525 W. 190th Street
Torrance, CA 90504
410-891-2281
guillermo.pont@yhoneywell.com

Arthur Pontau
Senior Manager
Sandia National Laboratories
7011 East Avenue, MS9161
Livermore, CA 94550
732 205 6633
aeponta@sandia.gov

Branko Popov
Professor
Department of Chemical Engineering,
University of South Carolina
301 Main Street
Columbia, SC 29208
310-512-2483
popov@engr.sc.edu

Zdenek Pors
Nuclear Research Institute Rez plc
Husinec - Rez 130
Rez, CZ 25068
925-294-3159
porsch@seznam.cz

Lisa Powell
Research Fellow
LMI
2000 Corporate Ridge
McLean, VA 22102
803-777-7314
lpowell@lmi.org

Philip Power
Professor
UC Davis, Chem. Dept.
Davis, CA 95616
(42022) 252-0916
pppower@ucdavis.edu

Frank Preli
Vice President Engineering
UTC Power
195 Governors Highway
South Windsor, CT 6074
703-917-7126
Francis.Preli@utcpower.com

Robert Privette
Director, US Fuel Cell Development
Umicore
2347 Commercial Drive
Auburn Hills, MI 48326
530-752-6913
rmpagain@yahoo.com

Graham Pugh
Exec Director, IPHE
U.S. Department of Energy
860-727-2342
graham.pugh@ee.doe.gov

Alex Puretzky
Senior Research Staff
Oak Ridge National Laboratory
1Bethel Valley Road ORNL, CMSD, BLDG3150
Oak Ridge, TN 37831
248-292-2257
1ap@ornl.gov

Zhigang Qi
Fellow
Plug Power Inc.
968 Albany Shaker Road
Latham, NY 12110
202-586-8029
zhigang_qi@plugpower.com

Cheng-Guan Quah
VP - CTO/Exec Director
NextEnergy/CTC
22040 Abington Drive
Farmington Hills, MI 48335
865-241-8294
cgmquah@yahoo.com

Damodaran Raghu
Shell Hydrogen/Shell Global Solutions
3333, Hwy-6 South
HOUSTON, TX, TX 77082
518-738-0229
d.raghu@shell.com

Venki Raman
President
Protium Energy Technologies
4804 Parkview Dr. South
Emmaus, PA 18049
248-477-2930
venki.raman@protiumenergy.com

Shwetha Ramkumar
The Ohio State University
140 W. 19th Ave, 125A Koffolt Labs
Columbus, OH 43210
281-544-7231
ramkumar.3@osu.edu

Kristen Rannels
Sentech, Inc.
7475 Wisconsin Ave, Suite 900
Bethesda, MD 20814
610-965-4589
krannels@sentech.org

Udaya Rao
Acting Division Director
DOE/NETL
626 Cochrans Mill Road
Pittsburgh, PA 15236
614-688-4912
rao@netl.doe.gov

Scot Rassat
Pacific Northwest National Laboratory
P.O. Box 999, M/S K6-28
Richland, WA 99352
202-586-1145
sd.rassat@pnl.gov

Dan Rastler
Technical Leader/Distributed Energy Resources
Electric Power Research Institute (EPRI)
3420 Hillview Avenue
Palo Alto, CA 94304
412-386-4743
drastler@epri.com

Carole Read
U.S. Department of Energy
1000 Independence Ave SW EE2H
Washington, DC 20585
509-376-1410
carole.read@ee.doe.gov

James Reilly
Guest Scientist
Brookhaven Nat'l Lab.
Bldg. 815
Upton, NY 11793-5000
650-855-2521
jreillys@bnl.gov

Jessica Reinkingh
Johnson Matthey Fuel Cells Inc.
1397 King Road
West Chester, PA 19380
202-586-3152
reinkj@jmusa.com

Robert Remick
Director, Colorado Fuel Cell Center
1310 Maple St.
Golden, CO 80401
631-344-4502
rremick@mines.edu

Jaime Reyes
Lockheed Martin
230 Mall Blvd.
King Of Prussia, PA 19406
610-232-1908
jaime.m.reyes@lmco.com

Richard Ricker
Metallurgist
NIST
100 Bureau Dr. MS 8553
Gaithersburg, MD 20899
303-273-3742
richard.ricker@nist.gov

Blake Ridgeway
VP, Intelligent Energy/MesoFuel
9019A Washington St NE
Albuquerque, NM 87113
610-354-1553
blake.ridgeway@intelligent-energy.com

Matt Ringer
Sr. Engineer
National Renewable Energy Laboratory
1617 Cole Boulevard, MS 3512
Golden, CO 80401
301-975-6023
matthew_ringer@nrel.gov

Michael Rinker
Relationship Manager, Hydrogen
Battelle, Pacific Northwest National Laboratory
902 Battelle Blvd, K5-22
Richland, WA 99354
505-314-8145
mike.rinker@pnl.gov

James Ritter
Professor
University of South Carolina
Department of Chemical Engineering
Columbia, SC 29208
303-384-7747
ritter@engr.sc.edu

Vernon Roan
Professor Emeritus
Univ of FL, ANL
5915 NW 19th Place
Gainesville, FL 32605
509-375-6623
vproan@juno.com

John Robbins
Portfolio Manager
ExxonMobil Research and Engineering
1545 Route 22 East
Annandale, NJ 8559
803-777-3590
john.l.robbins@exxonmobil.com

Ian Robertson
Head and Professor
University of Illinois
1304 W. Green St.
Urbana, IL 61801
352-378-8573
kdysart@uiuc.edu

Richard Rocheleau
Director
Hawaii Natural Energy Institute, U of Hawaii
1680 East-West Road, POST 109
Honolulu, HI 96822
908-730-3237
hnei@hawaii.edu

Tommy Rockward
Los Alamos National Laboratory
P.O. Box 1663
Los Alamos,, NM 87545
217-333-1440
trock@lanl.gov

Daniel Rodriguez
Program Manager
Plug Power
968 Albany Shaker Road
Latham, NY 12110
808-956-8890
dan_rodriguez@plugpower.com

Mark Roelofs
Sr. Research Associate
DuPont Company
POB 80262
Wilmington, DE 19880-0262
505-667-9587
mark.g.roelofs@usa.dupont.com

Jerry Rogers
FreedomCAR Triad
GM R&D Center
MC 480-106-269 30500 Mound Rd
Warren, MI 48090-9055
518-782-7700
jerry.rogers@gm.com

Bumwook Roh
Researcher
Hyundai Motor Com
Mabuk-dong
Yongih-si,
302-695-2503
bwroh@hyundai-motor.com

Ewa Ronnebro
Sandia National Laboratories
7011 East Ave
Livermore, CA 94551
586-986-1607
ecronne@sandia.gov

Robert Rose
Executive Director
US Fuel Cell Council
1100 H Street, NW, Suite 800
Washington, DC 20005
brose@fuelcells.org

Philip Ross
Senior Scientist
Lawrence Berkeley Laboratory
1 Cyclotron Rd
Berkeley, CA 94720
925-294-6493
pnross@lbl.gov

Rhett Ross
President
Teledyne Energy Systems
10707 Gilroy Road
Hunt Valley, MD 21031
202-785-4222
rhettross@teledyne.com

Laura Rovetto
Colorado School of Mines
1600 Illinois St.
Golden, CO 80401
510-486-6226
lrovetto@mines.edu

Leon Rubinstein
Technology Advisor
Shell Hydrogen
700 Milam St
Houston, TX 77002
410-891-3387
leon.rubinstein@shell.com

Antonio Ruiz
U.S. Department of Energy
1000 Independence Ave., SW
Washington, DC 20585
303-273-3561
antonio.ruiz@ee.doe.gov

John Rush
Adjunct Professor, NIST Senior Fellow(emeritus)
U.MD,NIST(retired)
NIST
Gaithersburg,MD., MD 20899
713 546 8341
john.rush@nist.gov

Mark Ruth
Macro-System Model Engineer
Systems Integration at NREL
1617 Cole Blvd.
Golden, CO 80401
202-586-0729
mark_ruth@nrel.gov

Chuck Ryan
Vice President, Technology
NCMS
3025 Boardwalk
Ann Arbor, MI 48108
301-926-0657
chuckr@ncms.org

Ichiro Sakai
Assistant Vice President
American Honda Motor
1001 G Street, NW, Suite 950
Washington, DC 20001
303-384-6874
ichiro_sakai@ahm.honda.com

Ryogo Sakamoto
Honda R&D Co.,Ltd.
1-4-1 Chuo, Wako-city, Saitama, Japan 351-0193
734-995-4905
ryogo_sakamoto@n.f.rd.honda.co.jp

Chris Sam
Advanced Engineering Associate
Exxon-Mobil FCFF CSTT Member
3225 Gallows Road, Room 3A-1910
Fairfax, VA 22037
202-661-4400
chris.r.sam@exxonmobil.com

Anthony San Pietro
Consultant to NREL Biohydrogen Program
NREL / Indiana University
1621 Pickwick Place
Bloomington, IN 47401 - 6143
81-48-462-5342
sanpietr@indiana.edu

Robert Sandoli
Program Examiner
OMB
725 17th Street NW
Washington, DC 20503
703-846-2574
rsandoli@omb.eop.gov

Gary Sandrock
Consultant to DOE/SNL
810 Hacienda Ave.
Davis, CA 95616
812-332-4650
gary.sandrock@sbcglobal.net

Suryanarayan Sankar
President
Advanced Materials Corporation
850 Poplar Street
Pittsburgh, PA 15241
202-395-4573
sankar@advanced-material.com

Masato Sato
Senior Manager
GM Asia Pacific (Japan)
Shinagawa Seaside East Tower 8th Floor, 4-12-8, Hi
Tokyo, 140-8687
530-753-0451
masato.sato@gm.com

Sunita Satyapal
Hydrogen Storage Team Leader
U.S. Department of Energy
1000 Independence Ave, SW
Washington, DC 20585
412-921-9600 X2223
sunita.satyapal@ee.doe.gov

Vesna Scepanovic
Program Manager
Natural Resources Canad
568 Booth Street
Ottawa, K1A 0E4
81-3-6711-5708
vscepan@nrcan.gc.ca

Greg Schenter
Pacific Northwest National Laboratory
PO Box 999, K1-83
Richland, WA 99352
202-586-2336
Donna.Sturdevant@pnl.gov

David Schiraldi
Associate Professor
Case Western Reserve University
Dept of Macromolecular Sci & Eng
Cleveland, OH 44106-7202
613-996-6228
das44@cwru.edu

STEVEN SCHLASNER
ConocoPhillips
346 PL
Bartlesville, OK 74004
509-372-6151
steven.m.schlasner@conocophillips.com

Edward Schmetz
Leonardo Technologies, Inc.
9801 Dellcastle Road
Montgomery Village, MD 20886
216-368-4243
ebschmetz@msn.com

Peter Schmidt
Senior Engineer
Directed Technologies Inc.
3601 Wilson Blvd Ste 650
Arlington, VA 22201
918-661-0647
pschmidt@directedtechnologies.com

Eileen Schmura
Sr Engineer
Concurrent Technologies Incorporated
425 Sixth Avenue
Pittsburgh, PA 15219
301-977-3581
schmurae@ctc.com

Mark Schneider
Engineering Manager
Delaware County Electric Coop.
39 Elm St.
Delhi, NY 13753
703-778-7125
mark.schneider@dce.coop

Jesse Schneider
DaimlerChrysler
800 Chrysler Drive
Auburn Hills, MI 48326
412-992-5367
Jesse.Schneider@DaimlerChrysler.com

Susan Schoenung
President, Longitude 122 West, Inc.
1010 Doyle Street, Suite 10
Menlo Park, CA 94025
607-746-9297
schoenung@aol.com

Raymond Schubert
Senior Technologist
TIAX, LLC
1601 S. De Anza Blvd.
Cupertino, CA 95014
248-576-3324
schubert.raymond@tiaxllc.com

JOANN SCHWARTZ
Dupont
Dupont Experimental Station
Wilmington, DE 19808
650-329-0845
Jo-Ann.T.Schwartz-1@usa.dupont.com

Rick Schwerdtfeger
Senior Project Scientist, Fuel Cell Technology
EMTEC
3155 Research Blvd., Suite 106
Dayton, OH 45420
408-517-1561
dely@emtec.org

Craig Scott
Fuel Cell Program Manager
Toyota
19001 S Western Ave, A402
Torrance, CA 90501
302 695-1923
craig_scott@toyota.com

Matthew Seabaugh
NexTech Materials, Ltd.
404 Enterprise Drive
Lewis Center, OH 43035
937-259-1365
seabaugh@nextechmaterials.com

Rosalind Sell
Commercial Program Manager
General Motors
30500 Mound Rd, MC 480106241
Warren, MI 48090
310-468-2634
roz.sell@gm.com

Jeffrey Serfass
President
National Hydrogen Association
1800 M St, NW, Suite 300 North
Washington, DC 20036
614-842-6606
serfassj@hydrogenassociation.org

Patrick Serfass
Director, Program and Technology Development
National Hydrogen Association
1800 M Street, NW Suite 300-North
Washington, DC 20036
586-986-7676
serfassp@hydrogenassociation.org

Norbert Seyr
Liaison Engineer for Research and Technology
BMW Group
P.O. Box 11000
Spartanburg, SC 29304-4100
202-223-5547 x312
Norbert.Seyr@bmw.de

Evgeny Shafirovich
Purdue University
Chemical Engineering
West Lafayette, IN 47907-2100
202-370-6325
eshafir@purdue.edu

Naresh Shah
Research Associate Professor
University of Kentucky
533 South Limesone Street, Suite 107
Lexington, KY 40508-4005
864-989-5934
naresh@Uky.edu

Leon Shaw
University of Connecticut
97 North Eagleville Road U-3136
Storrs, CT 6269
765-496-2969
leon.shaw@uconn.edu

Paul Sheaffer
Director, Energy Technology
Resource Dynamics Corp
8605 Westwood Center Dr.
Vienna, VA 22182
859-257-5119
sheaffer@rdcnet.com

Thomas Sheahen
system integration analyst
National Renewable Energy Laboratory
901 D st SW suite 930
Washington, DC 20024
860-486-2592
tom_sheahen@nrel.gov

John Shen
U.S. Department of Energy
FE-24
Germantown, MD 20874
703-356-1300 x215
john.shen@hq.doe.gov

James Sherman
Vice-President
American Wind Power & Hydrogen
92 Park Street
Montclair, NJ 7042
202-646-5216
Dynequ@comcast.net

Steven Sherman
Idaho National Laboratory
P.O. Box 1625
Idaho Falls, ID 83415-3865
301-903-4344
Steven.Sherman@inl.gov

James Sherman
Vice-President
American Wind Power & Hydrogen
445 Park Ave
New York, NY 10022
973-783-0284
Dynegy@comcast.net

John Shewchun
DTE Energy/Wayne State University
4394 Charing Way
Bloomfield Hills, MI 48304
208-526-6582
shewchun@@wayne.edu

Yuichiro Shimura
Senior Consultant
Mitsubishi Research Institute, Inc.
3-6, Otemachi 2-Chome Chiyoda-ku
Tokyo, 100-8141
973-783-0284
yuichiro@mri.co.jp

Hwansoo Shin
Hyundai motor company
104, Mabuk-Dong, Giheung-Gu,
Yongin-Si,
Gyunggi-Do, 446-912
248-647-4506
hsshin74@hyundai-motor.com

Hideo Shindo
Chief Representative
NEDO DC
2000 L Street NW, Suite 605
Washington, DC 20036
81-3-3277-4519
shindo@nedodc.org

Lawrence Shore
Senior Research Associate
Engelhard Corporation
101 Wood Avenue
Iselin, NJ 8830
82-31-899-3199
larry.shore@engelhard.com

John Siciliano
Assoc. Editor
Inside Washington Publishers
1225 S Craig St.
Arlington, VA 22202
202-822-9298
jciciliano@iwprews.com

Robert Sievers
Director of Government Programs and Regulatory Aff
MTI Micro Fuel Cells
431 New Karner Rd
Albany, NY 12205
732-205-5447
rsievers@mechtech.com

Jack Simon
Office of Vice Pres. of Research
Savannah River National Laboratory
227 Gateway Dr.
Aiken, SC 29803
703-416-8576
jack.simon@srnl.doe.gov

Lin Simpson
National Renewable Energy Laboratory
16717 Cole Blvd
Golden, CO 80401
518-533-2238
lin_simpson@nrel.gov

Carl Sink
Program Manager, DOE, NHI
1000 Independence Ave., SW
Washington, DC 20585-1290
803-819-8420
carl.sink@nuclear.energy.gov

David Sjoding
WSU Extension Energy Program
925 Plum St SE Bldg #4
Olympia, WA 98504-3165
303-384-6625
SjodingD@energy.wsu.edu

Edward Skolnik
Program Manager/Senior Scientist
Energetics Incorporated
901 D St. SW (Suite 100)
Washington, DC 20024
301-903-5131
eskolnik@energetics.com

Darlene Slattery
Sr. Research Chemist
Florida Solar Energy Ctr.-UCF
1679 Clearlake Rd
Cocoa, FL 32922
360-956-2004
dkslatt@fsec.ucf.edu

Panagiotis Smirniotis
Professor
University of Cincinnati
497 Rhodeo Hall
Cincinnati, OH 45221
202-406-4125

Matthew Smith
Assistant Director
U of South Florida's Clean Energy Research
4202 e fowler ave ENB118
Tampa, FL 33620
321-638-1449
masmith@eng.usf.edu

Ashley Smith
SENTECH, Inc.
700 S. Illinois Ave. Ste. 201-A
Oak Ridge, TN 37830
513-556-1474
asmith@sentech.org

Sean Smith
Principal Scientist
Osram Sylvania
Hawes Street
Towanda, PA 18848
813-974-4787
sean.smith@sylvania.com

Bradley Smith
Shell Hydrogen
700 Milam PNT 11076
Houston, TX 77002
865-483-0359 x103
brat.smith@shell.com

Eugene Smotkin
CEO
NuVant Systems, Inc.
9800 Connecticut Drive
Crown Point, IN 46307
570-268-5355
esmotkin@nuvant.com

Larry Sneddon
Professor
University of Pennsylvania
231 South 34th Street
Philadelphia, PA 19104-6323
713-546-8328
lsneddon@sas.upenn.edu

Petros Sofronis
Professor
University of Illinois at Urbana-Champaign
216 Talbot Laboratory, 104 South Wright Street
Urbana, IL 61801
787-723-7334
sofronis@uiuc.edu

Hong Yong Sohn
Professor
University of Utah
135 S 1460E Rm. 412
Salt Lake City, UT 84112
215-898-8632
hysohn@mimes.utah.edu

Jennifer Sollars
U.S. Department of Energy
217-333 2636

Lee Spangler
Director of Special Projects
Montana State University
207 Montana Hall
Bozeman, MT 59717
801-581-5491
spangler@montana.edu

Mark Spitler
Detaillee
Office of Science
19901 Germantown Road
Germantown, MD 20874
mark.spitler@science.doe.gov

Mary Spruill
Program Director
The NEED Project
8408 Kao Circle
Manassas, VA 20110
406-994-2891
mspruill@need.org

Sesha Srinivasan
Research Scientist
University of South Florida
4202 E Fowler Ave
Tampa, FL 33620
301 903 4568
sesha@eng.usf.edu

Gary Staffo
Safety & Health Manager
US DOE EE-3C
1000 Independence Ave SW
Washington, DC 20585
703-257-1117
gary.saffo@ee-doe.gov

Douglas Stalhein
President
DGS Metallurgical Solutins
16110 NE 4th Street
Wancouver, WA 98684
813-974-0759
dgstalhein@comcast.net

Rhonda Staudt
Manager, High Temperauture PEM Program
Plug Power Inc.
968 Albany-Shaker Road
Latham, NY 12110
202-586-9577
rhonda_staudt@plugpower.com

Elias Stefanakos
Director
U of South Florida's Clean Energy Research
4202 East Fowler Avenue, ENB 118
Tampa, FL 33620-5350
360-713-2407
stefanak@eng.usf.edu

Detlef Stolten
Prof. Dr.
Research Center Juelich
IWV-3
Juelich, 52425
518-782-7700 ext 1215
d.stolten@fz-juelichh.de

Harry Stone
Project Manager
Battelle
10300 Alliance Road, Suite 155
Cincinnati, OH 45242
813-974-7322
castlek@battelle.org

Peter Strasser
Professor
University of Houston
4800 Call…. Rd Ey B 1 S222
Department of chemical energy
Houston, TX 71098
pstrasser@uh.edu

Tim Strobel
Colorado School of Mines
1600 Illinois St
Golden, CO 80401
513-362-2600
tstrobel@mines.edu

Ken Stroh
Group Leader / Program Manager
Los Alamos National Laboratory
Mail Stop D429
Los Alamos, NM 87545
713-743-4310
stroh@lanl.gov

Viktor Struzhkin
Carnegie Institution of Washington
5251 Broad Branch Rd., N.W.
Washington, DC 20015
303-273-3561
struzhkin@gl.ciw.edu

Athanasios Stubos
Research Director
NCSR Demokritos
Aghia Paraskevi Attikis
Athens, 15310
505-667-7933
stubos@ipta.demokritos.gr

Roland Stumpf
Sandia National Laboratories
5880 Welch Ln
Livermore, CA 94550
202-478-8952
rrstump@sandia.gov

William Summers
Program Manager, Energy Security
Savannah River National Laboratory
SRS Building 773-42A
Aiken, SC 29808
210-650-3447
william.summers@srnl.doe.gov

Xiaolei Sun
Senior Principal Scientist
Millennium Cell Inc.
One Industrial Way West
Eatontown, NJ 7724
925-294-6114
sun@millenniumcell.com

Darija Susac
University of British Columbia
2036 Main Mall
Vancouver, V6T 1Z1
803-725-7766
daria@chem.ubc.ca

Peter Sutter
Brookhaven National Laboratory,
Center for Functio
Bldg 555
Upton, NY 11973-5000
732-542-4000
psutter@bnl.gov

George Sverdrup
Technology Manager
National Renewable Energy
Laboratory
1617 Cole Blvd
Golden, CO 80401
604-822-4540
george_sverdrup@nrel.gov

Karen Swider-Lyons
Naval Research Laboratory
4555 Overlook Ave SW, Code 6113
Washington, DC 20375
631-344-3109
karen.lyons@nrl.navy.mil

Tetsuhisa Tada
Assistant Manager
COSMO OIL CO., LTD
1-1 Shibaura 1-Chome, Minato-ku
Tokyo, 105-8528
303-275-4433
tetsuhisa_tada@cosmo-oil.co.jp

Masaki Tajima
Chief Manager Hydrogen Bussiness
Section
Tokyo Gas Co. Ltd.
5-20, Kaigan 1-Chome, Minato-Ku,
Tokyo, Japan
Tokyo, 105-8527
202-404-3314
tajima@tokyo-gas.co.jp

Osamu Tajima
SANYO Electric.Co., Ltd.
1-1-1 Sakata Oizumi-Machi
Ora-gun,Gunma, 370-0596
3-3798-3128
tosamu.tajima@sanyo.co.jp

Hirofumi Takami
SANYO Electric Co.,Ltd
1-1-1, Sakata, Oizumi-Machi, Ora-
Gun
Gunma, 370-0596
81-3-5400-7597
hirofumi.takami@sanyo.co.jp

Satish Tamhankar
The BOC Group, Inc.
100 Mountain Avenue
New Privdence, NJ 7974
276-61-9259
satish.tamhankar@boc.com

Yoshitaka Tamo
Chief Officer
NEDO
20F Muza Kawasaki Building, 1310,
Omiya-cho, Saiwa
Kawasaki-shi, Kanagawa-ken, 212-
8554
276-61-8131
tamouyst@nedo.go.jp

Hiroshi TANAKA
Analyst
Mitsubishi Research Institute,Inc.
Otemachi 2-3-6
TOKYO, 213-0001
908-771-6419
tanakahr@mri.co.jp

Xia Tang
United Technologies Research Center
411 Silver Lane
E Hartford, CT 6108
81-44-520-5261
tangx@utrc.utc.com

Kyoichi Tange
Project manager
TOYOTA MOTOR CORP.
6-25-603
Mishima city Shizuoka prefecture,
411-0857
81-3-3277-533
tange@fp.tec.toyota.co.jp

Shoji Tange
Technical Adviser
Japan Automobile Research Institute
1-1-30 Shibadaimon,Minato-ku
Tokyo, 105-0012
860-610-7432
stange@jari.or.jp

Shigeharu Tanisho
Professor
Yokohama National University
79-2 Tokiwadai, Hodogaya-Ku
Yokohama, 240-8506
81-055-997-7086
tanisho@ynu.ac.jp

Greg Tao
MSRI
5395 West 700 South
Salt Lake City, UT 84104
3-5733-7927
gtao@msrihome.com

APPENDIX A: ATTENDEE LIST

Hazem Tawfik
Distinguished Professor and Director
State University of New York & BNL
2350 Broadhollow Rd
Farmingdale, NY 11735
81-45-339-3996
tawfikhh@farmingdale.edu

Dale Taylor
Vice President
Ceramatec, Inc.
2425 South, 900 West
Salt Lake City, UT 84119
801-530-4987
dtaylor@ceramatec.com

Amy Taylor
DOE-NE
19901 Germantown Road
germentown, MD 20874
631-420-2307
amy.taylor@nuclear.energy.gov

Tomoaki Terada
Cataler Corporation
7800 Chihama
Kakegawa-City, Shizuoka-Pref, 437-1492
801-978-2132
t-terada@cataler.co.jp

Pascal Tessier
Research scientist
Air Liquide
305 Water Street
Newport, DE 19804-2410
301-903-7722
pascal.tessier@airliquide.com

Praveen Thallapally
Post-doctoral research fellow
University of Missouri-Columbia
601 S.College Ave., Room 125
Columbia, MO 65211
81-537-72-6416
thallapallyk@missouri.edu

Brinda Thomas
Sentech, Inc.
7475 Wisconsin Ave.
Bethesda, MD 20814
302-225-2113
brinda.thomas@ee.doe.gov

C.E. (Sandy) Thomas
President
H2Gen Innovations, Inc.
4740 Eisenhower Avenue
Alexandria, VA 22304
573-882-1811
thomas@h2gen.com

George Thomas
Hydrogen Storage Team
U.S. Department of Energy
1000 Independence Ave., SW
Washington, DC 20585
202-586-6644
george.thomas@ee.doe.gov

Holly Thomas
National Renewable Energy Laboratory
1617 Cole Blvd
Golden, CO 80401
703-778-3122
holly_thomas@nrel.gov

Michael Thompson

PNNL
PO Box 999
Richland, WA 99352
202-586-8058
michael.thompson@PNL.gov

David Thorn
Los Alamos National Laboratory
MS J582
Los Alamos, NM 87545
303-275-3755
dthorn@lanl.gov

Rick Tidball
Director
Energy and Environmental Analysis
12011 NE First Street
Bellevue, WA 98005
509-375-6471
rtidball@eea-inc.com

Tom Timbario
STAFF
505-667-4178

John Titchen
Manager Technology and Commercialisation
Hydro Tasmania
4 Elizabeth Street
Hobart, 7005
425-688-0141
john.titchen@hydro.com.au

Sean Todd
FST, Inc. and Virent Energy Systems
1910 Hackamore Lane
Alexandria, VA 22308
sean@foxpotomac.com

Rick Todd
H2Gen Innovations Inc
4740 Eisenhower Avenue
Alexandria, VA 22304
(6141) 814-5197
rtodd@h2gen.com

KENSUKE TOJIMA
President & CEO
H3 ENERGY LTD.
2800-200 Granville Street
Vancouver, V6G 3H7
202-543-0808
kensuke.tojima@mitsubishicorp.com

George Tolley
President
RCF Economic and Financial Consulting, Inc.
333 N. Michigan Ave., Suite 804
Chicago, IL 60601
703-778-3124
gtolley@rcecon.com

Peter Tortorelli
Distinguished Staff Member/Grp. Leader
Oak Ridge National Laboratory
P.O. Box 2008
Oak Ridge, TN 37831-6156
604-685-8008
tortorellipf@ornl.gov

James Tour
Rice University
6100 Main Street; MS 222
Houston, TX 77005-1892
312-431-1540
tour@rice.edu

Ali T-Raissi
Director, Hydrogen R&D Division
Florida Solar Energy Center
1679 Cearlake Road
Cocoa, FL 32922-5703
865-574-5119
ali@fsec.ucf.edu

Doanh Tran
Manager, Fuel Cell Systems
DaimlerChrysler
800 Chrysler Drive, CIMS 483-00-08
Auburn Hills, MI 48326-2757
713-348-6246
dtt4@dcx.com

Nick Tran
Science advisor, FST
1727 Massachusetts Ave. NW, # 812
Washington, DC 20036
321-638-1446
nicketran@gmail.com

Mike Trenary
Professor, Univ. of Illinois at Chicago
845 W Taylor Street
Chicago, IL 60607-7061
248-576-4297
mtrenary@uic.edu

Pascal Tromeur
R&D Group Manager
Air Liquide - MEDAL
305 Water Street
NEwport, DE 19804
202-246-6805
pascal.tromeur@airliquide.com

Yu-Min Tsou
E-TEK Division, PEMEAS Fuel Cell Technologies
39 Veronica Avenue
Somerset, NJ 8873
312 996-0777
ym.tsou@etek-inc.com

Tony Tubiolo
SENTECH, Inc.
7475 Wisconsin Ave
Bethesda, MD 20814
302-225-2192
ttubiolo@sentech.org

William Tumas
Program Manager
Los Alamos National Laboratory
MS C331
Los Alamos, NM 87544
732-545-5100 x 145
tumas@lanl.gov

Elizabeth Turnbull
Research Analyst
Sentech, Inc
7475 Wisconsin Ave
Bethesda, MD 20817
240-223-5545
eturnbull@sentech.org

John Turner
Principal Scientist
National Renewable Energy Laboratory
1617 Cole Blvd
Golden, CO 80401-3393
505-667-3803
jturner@nrel.gov

Reginald Tyler
Project Officer
DOE/GO
1617 Cole Boulevard
Golden, CO 80123
978-758-1051
reginald.tyler@go.doe.gov

Terrence Udovic
Senior Scientist
National Institute of Standards & Technology
100 Bureau Dr., MS 8562
Gaithersburg, MD 20899-8562
303-274-4270
udovic@nist.gov

Masahiro Usami
Chief Officer
NEDO
20F Muza Kawasaki Building,1310 Omiya-cho Saiwai-k
Kawasaki-shi Kanagawa-ken, 212-8554
303-275-4929
usamimsh@nedo.go.jp

John Vajo
HRL Laboratories, LLC
3011 Malibu Canyon Road
Malibu, CA 90265
301-975-6241
vajo@hrl.com

Jeroen Valensa
Sr. Principal Engineer
Modine Manufacturing
1500 Dekoven Ave.
Racine, WI 53403
81-44-520-5261
j.valensa@na.modine.com

Klaus van Benthem
Oak Ridge National Laboratory
1 Bethel Valley Road
Oak Ridge, TN 37831
310-317-5754
benthem@ornl.gov

Andre Van Rest
Executive Director
Mid-Atlantic Hydrogen Coalition
111 South Calvert Street, suite 2201
Baltimore, MD 21202
262-636-1812
avr1743@msn.com

John Van Zee
Professor, Interim Director, Future Fuels TM Initi
University of South Carolina
Swearingen Engineering Center, 301 Main Street
Columbia, SC 29208
865-241-4573
vanzee@engr.sc.edu

Nicholas Vanderborgh
Consultant
495 Locust Place
Boulder, CO 80304
410-547-0033
n.vanderborgh@comcast.net

Thomas Vanderspurt
Fellow
United Technologies Research Center
411 Silver Lane
East Hartford, CT 6108
803-777-2285
vanderth@utrc.utc.com

Keith Vanderveen
PMTS
Sandia National Laboratories
7011 East Ave.
Livermore, CA 94551
720-406-9951
kbvande@sandia.gov

Michael VanDerwerken
Advanced Technology BD Manager
GE Global Research
One Research Circle, KW-C259
Niskayuna, NY 12302
860-610 7172
vanderwerken@research.ge.com

Laura Verduzco
Sentech, Inc.
7475 Wisconsin Ave Suite 900
Bethesda, MD 20814
925-294-3207
laura.verduzco@ee.doe.gov

Puneet Verma
Chevron
1254 Daleview Drive
McLean, VA 22102
518-387-6141
pverma@chevron.com

Hendrik Verweij
Professor
The Ohio State University
291A Watts Hall, 2041 N. College Rd.
Columbus, OH 43210
202-586-6757
verweij.1@osu.edu

Harry Vidas
Managing Director
EEA Inc.
1655 N Fort Myer Drive Suite 600
Arlington, VA 22209
703-848-0419
hvidas@eea-inc.com

Jud Virden
Deputy Associate Laboratory Director
Pacific Northwest National Laboratory
POB 999 K1-98
Richland, WA 99352
614-247-6987
jud.virden@pnl.gov

Anil Virkar
Vice President
Materials and Systems Research, Inc.
5395 West 700 South
Salt Lake City, UT 84104
703-528-1900
anil.virkar@m.cc.utah.edu

Gerald Voecks
NREL Consultant
2408 Janet Lee Dr.
La Crescenta, CA 91214
509-375-6512
gvoecks@aol.com

Scott Voorhees
Interim CEO
Hydrogen Solar LLC / UNLV
350 Sunpac Court
Henderson, NV 89015-4440
801-581-5396
swv@hydrogensolar.com

Henry Voss
VP Engineering, PolyFuel
1245 Terra Bella Ave
Mountain View, CA 94043
818-249-9279
hvoss@polyfuel.com

Arthur Voter
Lab Fellow
Los Alamos Natinal Lab
T-12 MS B268
Los Alamos, NM 87545
702-579-7600
afv@lanl.gov

Miomir Vukmirovic
Brookhaven National Laboratory
Chemistry Department, Bldg. 555
Upton, NY 11973
650-429-4643
miomir@bnl.gov

Frederick Wagner
Program director
Energetics Inc.
7164 Gateway Drive
Columbia, MD 21046
505-667-6198
fwagner@energetics.com

Frederick Wagner
Technical Fellow
General Motors Fuel Cell Activities
10 Carriage St., P.O. Box 603
Honeoye Falls, NY 14472-0603
631-344-8444
frederick.t.wagner@gm.com

Keiko Waki
Associate professor
Tokyo Institute of Technology
Nagatsuta-cho 4259, Midori-ku
Yokohama, 226-8502
410-953-6232
wen@es.titech.ac.jp

James Waldecker
Ford Motor Company
1201 Village Road
Dearborn, MI 48124
585-624-6726
jwaldeck@ford.com

Craig Walker
United Technologies Research Center
411 Silver Lane
East Hartford, CT 6108
45-924-5614
walkercr@utrc.utc.com

Xiaoping Wang
Chemist
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439
313-594-1435
wangx@cmt.anl.gov

Conghua Wang
TreadStone Technologies, Inc.
201 Washington Rd
Princeton, NJ 8543
860-610-7283
cwang@TreadStone-Technologies.com

Michael Wang
Section Manager
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439
630-252-1182
mqwang@anl.gov

Heli Wang
Scientist
National Renewable Energy Laboratory
1617 Cole Blvd
Golden, CO 80401
609-734-3071
heli_wang@nrel.gov

James Wang
Sandia National Laboratories
7011 East Ave. MS9403
Livermore, CA 94550
630-252-2819
jcwang@sandia.gov

Pen-Cheng Wang
University of Pennsylvania
231 South 34th Street
Philadelphia, PA 19104
303-275-3858
pwang0@sas.upenn.edu

Xinwei Wang
Postdoc
SUNY-ESF, Syracuse Campus
240 Jahn Lab / 1 Forestry Dr.
Syracuse, NY 13210
925-294-2786
wangxinwei@yahoo.com

Yong Wang
Pacific Northwest National Laboratory
902 Battelle Blvd / MSIN: K8-93
Richland, WA 99354
215-898-2224
yong.wang@pnl.gov

Shogo Watanabe
Deputy General Manager
Japan Automobile Researh Institute
2530, Karima
Tsukuba, Ibaraki, 305-0822
315-470-6845
swata@jari.or.jp

Rick Watson
RTI International
3040 Cornwallis Road
Research Triangle Park, NC 27709
509-376-5117
rwatson@rti.org

Sharlene Weatherwax
Program Manager
Dept. of Energy/ Office of Science
SC-23.1/GTN, 1000 Independence Ave., SW
Washington, DC 20585
81-29-856-1169
sharlene.weatherwax@science.doe.gov

Adam Weber
Scientist
LBNL
1 Cyclotron Road, MS 70-108B
Berkeley, CA 94720
919-541-8831
azweber@lbl.gov

Robert Wegeng
Program Executive
NASA
300 E Street SW
Washington DC, DC 20546
301-903-6165
robert.wegeng-1@nasa.gov

James Wegrzyn
PI
Brookhaven National Laboratory
Building 815
Upton, NY 11973
510-643-1972
jimtheweg@bnl.gov

K. Scott Weil
Pacific Northwest national
Laboratory
902 Battelle Blvd
Richland, WA 99352
202-358-1401
scott.weil@pnl.gov

Alan Weimer
Professor of Chemical Engineering
University of Colorado
Department of Chemical and
Biological Engineering
Boulder, CO 80309-0424
631-344-7917
alan.weimer@colorado.edu

Steven Weiner
Senior Program Manager
Pacific Northwest National
Laboratory
901 D Street SW, Suite 900
Washington, DC 20024-2115
509-375-6796
sc.weiner@pnl.gov

Andrew Weisberg
Physicist
LLNL
P.O. Box 808, L-270
Livermore, CA 94551-0808
303-492-3759
weisberg1@llnl.gov

Cory Welch
Senior Project Leader
National Renewable Energy
Laboratory
1617 Cole Blvd
Golden, CO 8040
202-646-7870
cory_welch@nrel.gov

Gary Welch
Chair, fuel cell task force
Renewable Fuels Assoc.
248 Randolph
Peoria, IL 61606
925-422-7293
gary.uelch@arentinerei.com

Edward Wenzinger
Nuclear Engineer/Project Manager
MPR Associates
320 King Street
Alexandria, VA 22314
303-275-4436
ewenzinger@mpr.com

David Wesolowski
Distinguished R&D Staff
Oak Ridge National Laboratory
P.O. Box 2008
Oak Ridge, TN 37831-6110
309-347-9271
wesolowskid@ornl.gov

Rosemarie Wesson
Program Director
National Science Foundation
4201 Wilson Blvd
Arlington, VA 22230
703-519-0518
rwesson@nsf.gov

Douglas Wheeler
DJW TECHNOLOGY, LLC /NREL
5018 Ballybridge Drive
Dublin, OH 43017
865-574-6903
douglas.wheeler@djwtechnology.co
m

Jay Whitacre
Senior Member Technical Staff
Jet Propulsion Laboratory/Caltech
277-212, 4800 Oak Grove Drive
Pasadena, CA 91109
703-292-7070
whitacre@jpl.nasa.gov

Ralph White
Professor
University of S. California
Dept. of Chem. Eng. 301 Main St.
Columbia, SC 29206
614-761-9287
rewesc.edu

Bruce Whitney
DTE Energy
2000 Second Ave
Detroit, MI 48226
818-354-4643
whitneyb@dteenergy.com

Robert Wichert
Technical Director
US Fuel Cell Council
6342 Parkcreek Circle
Citrus Heights, CA 95621
803-777-3270
wichert@fuelcells.com

R. Paul Williamson
Uninversity of Montana
32 Campus Dr
Missoula, MT 59812
313-235-8274
paul.williamson@umontana.edu

Andrew Williamson
Physicist
LLNL
7000 East Ave.
Livermore, CA 99550
916-966-9060

Michael Wilson
CEO
FuelSell Technologies, Inc.
5356 Locksley Ave.
Oakland,, CA 94618
406-243-2932
mike@fstenergy.com

Dane Wilson
Oak Ridge National Laboratory
P.O. 2008, 4500-S, MS-6156
Oak Ridge, TN 37831-6156
925-922-8285
wilsondf@ornl.gov

Mahlon Wilson
Los Alamos National Laboratory
MS D429
Los Alamos, NM 87545
510-985-1644
mahlon@lanl.gov

Robert Wimmer
Environmental Research Mgr.
Toyota Motor North America
601 13th Street, NW, Suite 910
Washington, DC 20878
865-576-4810
robert_wimmer@tma.toyota.com

Keith Wipke
Senior Engineer II
National Renewable Energy
Laboratory
1617 Cole Blvd
Golden, CO 80401
505-667-9178
keith_wipke@nrel.gov

Christopher Wolverton
Technical Leader
Ford Motor Company
MD3083/SRL P. O. Box 2053
Dearborn, MI 48121-2053
202-463-6843
cwolvert@ford.com

Winnie Wong-Ng
Research Chemist
NIST
A256 MATLS, NIST, 100 Bureau Dr
Gaith ersburg, MD 20899
303-275-4451
winnie.wong-ng@nist.gov

Thomas Wood
Senior Staff Scientist
3M
201-4N-01, 3M Center
Maplewood, MN 55144
313-390-9552
tewood2@mmm.com

Ian Wood
Program Manager
Electricore, Inc.
27943 Smyth Drive, Suite 105
Valencia, CA 91355
301-975-5791
ian@electricore.org

Jerry Woodall
Professor
Purdue University
465 Northwestern Ave.
West Lafayette, IN 47907
651-736-0820
woodall@ecn.purdue.edu

Neal Woodbury
Director
PI of DOE Grant
1001 S. McAllister Avenue
Tempe, AZ 85287-5201
661-600-6314
nwoodbury@asu.edu

Richard Woods
VP of R&D
Intelligent Energy
2955 Redondo
Long Beach, CA 90806
765-494-3479
root.woods@intelligent-energy.com

Feng Jung Wu
Albemarle Corporation
Gulf States Road
Baton Rouge, LA 70805
480-965-3294
robert_wu@albemarle.com

Hui Wu
NIST/UMD
100 Bureau Dr.
Gaithersburg, MD 20899
562-304-7683
huiwu@nist.gov

Shyue Wu
Process Engineer
Aramco Services Company
9009 West Loop South
Houston, TX 77096-1799
225-359-2158
Sherwin.Wu@AramcoServices.com

Steven Wu
Rice University
852 Pastureview
Baton Rouge, LA 70810
301-975-2387
vnork@rice.edu

Tony Wu
Research Engineer
Southern Company
PO Box 2641
Birmingham, AL 35291
713-432-4544
cywu@southernco.com

Ying Wu
Director, Research and Development
Millennium Cell Inc.
One Industrial Way West
Eatontown, NJ 7724
225-768-7850
wu@millenniumcell.com

Yue Wu
Professor
University of North Carolina-Chapel Hill
Department of Physics&Astronomy
Chapel Hill, NC 27599-3255
205-257-5375
yuewu@physics.unc.edu

Sherwin Wu
Aramco Services Company
732-542-4000

Xiao-Dong Xiang
Intematix Corporation
46410 Fremont Blvd
Fremont, CA 94538
919-962-0307
xdxiang@intematix.com

Hai Xiao
Associate Professor
New Mexico Tech
801 Lexoy Place
Socorro, NM 87801
510-668-0227 ext.102
xiao@nmt.edu

Yangchuan Xing
Assistant Professor
University of Missouri-Rolla
Department of Chemical &
Biological Engineering
Rolla, MO 65409
505-835-5199
xingy@umr.edu

Liwei Xu
President
Midwest Optoelectronics, LLC
2600 Dorr Street
Toledo, OH 43607
573-341-6772
xu@mwoe.com

Qing Xu
J Craig Venter Institute
9704 Med Ctr Dr
Rockville, MD 20850
419-724-3710
qxu@venterinstitute.org

Omar Yaghi
Professor
UCLA Chem.
607 Charles E. Young Drive East
LA, CA 90095
240-268-2648
yaghi@chem.ucla.edu

Boris Yakobson
Professor
Rice University
MS 321 MEMS Dept 6100 Main Str.
Houston, TX 77005
310-206-0398
biy@rice.edu

Kota Yamada
Asahi Glass Co., LTD
713-348-3572

John Yamamoto
Senior Scientist
Rohm and Haas
60 Willow street
North Andover, MA 1845
978-689-1502
jyamamot@rohmhaas.com

Lea Yancey
Project Engineer
Navarro Research and Engineering
1617 Cole Blvd
Golden, CO 80401
303-275-4944
lea.yancey@go.doe.gov

Gary Yang
Scientist V
Pacific Northwest National Laboratory
PO Box 999, K2-44
Richland, WA 99352
509-375-3756
zgary.yang

Ralph Yang
Dwight F. Benton Professor
University of Michigan
2300 Hayward
Ann Arbor, MI 48109
734-936-0771
yang@umich.edu

Xiao-Qing Yang
Scientist
Brookhaven National Lab.
Bldg. 555, BNL
Upton, NY 11973
631-344-3663
xyang@bnl.gov

Akio Yauda
Corporate manager
Toyota Motor North America
601 13th st.NW,Suite910 South
Washington, DC 20005
202-463-6850
akio_yasuda@tma.toyota.com

William (Bill) Yelon
Research Professor
University of Missouri-Rolla
Materials Research Center
Rolla, MO 65409
573-529-6820
yelonw@umr.edu

Jung Yi
Research Scientist
Arkema Inc.
900 1st Ave.
King of Prussia, PA 19406
610-878-6741
jung.yi@arkemagroup.com

Taner Yildirim
Physicist
NIST and UPENN
100 Bureau Dr., NIST
Gaithersburg, MD 20899
301-9756228
taner@nist.gov

Sung-Pil Yoon
Principal Researcher
Korea Institute of Science & Technology (KIST)
39-1 Hawolkog-dong Seongbuk-gu
Seoul, 136-791
82-2-958-5276
spyoon@kist.re.kr

Phyllis Yoshida
Director, FreedomCAR and Fuel Partnership
U.S. Department of Energy
EE-2G, 1000 Independence Ave., SW
Washington, DC 20585
202-586-7954
Phyllis.Yoshida@ee.doe.gov

Takuji Yoshimaru
ENAA
1-4-6,Nishi-Shinnbashi,Minatoku
Tokyo, 105-0003
3-3502-4444
tdd331@enaa.or.jp

Rosa Young
VP, Technology
Ovonic Hydrogen Systems, LLC
2983 Waterview Dr.
Rochester Hills, MI 48309
248-293-7230
ryoung@ovonic.com

Youxin Yuan
Senior Scientist
State University fo New York - Syracuse
one Forestry Drv.
Syracuse, NY 13210
315-470-4720
yxyuan@esf.edu

Elvin Yuzugullu
Sentech
7475 Wisconsin Ave., Suite 900
Bethesda, MD 20814
240-328-5378
eyuzugullu@sentech.org

Rick Zalesky
President Hydrogen
Chevron Technology Ventures
3901 Briarpark Dr
Houston, TX 77042
713-954-6102
rickzalesky@chevron.com

Thomas Zawodzinski
Professor
CaseWestern Reserve University
2920 Warrington Road
Shaker Heights, OH 44120
216-368-2472
rrw3@case.edu

Piotr Zelenay
Los Alamos National Laboratory
MST-11, MS D429
Los Alamos, NM 87545
505-667-0197
zelenay@lanl.gov

Taofang Zeng
Professor
North Carolina State University
3180A Broughton Hall
Raleigh, NC 27695
919-515-5298
tzeng2@ncsu.edu

Jiujun Zhang
Group Leader- PEMFC
National Research Council Canada
3250 East Mall
Vancouver, BC, V6T 1W5
604-221-3087
Jiujun.Zhang@nrc.gc.ca

Vicky Zhang
Sales Manager
Hydrogenics Corporation
5985 McLaughlin Road
Mississauga, Ontario L5N 6H1
905-361-4561
vzhang@hydrogenics.com

Lingzhi Zhang
The Ohio State University
140 West 19th. Ave.
Columbus, OH 43210
614-292-4993
zhang.464@osu.edu

Shengbai Zhang
National Renewable Energy Laboratory
1617 Cole Blvd.
Golden, CO 80401
303-384-6622
shengbai_zhang@nrel.gov

J.-C. Zhao
Hydrogen storage team leader
GE
1 Research Circle
Niskayuna, NY 12309
518-387-4103
zhaojc@research.ge.com

Yiping Zhao
Assistant Professor
University of Georgia
Department of Physics and
Astronomy
Athens, GA 30602
706-542-7792
zhaoy@physast.uga.edu

Yuan Zheng
Visiting assistant professor
Purdue University
500 Allison Rd.
West Lafayette, IN 47907
1-765-494-0061
zhengy@ecn.purdue.edu

Guanghui Zhu
Intematix Corporation
46410 Fremont Blvd
Fremont, CA 94538
510-668-0227x118
gzhu@intematix.com

Jane Zhu
Program Manager
DOE-Science
19901 Germantown Road
Germantown, MD 20874
301-903-3811
jane.zhu@science.doe.gov

Ragaiy Zidan
Fellow Scientist
Savannah River National Laboratory
Savannah River Site
Aiken, SC 29808
803-725-1726
ragaiy.zidan@srnl.doe.gov

Dick Ziegler
Senior Vice President
SENTECH, Inc.
PO Box 5954
Oak Ridge, TN 37831
865-805-8293
RZiegler@sentech.org

Stephen Zimmer
Director, Government Collaborative
Programs
DaimlerChrysler
800 Chrysler Drive, CIMS 482-01-20
Auburn Hills, MI 48326-2757
248-576-2890
sez@dcx.com

APPENDIX B: FY 2006 MERIT REVIEW AND PEER EVALUATION MEETING: PROJECTS NOT REVIEWED

2006 Projects Presented but Not Reviewed

	Title	Name	Organization
ANP-1	H2A New Developments	Margaret Mann	NREL
ANP-2	Impact of Renewables on Hydrogen Transition Analysis	Stephen Lasher	TIAX
ANP-3	Hydrogen Systems Analysis: Validation of “idealized city” models for H2 delivery in urban areas, with real-city data	Joan Ogden	UC Davis
BES/ST-1	Chemical Hydrogen Storage in Ionic Liquid Media	Larry Sneddon	U. of Pennsylvania
BES/ST-2	Control of Hydrogen Release and Uptake in Condensed Phases	Tom Autrey	PNNL
BES/ST-3	From fundamental understanding to predicting new nanomaterials for high capacity hydrogen storage and fuel cell technologies	Jack Fischer	U. of Pennsylvania
BES/ST-4	Metal-Organic Frameworks for Highly Selective Separations	Omar Yaghi	UCLA
BES/ST-5	Addressing Grand Challenges Through Advanced Materials	Millie Dresselhaus	MIT
BES/ST-6	Atomistic Transport Mechanisms in Revisible Complex Metal Hydrides	Peter Sutter	BNL
BES/ST-7	In-Situ Neutron Diffraction Studies of Novel Hydrogen Storage Materials	William Yelon	U. Missouri
BES/ST-8	In-Situ NMR Studies of Hydrogen Storage Systems	Mark Conradi	WUSTL
BES/ST-9	High Throughput Screening of Nanostructured Hydrogen Storage Materials	Gang Chen	MIT
BES/ST-10	Complex Hydrides -- A New Frontier for Future Energy Applications	Vitalij Pecharsky	Ames
BES/ST-11	Molecular Hydrogen Storage in Novel Binary Clathrate Hydrates at Near-Ambient Temperatures and Pressures	Dendy Sloan	Colorado School of Mines
BES/ST-12	Atomistic Mechanisms of Metal-Assisted Hydrogen Storage in Nanostructured Carbon	Nidia Gallego	ORNL
BES/ST-13	A Synergistic Approach to the Development of New Classes of Hydrogen Storage Materials	Jeff Long	LBNL
BES/ST-14	Elucidation of Hydrogen Interaction Mechanisms with Metal-Doped Carbon Nanostructures	Ragaiy Zidan	Savannah River National Lab
BES/ST-15	Characterization of Carbon Nanostructures in Pd Containing Activated Carbon Fibers Using Aberration-Corrected STEM	Nidia Gallego	ORNL

BES/ST-16	Theoretical Investigation of the Energetics of Hydrogen Interaction with Graphene Layers: the Effect of Interlayer Spacing on Hydrogen Storage	Rachel Aga	ORNL
BES/ST-17	Neutron Scattering Aided Studies of the Design, Synthesis and Thermo-dynamics of molecular hydrogen adsorption materials	John Larese	ORNL
BES/ST-18	First-Principles Studies of Phase Stability and Reaction Dynamics in Complex Metal Hydrides	Mei-Yin Chou	Georgia Institute of Technology
BES/ST-19	Crystal and electronic structures of LiNH2 and related compounds	W.B. Yelon	University of Missouri-Rolla
BES/ST-20	Understanding the Role (and Controlling the Behavior) of Transition Metal Dopants in NaAlH4 Systems	Tabbetha Dobbins	Louisiana Tech University
BES/ST-21	Integrated Nanoscale Metal Hydride – Catalyst Architectures for Hydrogen Storage	Yi-Ping Zhao	University of Georgia
BES/ST-22	The Molecular Design Basis for Hydrogen Storage in Clathrate Hydrates	Vijay John	Tulane University
BES/ST-23	First Principles Based Simulation of Hydrogen Interactions in Complex Hydrides	Qingfeng Ge	Southern Illinois University
BES/ST-24	Dehydrogenation of Boron-Nanoclusters	Michael Trenary	University of Illinois at Chicago
BES/ST-25	NMR Studies of Metal Hydrides: MgScHx	Mark Conradi	Washington University
CCP-2	Enabling Hydrogen Transitions - NETL	David Haberman	DOE - NETL; IF, LLC
FCP-1	Center for Intelligent Fuel Cell Materials Design Phase 1	Joe Mausar	Chemsultants International
FCP-2	Poly(p-phenylene Sulfonic Acid)s with Frozen-in Free Volume for use in High Temperature Fuel Cells	Morton Litt	Case Western Reserve University
FCP-3	Poly(cyclohexadiene)-Based Polymer Electrolyte Membranes for Fuel Cell Applications	Jimmy Mays	U of Tennessee
FCP-4	NanoCapillary Network Proton Conducting Membranes for High Temperature Hydrogen/Air Fuel Cells	Peter Pintauro	Case Western Reserve University
FCP-5	Lead Research and Development Activity for High Temperature, Low Relative Humidity Membrane Program	James Fenton	U of Central Florida
FCP-6	Protic Salt Polymer Membranes: High-Temperature Water-Free Proton-Conducting Membranes	Dominic Gervasio	Arizona State
FCP-7	Novel Approaches to Immobilized Heteropoly Acid (HPA) Systems for High Temperature, Low Relative Humidity Polymer-Type Membranes	Andrew Herring	Colorado School of Mines
FCP-10	High Temperature Membrane With Humidification-Independent Cluster Structure	Ludwig Lipp	FuelCell Energy, Inc.

FCP-11	Design and Development of High-Performance Polymer Fuel Cell Membranes	Joyce Hung	General Electric
FCP-12	Dimensionally Stable High Performance Membrane	Han Liu	Giner Inc.
FCP-14	Development of Higher Temperature Membrane and Electrode Assembly for Proton Exchange Membrane Fuel Cell Device	Tony DeCarmine	Oxford Perf. Matls.
FCP-15	Fluoroalkylphosphonic-acid-based proton conductors	Stephen Creager	Clemson
FCP-16	Dimensionally Stable High Temperature Membranes	Cortney Mittelsteadt	Giner Electrochemical Systems
FCP-17	New Proton Conductive Composite Materials with Co-continuous Phases Using Functionalized and Crosslinkable TFE/VDF Fluoropolymers	Serguei Lvov	Penn State
FCP-18	Advanced Materials for Proton Exchange Membranes	James McGrath	Virginia Tech
FCP-19	Characterization of PEMFC Membrane Durability	Robert Moore	U of So. Mississippi
FCP-21	PEM Fuel Cell Freeze Durability and Cold Start Project	Jeremy Meyers	UTC Power
FCP-22	Kettering University Fuel Cell Project	Joel Berry	Kettering University
FCP-23	Sub-Freezing Start-up of a Fuel Cell	Dennis Papadias	ANL
FCP-24	Fuel Cell Testing at the Argonne Fuel Cell Test Facility	Ira Bloom	ANL
FCP-28	Impurity Effects on Membrane-Electrode Assembly Components	Debbie Myers	ANL
FCP-30	Novel, Combinatorial Method for Developing Cathode Catalysts for Fuel Cells	Keith Kepler	Farasis Energy
FCP-31	Improved Fuel Cell Cathode Catalysts Using Combinatorial Methods	Eugene Smotkin	NuVant Systems
FCP-32	University of South Carolina Fuel Cell Design Project	John Van Zee	U of So. Carolina
FCP-33	Powering Cell Phones with Fuel Cells Running on Renewable Fuels	Malcolm Mann	Tekion, Inc.
FCP-34	Complex Coolant Fluid for PEM Fuel Cell Systems	Satish Mohapatra	Advanced Fluids Tech.
FCP-35	DMFC Prototype Demonstration for Comsumer Electronic Applications	Robert Sievers	MTI Micro Fuel Cells
FCP-36	Direct Hydrogen PEMFC Manufacturing Cost Estimation for Automotive Applications	Eric Carlson	TIAX
FCP-37	Mass Production Cost Estimation for Direct H2 PEM Fuel Cell System for Automotive Applications	Brian James	DTI
FCP-38	Economical High Performance Thermoplastic Composite Bipolar Plates	Michael Bortner	Nanosonic, Inc.
FCP-39	DMFC Power Supply for All-Day True-Wireless Mobile Computing	Brian Wells	Polyfuel, Inc.
FCP-41	Development of a kW Prototype Coal-based Fuel Cell	Steven Chuang	University of Akron

PDP-1	Autothermal Cyclic Reforming Based Hydrogen Generating System	Ke Liu	GE Energy
PDP-3	Materials Solutions for Hydrogen Delivery in Pipelines	Subodh K. Das	Secat, Inc.
PDP-4	Biological Systems for Hydrogen Photoproduction	Maria Ghirardi	NREL
PDP-5	Low Cost Hydrogen Production Platform	Tim Aaron	Praxair
PDP-7	Investigation of Bio-ethanol Steam Reforming over cobalt based catalysts	Umit Ozkan	Ohio State U
PDP-9	Carbon Molecular Sieve Membrane as Reactor for Water Gas Shift Reaction	Paul KT Liu	Media & Process Tech.
PDP-11	Maximizing Light Utilization Efficiency & Hydrogen Production in Microalgal Cultures	Tasios Melis	UC Berkeley
PDP-14	Photoelectrochemical Water Systems for H2 Production	John Turner	NREL
PDP-19	Forecourt Storage and Compression Options	Bill Liss	GTI
PDP-20	Photoelectrochemical Generation of Hydrogen Using Sonicated Hybrid Titania Nanotube Arrays	Mano Misra	U of Nev. Reno
PDP-21	Evaluation of Alternative Thermochemical Cycles	Michele Lewis	ANL
PDP-22	UNLV Research Foundation High Temperature Heat Exchanger Development	Tony Hechanova	UNLV
PDP-23	Membrane Applications for Nuclear Hydrogen Production Processes	Brian Bischoff	ORNL
PDP-24	Materials for Nuclear Hydrogen Production Processes: Planning & Coordinating Task	Dane Wilson	ORNL
PDP-25	Hybrid Sulfur Thermochemical Process Development	Bill Summers	SRS
PDP-26	Advanced Water Gas Shift Membrane Reactor	Thomas Vanderspurt	United Technologies Corp
PDP-27	Robust Low-Cost Water-Gas Shift Membrane Reactor for High-Purity Hydrogen Production from Coal-Derived Syngas	Zhijiang Li	Aspen Products Group
PDP-28	The Integration of a Structural Water Gas Shift Catalyst with a Vanadium Alloy Hydrogen Transport Device	Thomas Barton	Western Research Institute
PDP-29	Production and Storage of Hydrogen Using C1 Chemistry	Gerald Huffman	U of Kentucky Consortium
PDP-30	High-Performance, Durable, Palladium-Alloy Membrane for Hydrogen Separation & Purification	Scott Hopkins	Pall Corp.
PDP-35	Production of Hydrogen for Clean and Renewable Sources of Energy for Fuel Cell Vehicles	Xunming Deng	U of Toledo
PDP-37	Production, Fuel Cell, and Delivery Research	Yogi Goswami	U of South Florida
STP-1	High Density Hydrogen Storage System Demonstration Using NaAlH4 Complex Compound Hydrides	Dan Mosher	UTRC

STP-11	Metal Hydride Center of Excellence	Lennie Klebanoff	Sandia-Livermore
STP-13	Electron-Charged Graphite-Based Hydrogen Storage Material	Chinbay Fan	Gas Technology Institute
STP-14	Nanostructured Activated Carbon for Hydrogen Storage	Israel Cabasso	State University of New York
STP-20	DOE Carbon-based Hydrogen Storage Center of Excellence: Center Highlights and NREL Activities	Lin Simpson	NREL
STP-22	Process for the Regeneration of Sodium Borate to Sodium Borohydride	Ying Wu	Millenium Cell, Inc.
STP-23	Chemical Hydride Slurry for Hydrogen Production and Storage	Andrew McClaine	Safe Hydrogen, LLC
STP-24	Development of Regenerable High Capacity Boron Nitrogen Hydrides for Hydrogen Storage	Ashok Damle	Research Triangle Institute
STP-28	Safety Analysis and Applied Research on the Use of Borane-Amines for Hydrogen Storage	Clint Lane	Northern Arizona U.
STP-29	DOE Chemical Hydrogen Storage Center of Excellence	Bill Tumas	LANL
STP-30	A Synergistic Approach to the Development of New Classes of Hydrogen Materials	Jeffrey Long	UC Berkeley/LBNL
STP-31	Hydrogen Storage Materials with Binding Intermediate Between Chemisorption and Physisorption	Tony Cheetham	UC Santa Barbara
STP-32	Inorganic Clathrates for Hydrogen Storage	Viktor Struzhkin	Carnegie Institute of Washington
STP-32	A Radically New Method for Hydrogen Storage in Hollow Glass Microspheres	James Shelby	Alfred University
STP-33	Unexpected Gas Sorption Displayed by Organic Clathrates	Jerry Atwood	U of Missouri
STP-34	National Testing Laboratory for Solid-State Hydrogen Storage Technologies	Michael Miller	SwRI
STP-35	Low Cost, High Efficiency, High Pressure Hydrogen Storage	Jui Ko	Quantum Technologies, Inc.
STP-36	Advanced Concepts for Containment of Hydrogen and Hydrogen Storage Materials	Andrew Weisberg	LLNL
STP-37	Advanced Metal Hydrides	Jim Ritter	U of South Carolina
STP-38	Clean Energy Research: Project III: Hydrogen Storage Using Chemical Hydrides	Michael Matthews	U of South Carolina
STP-39	A Cassette Based System for Hydrogen Storage and Delivery	Wayne Britton	FST Energy
STP-40	Purdue Hydrogen Technology Program	Jay Gore	Purdue University
STP-41	Center for Hydrogen Storage Research at Delaware State University	Andrew Goudy	Delaware State University
STP-42	University of Arkansas at Little Rock Hydrogen Storage Project	Alexandru Biris	U of Arkansas
STP-44	First-Principles Computational Search for Reversible Room-Temperature Hydrides	Vidvuds Ozolins	UCLA
TVP-2	Business Opportunities Concept Project	Raymond Hobbs	Pinnacle

TVP-4	Hydrogen from Biomass for Urban Transportation	Kofi Bota	Clark Atlanta Univ.
TVP-7	Technology Validation: Fuel Cell Bus Evaluations	Leslie Eudy	NREL
TVP-12	R&D of a PEM Fuel Cell, Hydrogen Reformer, and Vehicle Refueling Facility (Las Vegas Energy Park)	Ed Kiczek	Air Products
TVP-13	To Evaluate Zero Emission Propulsion and Support Technology for Transit Buses	Arthur Douwes	Santa Clara Valley Trans Authority

APPENDIX C: FY 2006 MERIT REVIEW AND PEER EVALUATION MEETING: FEEDBACK AND RECOMMENDATIONS

These notes summarize the comments received from various participants at the May 15-19, 2006 Review:

Section 1: Comments received from Peer Reviewers during feedback sessions held immediately after each subprogram track was completed. The comments received were generally focused on the basic review process; however, where relevant, notes specific to a particular subprogram session are included.

Section 2: Scores and summarized answers to questions from the Review Questionnaire, filled out by approximately 124 of the participants.

Section 1 – Peer Reviewer Comments

General Review Comments

- Overall, the review seemed smoother than last year.
- This week in May is difficult for university attendees. It's so close to final exams and graduation; scheduling the review a week or two later would be helpful.
- The location was very convenient – close to the airport and subway.
- There were too many people in the corridors. It was difficult to move around during the breaks and it got loud outside when the sessions were still going. Suggestion: Move the food tables farther away from the session room doors.
- The security at the Reagan Center served as a choke point. Suggestion: Consider having it at a 'non-secure' place next year.
- Many do not listen to the lunch presentations. The program is too packed and doesn't allow time for 'processing' data or reacting to what's shown.
- The food was really good this year.
- No fees! Keep it free.
- Plenary Session
  - Safety didn't come across in the Plenary Session or any of the lunch presentations. There are some great things happening in Safety. Awareness needs to be raised; safety is important to pay for along the line.
  - The session was useful.
  - Liked how the session linked together the different areas of the Program.
  - Disliked that there was not enough time for more technical detail.
  - Suggestion: Cover other U.S. agencies in a similar manner.

Review Forms

- Concerns about Review Format: Bulleting comments and forcing the separation of strengths and weaknesses tends to oversimplify the thoughts behind the comments and constrains the potential depth of feedback. (The current format makes it difficult to incorporate qualifiers, i.e. "The project is effective, but…").
- On the first page of the form, consider putting the ranking and numbers on the top to allow notes and comments to stretch all the way down and across the page. The 'comments' boxes do not allow enough space to write.
- Formatting: Bullets don't pop up automatically in Excel spreadsheet.

- Instructions for completing the reviewer form could be improved.
- What is the purpose of the "relevance" question? If the Program is funding the project hasn't that determination already been made?
- A separate "Center Evaluation Form" would be useful in evaluating the centers. Would be able to give better feedback on the centers this way.

Poster Presentations

- Liked that there was more room this year, but it was still difficult to get around people at times.
- Felt that posters, in general, are difficult to review. There is not equal input; reviewers may not all hear the same thing from the PI.
- Suggestion: Limit the number of posters to be reviewed in a session to four per reviewer. This would give each reviewer at least 30 minutes for each poster. Otherwise, there is not enough time to thoroughly review the posters.
- Suggestion: Break up the different research areas (e.g., not have all of the Storage posters on the same night). This would be a way to give fewer posters to each reviewer in a given night.
- Poster Session at Reagan Center: The location away from the hotel presented logistical problems. Many of the hotel sessions didn't let out until 6:00 pm or later, and the security checks at the Reagan Center took too long. It might be better to have the reception in a "non-secure" building or stay in the hotel next year.
- Consider creating more of a break between the oral presentations and the poster sessions. There needs to be more time to relax. Suggestion: Start the poster sessions later.

Fuel Cell Review Session

Technical Progress

- Synergies Among Projects: The national labs tend to coordinate better than industry. With solicitations this year, it will become even more integrated.
- Advanced polymer electrolyte membranes need additional emphasis
- News of new polymer materials seemed to focus on only one research group. This is not the "only show in town," (e.g., Wright-Patterson Air Force Base is developing a new class of polymers with excellent properties).

Oral Presentations

- Having the presentations online ahead of time was a huge help. It allowed for better comment and helped reviewers ask better questions.
- The session moderators were assertive; they kept the presenters on time which provided more time for discussion.
- The technical discussions were much better this year. Because of the longer time allowance, the PIs had more time to present their work. Reviewers liked the longer format as well; better questions arose. Moving more of the 'required' slides to the 'optional' section allowed for a more substantive dialogue.
- In general, the sessions are timed too close together, not allowing the reviewer to "process" or react to the information presented.
- Only the first row of tables had access to power strips. It would be nice to have power strips in the second and third rows and around the room. Until laptops are powered by fuel cells, we need more power. Suggestion: More power strips toward the back.
- More comfortable chairs? We're sitting for a long time.

Systems Analysis Review Session

R&D Portfolio Balance

- A decision-making framework based on the modeling results would be appropriate and a structure for how the Analysis Subprogram supports the overall decision-making of the Hydrogen Program (i.e., which would be a better bang for the R&D buck: higher hydrogen purity in production, or fuel cells and membranes with greater tolerance for impurities?).
- The overarching macro model that will coordinate all the other models and research area is needed.
- Energy security has not been assessed in the portfolio.

Technical Progress

- The international drivers have not been addressed.
- The effect of limited hydrogen production sources for a geographic location should be addressed.
- Analysis of the effect of reductions in the cost of gasoline have on hydrogen competitiveness should be included. Forecasts of gasoline prices and natural gas prices need to be included in the online Hydrogen Analysis Resource Center.
- It is unlikely that hydrogen can ever compete on cost. It should be stated as an assumption of the program, or demonstrated in modeling, that H2 incentives or a constraint on carbon is the only way hydrogen can be competitive.
- Comparing the Hydrogen Program with other EERE programs, analysis of how hydrogen compares to alternative fuels and competing vehicle types, in both cost and emissions, should probably be a task performed centrally by EERE, not within the Hydrogen Program.
- We lack something larger that looks at models and systems in a larger way, including impact of the world economy on the U.S.
- Analysis Strength Areas:
  - Even though there is some overlap, having two models focused on the same question can be good to confirm the models are good – if they produce the same answers as outputs.
- Analysis Weakness Areas:
  - At times, more last year than now, there has been a lack of coordination and too much overlap among projects. Some people are doing modeling for the sake of modeling, and some projects seem to be doing exactly what has already been done.
  - The demand curve is the weakness of all the analyses; infrastructure is forced into the models rather than being a result.
  - Better input and buy-in are needed from automakers on the percentage of hydrogen fueling stations needed in the market.
  - Everything should produce results in $/gge for comparison purposes.

Oral Presentations

- The Team Lead's presentation should feed into a description of which models are going to be used for which decisions and provide information about the creation of a cross-cutting team.
- Analysis project presentations need a structure that requires them to include a summary of their inputs and assumptions, so that reviewers can follow where the model is coming from. Suggestion: Perhaps a one- or two-page summary of the assumptions for every model can be provided.
- Presenters were not able to show live demonstrations of a model. Perhaps this could be made available at some other time of the day, possibly during the poster session. This should at least be made available to the reviewers, if not to other attendees. It would allow reviewers to more easily ask questions or better questions.

- Would like to see more preliminary or illustrative results, to give an idea of what the outcomes will be and how the results will be used.

Storage Review Session

Technical Progress

- Concerned that there seems to be much more basic research. There is not so much emphasis on equipment, engineering/applied research, etc. EERE seems to be moving more toward BES-type research. It is agreed, however, that we need to understand materials and be able to narrow material options down before building a system.
- Question of whether materials will make it. The new materials are improvements, but they also have problems. Tremendous progress has been made from a scientific standpoint, but there is strong doubt that the 2010 targets will be met by 2010.

Oral Presentations

- Observation: There were mistakes between DOE technical targets and system material targets in the presentations. Some way to clarify that these are different and distinguish between the two might be useful.
- The safety aspect, particularly regarding new materials, is lacking. Will these new materials be something that can go safely and realistically in a car? Suggestion: Requiring a safety slide would be helpful.
- Centers of Excellence
  - Would like to see how the different groups within each Center are working together. Suggestion: A 40-minute presentation focused on the Center, a description of the groups working in that Center, and how the groups are working together would be extremely helpful.
  - Although not very realistic because the Centers are being reviewed for their progress, would like to hear the Centers' frank evaluation of how they believe they are doing.
  - Suggestion: If possible, group the presentations according to the different areas in each Center.

Safety Codes & Standards Review Session

Technical Progress

- How do the projects in the Safety Codes & Standards subprogram make their way into informing the actual Standards? Sandia has done a great job, but in some cases there is not a connection.

Oral Presentations

- DOE objectives are in project presentations, but the reviewer has no way of knowing if DOE and project objectives are aligned. If they're not aligned, it is important to explain why. Suggestion: Provide the DOE objectives for the project ahead of time.
- Presenters speak about the work they've done, but it is not always clear that the projects are making progress toward the physical objectives. There is not always as a way to measure progress.
- Suggestion: Have a stronger connection between the objectives and accomplishments from previous years – possibly require the PIs to present their project's history in beginning of the presentation, as well as explain why focus, mission, or goals have changed, if applicable?
- There seems to be a wide range in the number of reviews assigned to each reviewer (varying from 5 to 18).

Section 2 – Review Feedback Questionnaire Responses

DEMOGRAPHIC QUESTIONS

1a. What was your role in the review?

21 Peer Reviewer (please answer questions in Sections A. and B.).
21 Presenter of a Project -- Oral or Poster (please answer questions in Sections A. and C.).
0 Presenter of Program Overview (please answer questions in Sections A. and C.).
25 Attendee, neither Reviewer nor Presenter (please answer questions in Section A. only).

1b. What is your affiliation?

0 Government agency directly sponsoring the program under review.
20 National/government lab, private-sector or university researcher whose project is under review.
17 In an industry directly involved in the program under review.
6 In an industry with interest in the work under review.
1 Government agency with interest in the work.
8 National/government lab, private-sector or university researcher not being reviewed, but who has an interest in the work.
5 Other (please specify, e.g., consultant, retired employee, public, etc.): Consultant, Univ. Prof.

A. QUESTIONS 2 THROUGH 21 FOR ALL ATTENDEES

2.	Purpose and scope of the Hydrogen Program Review were well defined.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.5**
3.	The plenary presentations were helpful to understanding the direction of the Hydrogen Program.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.4**
4.	Sub-program overviews were helpful to understanding the research objectives (during Plenary and the start of each Sub-program track).	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.3**
5.	The quality, breadth, and depth of the following were sufficient to contribute to a well-considered review:	*disagree*				*agree*	
	a. Presentations	*1*	*2*	*3*	*4*	*5*	**4.0**
	b. Question & Answer periods	*1*	*2*	*3*	*4*	*5*	**4.2**
	c. Answers provided concerning programmatic questions	*1*	*2*	*3*	*4*	*5*	**4.0**
	d. Answers provided concerning technical questions	*1*	*2*	*3*	*4*	*5*	**4.0**
6.	Enough time was allocated for presentations.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.4**
7.	Time allowed for the Question & Answer period following the presentations was adequate for a rigorous exchange.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.1**
8.	The questions asked by reviewers were sufficiently rigorous and detailed.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**3.8**
9.	There were no problems with:	*disagree*				*agree*	
	Groupings of projects by technical area	*1*	*2*	*3*	*4*	*5*	**4.4**
	Proprietary data (should not be any at this Review)	*1*	*2*	*3*	*4*	*5*	**4.3**
	Quantity/level of the information presented	*1*	*2*	*3*	*4*	*5*	**3.8**

10. The review was conducted smoothly. *disagree* *agree* *1 2 3 4 5* **4.6**

11. The frequency (once per year) of this formal review process for this Program is:
 61 about right
 2 too frequent
 0 not frequent enough
 0 don't know the frequency of reviews

12. Logistics and amenities were satisfactory. *disagree* *agree* *1 2 3 4 5* **4.7**

13. The visual quality of the presentations was adequate. I was able to see all of the presentations I attended. *disagree* *agree* *1 2 3 4 5* **4.2**

14. The audio quality of the presentations was adequate. I was able to hear all the presentations I attended. *disagree* *agree* *1 2 3 4 5* **4.5**

15. The hotel accommodations were satisfactory. *disagree* *agree* *1 2 3 4 5* **4.3**

16. The information about the Review and the hotel accommodations sent to me prior to the Review was adequate. *disagree* *agree* *1 2 3 4 5* **4.3**

17. What was the most useful part of the review process?

- The opportunity to meet and talk with others working on Hydrogen issues.
- The different sessions pretty much stayed on schedule.
- The technical presentation.
- The information on the results of investigation.
- A quick review of many projects.
- The most important part of the review process was to determine how relevant the approach of the project is towards the present problem. Research progress is also very useful.
- Seeing the big picture and pace of advancement.
- Getting a concentrated review of the activities supported by DOE in each area. My own area of interest is production -- primarily bio-solar. Unfortunately some projects in the area suffered drastic financial cuts.
- Good overview of technical approaches.
- Being able to meet research colleagues and exchange info outside of formal meetings, new contacts made and new insights gained.
- Presentation of objectives and the results of individual programs. Also, reviewer question and responses.
- Information on new technology.
- Learning what others were doing that could be leveraged.
- The focuses of the program, the scope, the highlights of the progress, and the networking.
- Concentration of all program projects.
- Receiving the papers before the conference gave me enough time to prepare my plans at the conference. The reviewers were trained well. Overall the program managers were very accessible and useful in learning about the program.
- Networking with both center partners and others.

- The presentations were very good, however, more than once the batteries went dead on the laser-pointer during presentations.
- Question and Answer during the oral presentations.
- Informal discussions during the breaks.
- The networking, large crowd and uniformity of format.
- Looking at the breadth of work being funded.
- By using common format for all projects it was much easier to compare and contrast.
- The good overview of activities and new developments theory focus session.
- The presentations by PI's.
- The plenary sessions and the subprogram briefings.
- Learning the progress from other groups, building connections and talking to the program director.
- All presentations were at a very high level.
- Basically keeping the focus because it is easy to get off track and start pursuing fundamental issues.
- General information on other work gives perspective.
- Subprogram presentations and question and answer sessions following were critical to raise the significant issues and how to deal with them.
- Question and answer sessions showed insight into general audience and reviewers concerns.
- The presentation information.
- The presentations.
- Learning what was going on in Hydrogen Production (my area).
- In person contact with PIs to further discuss projects outside of formal review periods.
- Learning about the latest research.
- Technical information.
- Hearing about the work performed by others. Meeting with other researchers and discussing work.
- The review is helpful in refining the direction of our research.
- Having all necessary review info ahead of time.
- The contacts and the Q&A were very effective.
- Presentation and updated information.
- Update on the many useful programs funded by DOE and face-to-face meetings with peers and DOE personnel.
- Having external inputs from foreign industries and suppliers to this market.
- The presentation of program overview and the intention of DOE.
- The overview presented at the beginning of the session was very good. The salient points offered in the presentation and indicate how they impact on the goods of the program.

18. What could have been done better?

- All the people continuing their conversation during the lunch presentation was disconcerting, and it must have been very distracting for presenters.
- People interested in different aspects of the program had a hard time attending all the talks of interest due to timing conflicts. The meeting days were too long, and there was no spare time even at lunch. Also, the poster boards need to be further apart to reduce traffic congestion, and many of the presenters slides were overly busy.
- Audience questions and reviewer questions were sparse and not probing. Maybe if the audience was asked to hold questions, reviewers would have spoken up more.
- More time at breaks and lunch.
- Presentation time could be 20 minutes plus 10 minutes for questions.
- More discussion could be a positive.

- When the project is one of multiple related contracts, it could be useful to understand the broader picture in addition to the specific project. Synchronize clocks between sessions.
- The question period was insufficient for asking meaningful questions to the presenter.
- A map of where each presentation and poster fits in each track area for quicker targeting of sessions and posters.
- Notification: I was requested to review projects 3 weeks before the meeting. Definition metrics: A DOE summing of the project objectives (separate from presentation) would have been helpful.
- The conference needs small group discussion sections on selected and focused topics to allow competing groups to challenge and exchange groups.
- Create a visual matrix that illustrates how many pieces of this DOE venture and all of its components will achieve H2 economy.
- It appears that many program answers were using the same source for data H2. It would have been good to have an open panel questioning session about the model to understand limitations of source data inherent in scenario model for production and dispensing.
- Hotel information and reservation.
- More time per project.
- Continue focusing on critical harriers and their resolution.
- I would recommend that an 8 1/2 x 11 sheet be posted outside each salon and at one location centrally located, so that we can review each days program schedule without having to search in our briefcases.
- With regards to item 11 above there may some advantage to splitting the program review in half so that each project is formally reviewed every other year as required, so that each year the materials are more manageable at half the size.
- More room for people to circulate during breaks.
- We had extreme confusion about how many posters to prepare for our directed project.
- More sessions; that is, there should be more than just 3 concurrent sessions to allow more oral presentations.
- Attendees coming and going -- doors opening and closing were distracting.
- Shorter days: 12 hours of program and having lectures at lunch was far too much. Screen position was too low -- lower part of screen was blocked by reviewers.
- Tables for all -- difficult to take notes in chairs alone; list your programs by day, so that one can easily see what is being presented at any given time.
- More detailed info on projects, but this is difficult to do given large number of projects and time available.
- One of your lobby staff needs training for her position. She was unfriendly, rude, and unhelpful. she even questioned me about my use of the metro card offered for the offsite poster session 5/17. Like I am going to misuse it after spending my time and money to participate in the review session. She would do better as a drill sergeant and not as a conference worker.
- Instruction for poster preparation and specific topics for review. Both need to be more detailed for new comer to the DOE program.
- Too many proprietary talks and materials etc.
- The subprogram overview could have given a better overview to help those in the audience less familiar with the technology. For example, I am involved with reforming, but I was very interested in comparing it to other production methods.
- Possibly open discussion panels for sub-groups rather than doing the plenary session or maybe both.
- The PIs should be asked to do a self-evaluation summary on technical and programmable issues. It would also be advisable to identify failures or difficulties. These presentations tend to be too positive.

- Many presentation slides were much too busy and font too small to be read.
- Going through security at Reagan Center was inconvenient. Have coffee available throughout the sessions.
- Presentations should focus more on results impacting go/no-go milestones.
- No road trip to the Reagan Center for posters.
- More rooms at conference hotel.
- During question period consider having person carrying microphone to questioners carrying two microphones so that questioners can have microphone in hand before trying to speak. Second microphone could be given to questioner, while first question is being asked.
- Instructions on when posters should be displayed could have been better.
- Product and vehicle demonstration! People need to see the product.
- Poster reviewing was hard.
- The storage project was too basic and too much modeling, without any discussions of practical issues related to onboard storage.
- Keep the presentation/presenter focused on goals and targets, work accomplished in the last year, discovery and results, and conclusion.
- Progress of the program itself.

19. Overall, how satisfied are you with the review process?

very unsatisfied — *very satisfied*

1 2 3 4 5 **4.4**

20. Would you recommend this review process to others and should it be applied to other DOE programs?

52 *yes* **2** *no*

21. Please provide comments and recommendations on the overall review process.

- There should be at least a nominal fee for attendees, which is waived for the peer reviewers.
- It would be desirable to reduce the number of oral presentations, perhaps by limiting them only to projects that are funded at effort levels in excess of one person year.
- I see no way to avoid a full week for the reviews in the future.
- It's very important for information and coordination in DOE program.
- Overall the review process was good. Except for the timing factor, the rest of the process was satisfactory.
- I missed the Steve Chalk "grand kick - off " presentation -- inspiration is good. Steve helps provide that.
- Too structured control; there is little sense of spontaneity. Presenters are too self-centered with their projects; Center of Excellence presentations are too repetitious and self-serving.
- While the levels of the R & D efforts are high quality, one or two have the potential of achieving the DOE goals.
- Have someone explain relevance of each project and the program itself.
- It was excellent.
- There appear to be too many projects for substantive review and feedback. The reviews actually have little time. As a forum to exchange scientific ideas and give the broad picture to peers, the forum works well.
- Process goes smoothly, but on one level, it reduces a fiscal year of work to progress achieved by mid April (in order to meet slide deadline for merit reviews, which then goes into the program).
- You should convince your colleagues in DOE-FE to do the same for their fuel cell program (not simply seen). Logistically, very well run; only a few moderators allowed speakers to overrun their time.

- I did not respond to question 20 because same review process may not work for all DOE programs.
- To confirm the registration by email before the meeting.
- I am too new to the process to be able to criticize.
- The PD session moderators did an excellent job of keeping to the schedule and allowing adequate Q & A. They also did a great job of interjecting DOE motivation/priorities when appropriate.
- Recommendation -- split the review into subject matter three days rather than have all the presentations running concurrently.
- Presenters with English as a second language talk too fast.
- The PIs need to better define project relevance to the overall goals. The reviews need more emphasis on progress versus go/no-go milestones.
- Review should probably be in a small setting with more direct interaction/discussion between presenters and reviewers.
- I am very pleased with the reviewing process; feel that it is very effective.
- I did not see copy of the rating system used by the reviewers. I presented an un-reviewed poster. I have found it difficult to describe the work sufficiently in the time available during past oral presentations. Also, I have been frustrated when reviewer questions indicated that reviewers had missed or ignored discussions of issues during the talk. I found the poster presentation gave me an opportunity to discuss issues more completely, but not many people stopped to talk. In the past I felt that some of the review comments indicated bias.
- Group more by common areas, distribute more poster sessions, and check in advance contents of presentations to verify consistency with template.
- Very well organized. Appreciated good attendance of fuel cell TDMs and projectors at fuel cell sessions. Great venue for this meeting; suggest keeping it here next year; meeting rooms were just the right size. Excellent food and service; granola, yogurt and fruit served on Tuesday morning was very much appreciated and missed on following mornings, which had tasty but sugary food. Judi Abraham's help with accommodations is appreciated.
- The poster session needs help. Too many posters; not enough time to see and interface.
- I came from Japan. The open mindedness of DOE in allowing foreigners to attend the meeting is good. To make progress on hydrogen and fuel cell technologies that is useful to the market, cooperation through the world is very important.

B. QUESTIONS 22 THROUGH 33 FOR PEER REVIEWERS ONLY

22.	Information about the program/project(s) under review was provided sufficiently prior to the review session.	*disagree* *agree* *1 2 3 4 5*	**4.0**
23.	Review instructions were provided in a timely manner.	*disagree* *agree* *1 2 3 4 5*	**4.6**
24.	The information provided in the presentations was adequate for a meaningful review of the projects.	*disagree* *agree* *1 2 3 4 5*	**3.9**
25.	The evaluation criteria upon which the review was organized were clearly defined and used appropriately.	*disagree* *agree*	
	1. Relevance		
	2. Approach	*1 2 3 4 5*	**4.2**
	3. Technical Accomplishments and Progress	*1 2 3 4 5*	**4.4**
	4. Technology Transfer/Collaboration	*1 2 3 4 5*	**4.3**
	5. Proposed Future Research	*1 2 3 4 5*	**4.1**
		1 2 3 4 5	**4.4**

26. Explanation of the questions within the criteria was clear and sufficient. *disagree* … *agree*

	1	2	3	4	5	
1. Relevance	1	2	3	4	5	**4.3**
2. Approach	1	2	3	4	5	**4.6**
3. Technical Accomplishments and Progress	1	2	3	4	5	**4.4**
4. Technology Transfer/Collaboration	1	2	3	4	5	**4.4**
5. Proposed Future Research	1	2	3	4	5	**4.6**

27. The right criteria and weightings were used to evaluate the project(s)/program. *disagree* … *agree*

	1	2	3	4	5	
1. Relevance	1	2	3	4	5	**4.2**
2. Approach	1	2	3	4	5	**4.5**
3. Technical Accomplishments and Progress	1	2	3	4	5	**4.7**
4. Technology Transfer/Collaboration	1	2	3	4	5	**4.5**
5. Proposed Future Research	1	2	3	4	5	**4.7**

28. There were no problems with the rating scheme (1 through 4) that was available to the Peer Reviewers. *disagree* … *agree* *1 2 3 4 5* **4.6**

29. During the review, reviewers had adequate access to the Principal Investigators. *disagree* … *agree* *1 2 3 4 5* **4.2**

30. Information on the location and timing of the projects was adequate and easy to find. *disagree* … *agree* *1 2 3 4 5* **4.7**

31. The number of projects I was expected to review was
    a. Too many **11**
    b. Too few **4**
    c. About right **81**

32. The reviewers in your session had the proper mix and depth of credentials for the purpose of the review. *disagree* … *agree* *1 2 3 4 5* **4.2**
    27 *Don't know their credentials*

33. Altogether, the preparatory materials, presentations, and the Question & Answer period provided sufficient depth for a meaningful review. *disagree* … *agree* *1 2 3 4 5* **4.2**

C. QUESTIONS 34 THROUGH 45 FOR PRESENTERS ONLY

34. The request to provide a presentation for the review was provided sufficiently prior to the deadline for submission. *disagree* … *agree* *1 2 3 4 5* **4.6**

35. Instructions for preparing the presentation were sufficient. *disagree* … *agree* *1 2 3 4 5* **4.4**

36. The template for the presentation was helpful. *disagree* … *agree* *1 2 3 4 5* **4.4**

37. The PDF format provided adequate functionality for my presentation. *disagree* … *agree* *1 2 3 4 5* **4.3**

38.	The time limit for my presentation was adequate to present the information needed by reviewers.	*disagree*				*agree*		
		1	*2*	*3*	*4*	*5*	**4.6**
39.	The audio and visual equipment worked properly and were adequate.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.6**
40.	The evaluation criteria upon which the review was organized were clearly defined and used appropriately.	*disagree*				*agree*	
	1. Relevance	*1*	*2*	*3*	*4*	*5*	**4.3**
	2. Approach	*1*	*2*	*3*	*4*	*5*	**4.4**
	3. Technical Accomplishments and Progress	*1*	*2*	*3*	*4*	*5*	**4.4**
	4. Technology Transfer/Collaboration	*1*	*2*	*3*	*4*	*5*	**4.1**
	5. Proposed Future Research	*1*	*2*	*3*	*4*	*5*	**4.3**
41.	Explanation of the questions within the criteria was clear and sufficient.	*disagree*				*agree*	
	1. Relevance	*1*	*2*	*3*	*4*	*5*	**4.3**
	2. Approach	*1*	*2*	*3*	*4*	*5*	**4.3**
	3. Technical Accomplishments and Progress	*1*	*2*	*3*	*4*	*5*	**4.3**
	4. Technology Transfer/Collaboration	*1*	*2*	*3*	*4*	*5*	**4.1**
	5. Proposed Future Research	*1*	*2*	*3*	*4*	*5*	**4.2**
42.	The right criteria and weightings were used to evaluate the project(s)/program.	*disagree*				*agree*	
	1. Relevance	*1*	*2*	*3*	*4*	*5*	**4.0**
	2. Approach	*1*	*2*	*3*	*4*	*5*	**3.9**
	3. Technical Accomplishments and Progress	*1*	*2*	*3*	*4*	*5*	**4.1**
	4. Technology Transfer/Collaboration	*1*	*2*	*3*	*4*	*5*	**4.1**
	5. Proposed Future Research	*1*	*2*	*3*	*4*	*5*	**4.1**
43.	There were no problems with the rating scheme (1 through 4) that was used by the Peer Reviewers.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.1**
44.	During the review, reviewers had adequate access to the Principal Investigators.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**4.0**
45.	Altogether, the preparatory materials, presentations, and the Question & Answer period provided sufficient depth of review.	*disagree*				*agree*	
		1	*2*	*3*	*4*	*5*	**3.9**

APPENDIX D: FY 2006 MERIT REVIEW AND PEER EVALUATION MEETING: EVALUATION FORMS

DOE Hydrogen Program
2006 Annual Merit Review

Project Evaluation Form

Project Number: ______ **Reviewer:** ______

Title of Project: ______

Presenter Name: ______

Using the following criteria, rate the work presented in the context of the program objectives and provide **specific, concise** comments to support your evaluation. *** Write/print **clearly** please. ***

1. **Relevance** to overall DOE objectives – the degree to which the project supports the President's Hydrogen Fuel Initiative and the goals and objectives of the applicable Multi-Year RD&D plan. **(Weight = 20%)**

	score	comments
4 - Outstanding. The project is critical to realization of the President's Hydrogen Fuel Initiative and fully supports the RD&D plan objectives. 3 - Good. Most aspects of the project align with the President's hydrogen vision and the RD&D plan objectives. 2 - Fair. The project partially supports the President's hydrogen vision and the RD&D plan objectives. 1 - Poor. The project provides little support to the President's hydrogen vision and the RD&D plan objectives.		

2. **Approach** to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and integrated with other research. **(Weight = 20%)**

	score	comments
4 - Outstanding. The project is sharply focused on one or more key technical barriers to development of hydrogen or fuel cell technologies. Difficult for the approach to be improved significantly. 3 - Good. The approach is generally well thought out and effective but could be improved in a few areas. Most aspects of the project will contribute to progress in overcoming the barriers. 2 - Fair. Some aspects of the project may lead to progress in overcoming some barriers, but the approach has significant weaknesses. 1 - Poor. The approach is not responsive to project objectives and unlikely to make significant contributions to overcoming the barriers.		

3. **Technical Accomplishments and Progress** toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance (effectiveness, efficiency, cost, and benefits). **(Weight = 35%)**

	score	comments
4 - Outstanding. The project has made excellent progress toward objectives and overcoming one or more key technical barriers. Progress to date suggests that the barrier(s) will be overcome. 3 - Good. The project has shown significant progress toward its objectives and to overcoming one or more technical barriers. 2 - Fair. The project has shown modest progress in overcoming barriers, and the rate of progress has been slow. 1 - Poor. The project has demonstrated little or no progress towards its objectives or any barriers.		

Page 1 of 2

4. **Technology Transfer/Collaborations** with industry/universities/other laboratories – the degree to which the project interacts, interfaces, or coordinates with other institutions and projects. **(Weight = 10%)**

	score	comments
4 - Outstanding. Close coordination with other institutions is in place and appropriate; partners are full participants. 3 - Good. Some coordination exists; full and needed coordination could be accomplished fairly easily. 2 - Fair. A little coordination exists; full and needed coordination would take significant time and effort to initiate. 1 - Poor. Most of the work is done at the sponsoring organization with little outside interaction.		

5. **Proposed Future Research** approach and relevance – the degree to which the project has effectively planned its future, considered contingencies, built in optional paths or off ramps, etc. **(Weight = 15%)**

	score	comments
4 - Outstanding. The future work plan clearly builds on past progress and is sharply focused on one or more key technical barriers in a timely manner. 3 - Good. Future work plans build on past progress and generally address removing or diminishing barriers in a reasonable period. 2 - Fair. The future work plan may lead to improvements, but should be better focused on removing/diminishing key barriers in a reasonable timeframe. 1 - Poor. Future work plans have little relevance or benefit toward eliminating barriers or advancing the program.		

Strengths

Weaknesses

Recommendations for Additions/Deletions to Project Scope

Project Number: ______ **Reviewer:** ______

Page 2 of 2

DOE Hydrogen Program
2006 Annual Merit Review

Sub-Program Evaluation Form

Reviewer: ______________________

Title of Sub-Prog: ______________________

Presenter Name: ______________________

Using the following criteria, rate the work presented in the context of the program objectives and provide **specific, concise** comments to support your evaluation. *** Write/print **clearly** please. ***

1. Degree to which the Sub-Program area was adequately covered and/or summarized:

2. Were important problem/issue areas and challenges identified/discussed, including plans for addressing these in the future?:

3. Does the Sub-Program area appear to be focused, managed well, and effective in addressing the DOE Hydrogen Program R&D needs?:

4. Other Comments:

Page 1 of 1

(This page intentionally left blank.)

APPENDIX E: FY 2006 MERIT REVIEW AND PEER EVALUATION MEETING: SUBPROGRAM EVALUATIONS

During the first-day Plenary Session and at the beginning of each Subprogram track, Team Leads presented overview briefings of their Subprogram areas. The Peer Reviewers were provided an evaluation form, different from the one they used to evaluate individual projects, on which to provide comments on these overviews and the overall Subprogram. (The actual evaluation form is provided in Appendix D). These evaluations were voluntary, so the number of responses varied widely across the Subprograms.

The specific questions asked of the Reviewers were:

1. Was the Subprogram area adequately covered and/or summarized?

2. Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

3. Does the Subprogram area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

4. Other comments.

Following are consolidated summaries of the responses received for these Subprogram Evaluations.

Project # AN: Systems Analysis

Fred Joseck

Degree to which the Subprogram area was adequately covered and/or summarized

- A very good brief overview of the program.
- A good summary of achievements.
- A map of how the models fit with respect to the modeling needs and identification of what objectives each model is targeting would be helpful.
- Fred provided a very clear explanation of the Analysis subprogram. Overview was at an adequate level and complemented the presentations very well.
- Covered well—Goals and Objectives were clear and appropriate.
- One thing not clear was the extent of comparison of various hydrogen scenarios with non-hydrogen scenarios for alleviation of petroleum shortages.
- In addition, there was very little discussion of how these analyses will be used in down-selecting various hydrogen approaches for production, delivery, and other aspects of well-to-wheel utilization.
- Not clear whether need for carbon dioxide reduction is being included as a major driver in the various analyses (assuming sequestration at the hydrogen generation site).

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- The models are generally providing analysis of the transition to hydrogen adoption.
- The models don't appear to be as well suited to R&D investment decisions. Perhaps a decision programming approach could be used to assess R&D investments.
- The inclusion of plug in hybrid vehicle technology would be useful in a number of models.
- Challenges and barriers were identified and the approach to plugging those holes was described.
- Mostly yes.
- Market demand issue was not adequately addressed.
- Feedstock pricing volatility impacts need to be examined.
- There is some question in my mind as to whether considering SMR as reducing petroleum usage is appropriate inasmuch as I would tend to lump petroleum and SMR-derived hydrogen in the same bucket (I realize that this can be argued).

Does the Subprogram area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- The program is clearly focused on the overall program needs.
- The Resource Centre and the H2A model are starting to provide a strong backbone for the analysis effort.
- There appears to be overlap between the various models currently being developed.
- Program is well managed, but like other areas, faces the dilemma of having more needs than resources to satisfy those needs. Models being developed and exercised are good "first cuts", but as time and funding permits, they must be refined. Second order impacts on cost, demand, market penetration, etc., must be included in the models to provide a realistic forecast and a better guide for R&D in the other technology areas.
- Yes.

Other comments:

- The basis for assessing R&D investmement decisions is not clear – consideration could be made of using a decision making model framework – perhaps as a layer over the MSM.
- The adoption of cost, energy security and GHG measures across the model would be helpful.
- The modeling of plug in hybrids may be needed across a number of models.

- In the presentation, it is surprising that distributed hydrogen production using wind is assessed to have similar emissions to gasoline vehicles. Is fossil fuel consumption assumed here in order to increase electrolyzer duty? There may be other distributed wind configurations.
- Slide 14 shows the energy security and GHG factors well – these objectives don't appear to be always the objectives of the models that have been developed. Cost is generally the objective but perhaps a constraint should be placed on the models to achieve outcomes with the low GHG and low gasoline.
- The Systems Analysis area is to be complemented for resisting the temptation of simply jumping in and starting projects. It has taken a more organized approach of identifying the holes, filling those holes and then reevaluating the status as technology progresses. Plans for a crosscut analysis team are also important to assure consistency in EERE message to stakeholders.
- For whatever it's worth, I feel that AN-05 and AN-07 are very strong efforts and should be retained and possibly expanded.
- AN-01 and AN-03 also appear to contribute significant value, but I feel that the other four efforts could be discontinued to permit expansion on AN-05 and AN-07 (assuming that you are budget-constrained).

Project # ED: Hydrogen Education

Christy Cooper

Degree to which the Subprogram area was adequately covered and/or summarized

- The program overview was very clear and focused; nice presentation.
- Good overview of future direction. A wrap-up of closed out projects would help. What happened? For example, what is the status of the Hydrogen Learning Centers? For those who only see the whole program effort once a year, this gives continuity.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Between the program overview presentation and Baseline Survey presentation given at lunch, the issues and delays are very clear; there seems to be more incorrect information than knowledge of hydrogen.
- Budget issues clearly presented. Baseline information clearly presented the challenge.

Does the Subprogram area appear to be focused, managed well, and effective in addressing the hydrogen program R&D needs?

- The program is focused on raising "H_2IQ." It is well managed but constrained by budget.
- A lot is being done with very little funding. We need to have a full, uncut hydrogen education budget year.

Other comments:

- Priorities are appropriate for level of funding but there is so much more that needs to be done now.

Project # FC: Fuel Cells
Valri Lightner / Nancy Garland

Degree to which the Subprogram area was adequately covered and/or summarized

- The sub-program was well covered, including barriers, targets, sub-topics and tasks.
- Activities in the subprogram area were clearly summarized with good enthusiasm.
- Between the posters, talk, and the program review, most issues were covered well.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Revisions to the RD&D plan are being considered to modify the barriers and tasks.
- The Subprogram focuses on a very broad scope of issues, not all of which are of a primary importance. Especially in the context of rather limited resources, the Subprogram could benefit from prioritizing tasks and focusing on materials with an enabling role for the introduction of fuel cells to automotive transportation. Components crucial to the ultimate success of PEFCs for transportation, such as cathode catalysts, catalyst supports, membranes, and MEAs should be developed first, before significant effort and resources are invested in other areas.
- What is the impact of a fast, steady growth of in the Pt price on the technical targets established using Pt price less than half of what it is today? There might be a need for a new comprehensive analysis of the impact of the growing price of Pt on technical targets of the Program.
- Balance of plant not covered adequately.

Does the Subprogram area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Yes. I believe the current focus on partnering (industry, National Labs, etc.) and peer review will further advance the programs.
- Some Projects might benefit from consolidation with other closely related programs - in order to better establish "critical mass" and better utilize resources and funding. A method of "consolidation" might be to establish "program matrix" or "interactions" for certain topics, to encourage additional collaboration between the Projects. From the presentations - I can already see this in many of the National Lab projects. There may be additional opportunities with industry, trade organizations, etc.
- The sub-program is well focused and has put go/no-go decision points into many of the projects.
- The program is well managed. Is large funds for some of the industrial projects justified?

Other comments:

- After listening to the Catalyst Projects, one thought - a "summary/compilation" project would be useful to consolidate the findings, developments, etc., - since many of these programs are coming to an end. This "summary" would update any "revisions to theory" (as validated by more recent studies, etc.) and provide a useful guide for "going forward". In the past, this might appear in "The Fuel Cell Handbook" update edition. It would be well spent support money for the DOE program - particularly if assigned to respected colleagues from the Catalyst projects.
- The sub-program is also incorporating some of the recommendations that came from the NRC's review of the FreedomCAR & Fuel Partnership.
- Traditionally, the Annual Review includes projects with very different funding levels, often reviewed one after another. This may put projects with less funding in an inferior position, regardless of the quality of the work done.
- A very low funding level for some projects (a fraction of an FTE) has effectively prevented good quality work from being done. Projects with clearly insufficient, almost "symbolic" funding should be terminated.
- Lunches ought to be reserved for discussion and networking.
- DOE should not fund laboratories merely to test materials developed by others.

Project # PD: Production and Delivery

Patrick Davis

Degree to which the Subprogram area was adequately covered and/or summarized

- A good presentation was made for the sub-program on hydrogen production and delivery. Very informative and well organized.
- Pat did a good job of reviewing the DOE Production Program-Goals, Projects and tie in to Overall DOE strategies.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- The problem/issue areas were adequately discussed.
- As a consultant to the Renewable Fuels Association recognize I am highly biased toward ethanol as a renewable fuel. The plan for addressing the conversion of ethanol to hydrogen was fuzzy and unclear. There seems to be no place in DOE for ethanol. There were presentations on Coal to hydrogen, nuclear to hydrogen, natural gas to hydrogen. There are, properly, advocates for each within DOE. An advocate for ethanol is badly needed.

Does the Subprogram area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- The hydrogen production subprogram covers a wide range of routes to produce hydrogen from different feedstocks.
- A balanced program was presented.
- The program is focused, and managed very well. The comments above concern what is not being done. What is being done is being done well.

Other comments:

- The hydrogen delivery program covers applications for both central and distributed hydrogen production. Both coal to hydrogen and nuclear hydrogen programs are aimed for central hydrogen production. The inputs (comments and suggestions) from coal and nuclear energy offices to the hydrogen delivery program were not adequately discussed.
- In the case of hydrogen production from natural gas, the price of hydrogen is sensitive to natural gas price cost. Will it be more meaningful to say CLEARLY the natural gas price behind the hydrogen target cost?
- Selection of Delivery and Production projects need to be integrated. A difficult hydrogen production technology where the hydrogen delivery to the "forecourt" is inexpensive and readily accomplished is a good project. So is a project where the production of hydrogen is straightforward but delivery is difficult. However coupling a challenging/difficult production technology with a difficult/challenging delivery system should almost always be avoided. Requiring two low probability/high risk projects to be successful to achieve a successful outcome is almost always a loser. If DOE eliminated these low probability/high risk coupled projects, more funds could be applied to projects able to contribute to meeting overall objectives.

Project # SA: Hydrogen Codes and Standards
Antonio Ruiz

Degree to which the Subprogram area was adequately covered and/or summarized

- Program was very well summarized and made a good introduction to the presentations to be made.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- The biggest challenge is harmonization of domestic and international standards. This and the other challenges are recognized and being addressed.

Does the Subprogram area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Well focused on maintaining safety now by applying best practices and developing standards for the future to assure affordable safety.

Other comments:

- One slide showing the relationship of each of the projects would have been helpful.

Project # ST: Hydrogen Storage

Sunita Satyapal

Degree to which the Subprogram area was adequately covered and/or summarized

- The hydrogen storage program was well covered considering the extent of the program and the large numbers of projects being worked on. The opening presentation provided an excellent overview of the storage program for the group and to set the stage for the more detailed review of R&D in storage. The presentation summarized the high level strategy, reported some interim results and provided a plan (timeline, goals and future research solicitations) to continue R&D to move closer to the goals. DOE did a good job of ensuring that key technical areas were covered either in presentations or posters.
- Very good.
- The overview was complete and accurate with chief achievement description.
- The program was covered very well. Adequate amount of detail was provided.
- All the major areas were specifically covered, analysis programs were covered, plus a nice overview of the whole program, its goals and methods of meeting the goals and the challenges plus the plan to meet them. This criterion was well met.
- Reasonably well.
- Targets were defined in detail.
- Centers of Excellence (CoEs) were detailed in composition and general duties (see No. 4).
- Contacts listed and "open door" policy suggested.
- A very clear, comprehensive, down-to-the point summary of the sub-program, of its strategy and technical goals, recent achievements and work plans. Presentation very well delivered - excellent job!
- I was in attendance from presentation ST-05 through ST-28, so, my comments here and below are based on observations made in the framework of these progress reports. I cannot comment on the subprogram presentation because I was not able to be present, hence, I will focus on my general impression based on the presentations I did attend, with emphasis on the ones I reviewed: The broad spectrum of on going activity in this program area was very well covered. Having the three major storage Centers of Excellence give hour-long overviews was a good idea and should be continued. As is usually the case, the quality of the presentations varied considerably. Some projects did not (or so it seems) put their best foot forward.
- The hydrogen storage area was sufficiently covered. All the new aspects were clearly presented and summarized.
- The presentation was clear. The sub-program area was adequately covered given the time frame of the presentation. A comprehensive snapshot of the state of the storage program was also presented and accomplishments were highlighted.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Yes. The three major goals were covered, and the important issue of system vs. material performance was highlighted. The nature of the challenge presented was illustrated in several ways. Challenges in all of the individual approaches were also pointed out. Approaches to solve the problems were touched on. This criterion was met.
- Yes.
- The challenge is "grand" and was well stated in terms of difficulty and importance.
- Longer-term plans were covered, including go/no go decision points. Upcoming solicitations were described.
- Main accomplishments were identified and discussed along with the persisting problem areas. Attention was drawn to research gaps and challenges in order to guide future R&D activities. Emphasis was paid on short- and long-term plans, milestones and go/no go decisions.

- Most presenters put forth future plans that were consistent with findings/issues emanating from work to date. In some cases, the proposed FY 2007 and FY 2008 tasks were not fully elaborated on or clarified to the point where one could appreciate the future plans. Future planning should be driven by two considerations (1) what is learned from the accumulated knowledge based on work to date and (2) what careful analysis says about the most promising pathways to meeting targets.
- Yes. The lead-in presentation set the stage for – and subsequent presentations provided ample opportunity for the important topics and subject matter areas to be discussed. The allocation for a full hour for presentations by each of the CoEs was a good idea considering the importance of the three centers to the delivery of a viable storage system.
- All the important problems were focused and discussed. In addition challenges and targets were evaluated and set for the feature.
- Onboard storage is critical and remains a big challenge. The + and – of the different families of storage materials were outlined and directions discussed. Established a robust portfolio of R&D projects emphasizing HSA [high surface area] materials, and hydrides- complex and chemical.
- The major technical issues have been identified and presented for an open discussion and updating.
- Surface sciences and chemical and material problems were identified and discussed well. But engineering problems, including heat management, impurity in hydrogen, system integration and reactors design were very "slowly" identified and discussed in projects.
- Yes. The barriers were clearly addressed. The work of CoEs and the individual projects were discussed in sufficient detail.

<u>Does the Subprogram area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?</u>

- The strategy is appropriate, to continue emphasizing a wide net of higher risk - high reward research. The use of the CoEs is an appropriate method to manage this diverse portfolio and promote synergy and enhance innovation. Maintaining independent programs ensures agility. Significant progress has been made already, illustration of the effectiveness of the chosen research. Budget increase in this highly difficult and critical area is appropriate. Frankly, funding needs to be expanded even further, the budget shown represents every American contributing an average of a dime per year; surely the richest nation on earth can do better. Given the budget, the DOE team has met this criterion with excellence.
- The storage subprogram is getting very large. It must be an increasing challenge to the DOE staff to manage the details (progress understanding, duplication, technical judgments, etc.).
- A reasonable balance (not perfect) between program needs and people needs (e.g., questionable or burdensome paperwork).
- This appears to be a very well organized and structured sub-program, efficiently managed and sharply focused on addressing the DOE Hydrogen R&D needs.
- The Hydrogen Storage sub-program is clearly well managed at the DOE TDM [technology development manager] level. The implementation effectiveness is also impressive, considering that earmarking has greatly hampered both the CoE funding and the coordination of all R&D efforts to minimize unnecessary overlap.
- Yes. DOE has done a good job of allocating funds for storage work and ensuring that in recent years research dollars for storage have been protected. The concept of CoEs supplemented by independent research in "stretchy" areas of research seems to be focusing key research entities toward a common goal, and we are starting to see remarkable progress.
- The subprogram is well focused in the most important directions of solving the hydrogen storage problem. The management is exceptional for European standards.
- The storage sub-program is focused on the DOE targets, and seems well managed. It is getting more refined, and constantly evolving in the right direction.
- The sub-program is central in the DOE Program.
- The focus is on key scientific aspects.
- There is a need for slightly increasing the technological and operability aspects in the funded activities.
- The projects and CoEs are well organized. The subprogram has done an excellent job coordinating the efforts and maintaining an effective communication with and within CoEs.
- Yes.

- Subprogram people know the participants very well personally.
- Yes, but DOE targets should be extended and include: minimal [minimum] scale [of examples], maximum temperature, flow rates of hydrogen, impurities limitation and other important [points] for engineering.

Other comments:

- Storage requires breakthrough discoveries. The subprogram activities in the past two years have done an outstanding job in developing the proper background and foundation for achieving these discoveries. The creation and development of the CoEs is proving very effective. All the DOE-EERE people involve with these efforts should be congratulated for their contributions.
- The increasing funding is appropriate, though in fact the DOE does need to have an increasing PEER SELECTED portfolio, which can only be achieved if congressionally directed programs bring their own funding rather than diminish the funds available for expert selected programs. One area that needs more emphasis is tank systems and the materials that are required. Most people feel initial mobile and many stationary systems will use compressed tanks, but these are still too expensive and there is room for research to improve this situation on the material and engineering levels. This will be important as well in future [for] solid-phase materials, which will also use pressure tanks of up to 100 bar to enable refueling in 5 minutes.
- I would like to have heard more about how the CoEs are working:
  - How often do they meet? Telecons?
  - Is there good openness (especially by companies)?
  - Are good collaborations happening?
  - Are there frictions over boundaries?
  - Are there many duplications of effort?
  - Anything of a potential concern to their continuation?
  - Should they be larger? Smaller?
- This is indeed a very strong and well-focused Team Leader with a real vision.
- Recommendations:
  - Keep reminding PIs of system rather than material targets and of the need to address engineering aspects and all critical issues and not just gravimetric capacities.
  - Follow closely the progress of the CoEs; their effectiveness also depends on their coordination which still remains challenging due to their size and scientific diversity.
  - Consider interaction mechanisms among the three CoE: there are commonalities in some problems and expertise in Centers that could be shared for tackling them. See for instance, off-board regeneration issues arising in projects in the Metal Hydride CoE that could benefit from interactions with the Chemical Hydrogen Storage CoE.
- Here I offer the following suggestion. Clearly, all the hydrogen storage CoEs are struggling to approach 2010 "system" targets for gravimetric and volumetric capacity. The 2015 targets seem out of reach for at least two of the CoEs. It's possible that this will cause discouragement over the progress of these centers towards the current targets in future years. Targets for 2015 that are based on a smaller, lighter, shorter mileage vehicle would be on the edge of commercial reality but still be in the radar range of the three CoEs.
- The UNLV congressionally mandated program has a large sum of money in it and is not very well planned out or well connected with the rest of the HFCIT Program. Next year their “feet should be held to the fire”. They should be expected to show significant progress towards resolution of barriers to [and] progress in the hydrogen storage and fuel cell sub-programs. They should also show responsiveness to the comments of this year's review panel. EERE is not in the same business as the Office of Science. The UNLV program looks like an Office of Science enterprise.
- If we are going to finish it's more preferable to take a “big fish-hook”; because it's more pleasant not to “hunt a big fish”, than a small one- the same for sciences . Best rewards.

- The storage sessions were very well run. Having sat in several sessions throughout the week, it seemed that the storage session was the only one that rigorously allowed the reviewers to ask questions before opening the Q&A session to the floor. As a result the questions were well-thought out and helped to elucidate the information being presented. Presentations were kept on schedule and agenda moved along smoothly.
- The hydrogen storage subprogram of the DOE is very well organized and always it is mentioned as an example from European scientists.
- In addition to the technical and cost goals for storage materials, the subprogram maybe could/should also address the hazard and safety aspects of the investigated materials. This is a key issue for fuel systems.

Project # TV: Technology Validation
Sig Gronich

Degree to which the Subprogram area was adequately covered and/or summarized

- Adequate.
- Good summary of program, with clear objectives and path forward.
- Presentation summarized program well, and gave a good overview of the impressive progress that has occurred in the last year.
- Program area was very adequately defined. Specific project/programs were identified and summarized.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- The challenges were not delineated.
- Problem areas were addressed, and program is structured to provide necessary information for resolution.
- Yes, Programmatic level events and future "coming attractions" were covered.
- Issues such as vehicle range, durability and costs were noted. Program issues such as how to handle proprietary data as well as the volume of data were identified.

Does the Subprogram area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Adequate.
- Very well-managed program with clear ties to technical targets.
- Yes. Hydrogen infrastructure and FCV's are beginning to enter "the valley of public opinion" when the public decides to get behind this effort and help…or not. The Tech Val programs are the first real public exposure to much of this technology and the careful planning that went into deployment is beginning to show. For instance, there have been no safety incidents of note at any of the sites.
- The program focuses on important issues such as capital costs, durability and efficiencies but it is so broad that some details get lost. It appears to be managed well and good results are being identified but it is still very early.

Other comments:

- This program is a success so far. Maintaining that high performance factor will be challenged by events as they evolve. Flexibility and wisdom in changing the program concurrently will be important. For instance (and not to make a singular fuss over only one thing) it is clear that Honda has a very good, perhaps superior FCV on the road, but this is not included in the present set of DOE Tech Val programs. This may not be important to the validity of DOE's program, but programmatically issues like that and others as they arise need to be fed into the program plan to see if the Technology Validation program should change in some way to accommodate the evolved issue(s).
- Most of the data so far are on small scale demonstrations. They should be scaled up in size to establish the validity of the modeling and results toward the DOE goals.

REPORT DOCUMENTATION PAGE	*Form Approved* *OMB No. 0704-0188*

1. REPORT DATE *(DD-MM-YYYY)*	**2. REPORT TYPE**	**3. DATES COVERED** *(From - To)*
September 2006	Technical Report	10/01/05-09/30/06

4. TITLE AND SUBTITLE
DOE Hydrogen Program: 2006 Annual Merit Reivew and Peer Evaluation Report

5a. CONTRACT NUMBER
DE-AC36-99-GO10337

5b. GRANT NUMBER

5c. PROGRAM ELEMENT NUMBER

6. AUTHOR(S)
JoAnn Milliken

5d. PROJECT NUMBER
NREL/BK-150-40452

5e. TASK NUMBER
HS061006

5f. WORK UNIT NUMBER

7. PERFORMING ORGANIZATION NAME(S) AND ADDRESS(ES)
U.S. Department of Energy
Office of Hydrogen, Fuel Cells and Infrastructure Program
1000 Independence Ave., SW
Washington, D.C. 20585

8. PERFORMING ORGANIZATION REPORT NUMBER
DOE/GO-102006-2351
NREL/BK-150-40452

9. SPONSORING/MONITORING AGENCY NAME(S) AND ADDRESS(ES)
National Renewable Energy Laboratory
1617 Cole Blvd.
Golden, CO 80401-3393

10. SPONSOR/MONITOR'S ACRONYM(S)
NREL

11. SPONSORING/MONITORING AGENCY REPORT NUMBER
NREL/BK-150-40452

12. DISTRIBUTION AVAILABILITY STATEMENT
National Technical Information Service
U.S. Department of Commerce
5285 Port Royal Road
Springfield, VA 22161

13. SUPPLEMENTARY NOTES

14. ABSTRACT *(Maximum 200 Words)*
This report summarizes comments from the Peer Review Panel at the FY 2006 DOE Hydrogen Program Annual Merit Review, held on May 16-19, 2006, in Arlington, Virginia. The projects evaluated support the Department of Energy and President Bush's Hydrogen Initiative. The results of this merit review and peer evaluation are major inputs used by DOE to make funding decisions. Project areas include hydrogen production and delivery; hydrogen storage; fuel cells; technology validation; safety, codes and standards; education; and systems analysis.

15. SUBJECT TERMS
Hydrogen; hydrogen research; hydrogen analysis; Hydrogen Initiative; DOE hydrogen projects; hydrogen evaluations; hydrogen education

16. SECURITY CLASSIFICATION OF:			**17. LIMITATION OF ABSTRACT**	**18. NUMBER OF PAGES**	**19a. NAME OF RESPONSIBLE PERSON**
a. REPORT	**b. ABSTRACT**	**c. THIS PAGE**			
Unclassified	Unclassified	Unclassified	UL		**19b. TELEPHONE NUMBER** (*Include area code*)

Standard Form 298 (Rev. 8/98)
Prescribed by ANSI Std. Z39.18

F1147-E(12/2004)